Does Education Make Women Work? Labor Market Impacts of Free Primary Education in Africa

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Abstract

Women's economic participation is central to growth and structural transformation, yet female labor force participation (LFP) in sub-Saharan Africa – where growth is most needed – remains understudied. This paper examines whether the rollout of free primary education (FPE) across seven African countries since the 1960s affected women's human capital and later labor market outcomes. Using a country-specific difference-in-differences design comparing age-eligible and ineligible cohorts across decades, I find that FPE increased female schooling by 0.7 years, literacy by 6 percentage points, and primary completion by 10 percentage points, with additional gains in entry into secondary education. However, these schooling gains did not consistently translate into higher female LFP. While early FPE expansions in the 1960s and 1970s increased women's later labor force participation, exposure to FPE in the 1990s either reduced, or did not change, female labor market participation. These results suggest that the relationship between schooling and women's work varies across contexts, underscoring the need for more country-specific evidence on how education translates into labor market outcomes across Africa. [168 words]

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1 Introduction

Women's economic participation is central to growth and structural transformation. A growing literature shows that barriers to women's work can generate a substantial misallocation of talent in the labor market and large aggregate productivity losses (e.g., Ashraf et al. (2025); Chiplunkar and Goldberg (2025); Lee (2024)). Recognizing potential gains from removing these barriers, international organizations and governments in many low- and middle-income countries are now prioritizing policies to expand women's labor force participation and promote gender equality in labor markets. Yet, female labor force participation in sub-Saharan Africa—where growth is most urgently needed—remains understudied.

Improved access to education has historically played a central role in increasing women's labor market participation and facilitating their movement into higher-skilled jobs, as documented for white women in the United States (Goldin, 2006; Hsieh et al., 2019). While many African countries have reduced or eliminated schooling costs over recent decades, it remains unclear whether expanded access to education has translated into greater labor market engagement for women. Existing evidence from developing countries has largely focused on the effects of schooling on men's labor market outcomes (e.g., Duflo (2004); Porzio et al. (2022); Khanna (2023)), with relatively little attention to women, especially in sub-Saharan Africa. A recent exception is Duflo et al. (2025), who analyze a randomized tuition lottery in Ghana to show that reducing the cost of secondary schooling increases educational attainment and leads to modest improvements in women's employment and wages after 15 years. [1]

Theoretically, the relationship between education and women's market engagement is ambiguous. On the one hand, at early stages of development, female labor force participation may decline as rising household incomes reduce the need for women's market work and as jobs become less compatible with family responsibilities (a family-level income effect) (Goldin, 1995; Olivetti and Petrongolo, 2017; Ngai et al., 2024). On the other hand, as economies grow, "cleaner" office and white collar jobs tend to emerge as in Vidart (2023); these jobs are considered more suitable for women with some schooling and they tend to pay higher wages for this higher level of schooling, drawing women into the market (a substitution effect). The balance of these forces tends to generate a U-shape of female LFP (Goldin, 1995; Heath and Jayachandran, 2018; Mammen and Paxson, 2000).

[1] New datasets like the Jobs of the World project Bandiera et al. (2022) and the World Bank's Global Jobs Indicators Database (JOIN) Agte et al. (2024)are facilitating new work on gender gaps in developing country labor markets. Several recent papers focus on understanding the lack of women's work in India in particular (e.g. Munshi and Singh (2024); Heath et al. (2024)), where service-led growth has not translated into more female jobs. Akresh et al. (2022) return to Duflo (2001)'s Indonesia study and show that women most exposed to the school construction episodes do have higher living standards and better schooling outcomes for their children, while finding no statistically significant impacts on women's market work.

This paper examines whether the rollout of free primary education (FPE) across seven African countries affected women's human capital and later labor market outcomes. Between the 1960s and the 2010s, most African countries have implemented some form of free primary schooling, which (it turns out) has disproportionately affected girls. In the seven countries analyzed here – Ghana, Kenya, Tanzania, Zambia, Ethiopia, Uganda, and Malawi – these FPE-exposed female cohorts have now aged into the working-age population, for which multiple waves of publicly-available micro-level Census data exist.[2] I build a new dataset of free primary education policy changes and match this to multiple waves of Census data to investigate the impact of FPE policies on women's educational attainment and on their subsequent labor market outcomes. I examine how schooling affects the extensive margin of labor force participation (LFP) and the sectoral allocation of female labor across agricultural and non-agricultural jobs.

Treating each of these countries as a separate natural experiment, I apply a consistent differences-in-differences (DiD) research design to each country to causally identify the impacts of FPE on schooling and on labor market outcomes. I compare education (and labor market) gradients of younger and older female cohorts during times when the young would have been exposed to FPE, differencing out younger-older education (or labor market) gradients from times when FPE policies were not in place. Importantly, I exploit both the *removal* of early wave 1960s and 1970s FPE in some countries (Kenya, Ghana, Tanzania, and Zambia), and the later *introduction* of 1990s FPE in other countries (Ethiopia, Malawi, and Uganda) in my DiD approach.[3] [4]

Although I cannot directly test for parallel trends in all countries, I address concerns about threats to a parallel trends assumption by implementing a differences-in-regression discontinuity (DiRD) design. Using the youngest age of eligibility for primary school as the cutoff, I construct fuzzy DiRD estimates to test whether education and labor market outcomes exhibit a discrete jump in values at the earliest age at which someone was eligible for FPE, after differencing out any existing jump at the same age value from a different Census wave. The seven countries analyzed here differ in their historical institutions, their social norms about women's work, the prevalence of patriarchal institutions, their colonial masters, their political profiles in post-colonial years, their geography, their experience of the HIV/AIDS epidemic, as well as the decades in which FPE was implemented. Yet the com-

[2] In the remaining African countries, there is either no Census data yet available, or FPE has not been in place long enough to observe those affected age into the labor force captured in a Census.

[3] Specifically, I use a standard DiD design in Late Wave countries and reverse the order of FPE-treated and control waves for the Early FPE countries.

[4] Ghana had both: implementing FPE in the 1960s, ending it by the 1970s, and then reintroducing FPE in the early 2000s. I will exploit both waves in future work.

bination of the DiD and DiRD designs, consistently implemented across countries, produces results with strong internal and external validity.

The first main finding from the DiD results is that FPE significantly increased female human capital attainment in a consistent way across contexts and across time periods, and significantly more so than for males. Both early and late FPE raised primary school completion and total years of schooling for women in all countries, and improved female literacy in Ghana, Tanzania, Zambia, and Kenya. Late FPE in Malawi and Uganda did not significantly raise female literacy, which resonates with current concerns about school quality in African settings e.g. Evans and Acosta (2020). The DiRD estimates corroborate these DiD estimates: years of schooling and primary completion are higher for women in all countries (except Zambia where the difference in RD estimate of years of schooling is positive but not significant), and literacy is higher in all early wave FPE countries but not significantly different for the late wave FPE countries of Malawi and Uganda.

In a subset of countries, FPE exposure significantly raised female enrollment in high schools too, suggesting that free schooling pushed some female students further up the education distribution. Tanzania consistently exhibits the largest schooling gains from their early FPE policies.[5] Across all countries, FPE increased female schooling by 0.7 years, female primary school completion rates by over 10 percentage points, and female literacy by about 6 percentage points on average.

The second key finding is that despite these improvements in female human capital, widespread education upgrading did not uniformly translate into improved labor market outcomes for exposed women in adulthood. Greater FPE exposure significantly raised female LFP rates in Early Wave countries but had no impact or even reduced LFP rates in Late Wave countries. Effect sizes are large: additional FPE exposure raised female LFP by between 2 and 15 percentage points in Ghana, Kenya, Zambia, and Tanzania, but reduced female LFP by 1.2 and 1.6 percentage points in Uganda and Malawi (although point estimates are not statistically significant for the latter).[6] Conditional on being in the labor market, female employment increased by 1.2-1.9 percentage points among cohorts exposed to Early Wave FPE; but fell by 2.8 to 14 percentage points in Late FPE Wave countries. Working women affected by FPE policies were slightly more likely to shift away from farming and towards non-farm jobs. This is consistent with macro trends from the last 50 years showing that women have shifted out of farming in Africa and directly into the service sector (Dinkelman and Ngai, 2022).[7]

[5]Julius Nyerere, the first post-colonial leader of Tanzania, had himself been a teacher before entering politics, and pushing for universal literacy was a key part of his post-colonial reforms.

[6]Ethiopia does not have labor market data in one Census wave.

[7]Labor market impacts are different for men, and I discuss these further in the results section. I replicate

Why did schooling gains not consistently translate into higher female LFP across countries and periods? I consider two potential channels: family formation forces and the low prevalence of "clean" jobs suitable for women.

If the family formation channel is important, we should observe *lower* family formation among FPE-exposed African women who enter the labor market at higher rates. I test for this by estimating changes in marriage rates and the onset of fertility using the same DiD strategy. My results show that while family formation *is* affected by exposure to free primary schooling, the cross-country patterns do not line up with the cross-country patterns of female LFP responses to FPE.

If the availability of clean jobs is important, we should observe differential responses to FPE exposure in places with different concentrations of these clean jobs. Measuring the prevalence of clean jobs is difficult. In the five African countries for which I have detailed occupation-level data, I describe the share of men and women who work in so-called clean jobs: these are more highly skilled jobs, and public sector jobs. In each country and in almost every Census wave, the share of women working in these clean jobs is 10% or less. These results suggest that the lack of a large enough number of suitable jobs for women with basic literacy and a little bit more education may be a key factor mediating the link between schooling and labor market participation. In ongoing work, I test for whether the presence of "clean jobs" for men in a local area can help us understand the differential response of female LFP to FPE-exposure.

My paper makes three important contributions. First, I generate new estimates of how improved access to schooling increases human capital and impacts later labor market outcomes for women across a range of African countries. While many economists would agree that human capital is a key factor underpinning economic growth and structural transformation (see for example, Lucas (1988); Mankiw et al. (2004); Bils and Klenow (2000); Case (2006); Buera et al. (2022)), my results show that increases in women's work alongside increases in schooling are not guaranteed along the development path. My findings raise questions for the literature on misallocation and structural transformation: If more educated women are leaving the labor market in low-income countries, what does this imply for aggregate productivity? Would increases in female-specific labor demand keep women and their daughters more connected to the labor market (e.g. as in Vidart (2023) for the US), or is any increase in labor demand sufficient? These are important questions to address as African countries contemplate how to invest in the next generation of workers.

the Census results for schooling and for employment using DHS data in an online Appendix. My results are also robust to concerns about how the HIV epidemic may have affected different cohorts across countries: see Appendix B for details.

The second contribution of this work is to bring attention to women as workers in developing countries. Most of what we know about the connection between schooling and work in developing countries comes from studies that focus on men. Economists like to measure the earnings returns to schooling, and men are more likely than women to work in wage-earning jobs in low-income settings. Measuring impacts of schooling on other labor market outcomes – the extensive margin of female LFP, women's sector of work, and female time use – can help us better understand the barriers women may face in the labor market and enrich our understanding of how the same policies (e.g. free schooling) can have different downstream impacts on male and female workers.

Third, my results expand the literature on African labor markets, where causal estimates of returns to schooling are few, focused on a limited set of countries, and often from Mincer regressions (Duflo et al., 2025; Gethin, 2025; Ozier, 2018; Psacharopoulos and Patrinos, 2004, 2018; Schultz, 1999)).[8] In a key contribution, Goldberg (2016) estimates the daily labor supply elasticity for men and women in Malawi, finding no differences in their labor responsiveness to weekly wage variation induced in an experimental task. Her estimates suggest fairly inelastic intensive margin labor supply (0.15) for both men and women in this agricultural setting. My results show that women's LFP on the extensive margin is more responsive to FPE policies than men's LFP, and that the long-run impacts of specific policies differ significantly across countries. The evidence here underscores how much more work is needed on individual countries in sub-Saharan Africa, to understand how local context shapes the link between schooling and labor market outcomes for women.

The paper starts by outlining the literature linking human capital to women's LFP over time, highlighting key facts about the link between women's schooling and work in Africa, and describing the policies of FPE across the continent. Section 5 describes the key difference-in-differences and differences-in-regression discontinuity empirical strategies and discusses how to interpret results as well as potential threats to identification. Section 6 introduces the data and key variables. Sections 7 and 8 present results for schooling outcomes and labor market outcomes respectively. Section 9 discusses how family formation and clean jobs contribute to cross-country heterogeneity in schooling-LFP results, and Section 10 concludes.

[8]Peet et al. (2015) present OLS estimates of Mincerian returns to education from 25 countries using LSMS data from the 1980s to the 2010s. Their findings for Africa (including Ethiopia, Ghana, Ivory Coast, Malawi, Niger, Nigeria, Tanzania, South Africa, and Uganda) show high average returns (around 9%) for all schooling levels; higher returns for females and those in urban areas; falling average returns over time in some countries; and a great deal of heterogeneity in average returns across countries. As with other similar studies, their samples are limited to individuals who report wage or salary earnings; such individuals tend to be in the minority in these African labor markets. Ozier (2018) examines the returns to secondary schooling in Kenya using an RD design and a cutoff in test scores. He finds that additional schooling shifts men away from self-employment. Gethin (2025) reports on only two African studies (for Tanzania and Nigeria) that have instrumental variable estimates of returns to education.

2 Human Capital and Women's LFP over time

The U-shaped pattern of female labor force participation (LFP) that describes how women engage with, disengage, and then re-engage with the market as an economy develops through the structural transformation is by now a well-known empirical regularity. This U-shape, first documented by Durand (1975) and Goldin (1995), is strongly supported by microeconomic evidence from time use data spanning 150 years of American history (Ngai et al., 2024). Much of the evidence for other countries comes from cross-sectional comparisons of countries at many different levels of development or from pooled cross-sectional cross-country data (for example, see Mammen and Paxson (2000); Heath and Jayachandran (2018); Agte et al. (2024)), with less evidence coming from panel data within a single country over a longer time line (exceptions include Mammen and Paxson (2000) who study within-country shifts in women's work in India and in Thailand).

Several forces drive this U-shape of female LFP. As Goldin (1995) describes and Ngai et al. (2024) formalizes, income and substitution effects that change in magnitude over the process of development account for some of this pattern. When households are poor, income effects dominate substitution effects for work. As households (and nations) get richer and market wages rise, these households tend to buy more female leisure time, allowing women to withdraw from the labor market. As households and nations reach even higher levels of income, the opportunity cost of home production rises further still, and substitution effects become more important. Higher productivity growth in the market (relative to the home) raises the opportunity cost of women working at home until this cost outweighs the income effects that value time as leisure, and women start to move back into the labor market. Social norms about women working outside the home, which are prevalent in some developing countries, tend to reinforce these patterns of LFP (Heath and Jayachandran, 2018). The jury is still out (although work is ongoing, e.g. Heath et al. (2024)) on whether social norms are responsive to a changing economic structure that provides women with more opportunity for market work.[9]

Human capital is often relegated to the background of discussions about female LFP and women's work through the structural transformation. For example, Ngai et al. (2024) note that women invest in more schooling as development progresses, thereby changing their opportunity cost of staying at home and pushing them towards market. In that paper, women bring a certain level of schooling with them into the labor market, without much discussion of the investment decision. Porzio et al. (2022) note that very little of the structural transfor-

[9]Pande and Roy (2021) discuss how social norms against women's market work were evident in the historical US and UK.

mation literature takes into account the important role of education in affecting which sector workers choose to work in. Their paper uses a general equilibrium (GE) framework to show how more schooling changes the nature of the workforce by increasing the supply of skilled labor. Their empirical evidence covering many countries, both rich and poor, makes two important points. First, each additional year of schooling decreases cohort-level agricultural employment by about 13.5%. Second, schooling enabled up to 40% of the shift of workers out of farming and into non-farm sector jobs. Their results show that schooling is an essential part of the structural transformation process. However, their paper focuses entirely on men and cannot tell us what to expect for the schooling-market work relationship for women.[10]

Vidart (2023) takes this idea further in the US context, showing how investments in human capital among women differ across *generations* and as women respond to changing relative demand for skilled female labor. Her paper uses the technological revolution of electrification in the workplace in the US in the 1930s as a demand shock for skilled (i.e. educated) labor. She models and then shows empirically how women who were young at the time of this demand shock were able to take advantage of new work opportunities because they still had time to invest in education (up to a critical 8 years of education). In contrast, older women already in the labor market at the time of electrification could not upgrade their human capital. Schooling is therefore the direct channel through which women of the next generation were able to switch into the more skilled manufacturing sector.

3 Women's Schooling and Female LFP in Africa

Across Africa, the relationship between female LFP and GDP per capita is negative: poorer countries have high rates of female LFP while richer countries have lower rates of female LFP (Dinkelman and Ngai, 2022). However, this cross-sectional relationship does not directly account for human capital accumulation.

To begin to answer the research question of whether education makes African women work or not, I examine the cross-sectional relationship between level of education and LFP for the women in my analysis sample of countries. Figure 1 plots the share of women in the labor force by their years of schooling for six of the seven countries in my analysis, using the most recent Census data from each country.[11] Labor force participation is first residualized

[10]The authors explain their choice to exclude women from the analysis by stating that they cannot properly compute cohort-level reallocation across sectors because women have such low labor force participation in many settings. While this is true in some low-income countries, it is not the case for most of sub-Saharan Africa, where female LFP is relatively high e.g. see Dinkelman and Ngai (2022).

[11]Ethiopia does not have Census data for LFP in both Census waves, but is still useful for the schooling analysis.

using age fixed effects to control for life-cycle patterns in female LFP. I add back the mean share of women in the labor force to plot the graph of (age-residualized) shares in the labor force against years of education.

There are two striking features of this figure. First, there is wide variation in LFP across countries with women generally working at higher rates than in the US (female LFP in the US is currently at 56%). Almost 60% of Zambian women without any education work while over 80% of Ugandan women without any schooling work. Second, the share of women doing market work *declines* as schooling rises. The share of women in the labor market is relatively stable until around 6 or 7 years of schooling which is the end of primary school. As women attain additional years of high school, their LFP falls. But as years of education increase further into the post-secondary education level, the share of women in the labor force begins to rise again (except in Malawi). The rest of this paper focuses on unpacking the causal evidence for this relationship between more schooling and changing female LFP.

4 Free Primary Education (FPE) in Africa

When a country implements FPE policies, this generally implies that parents or guardians can send their children to primary school for free. Ideally, this means zero direct and indirect costs of education (e.g. tuition fees, school uniforms, book fees, materials, transport). In practice though, FPE tends to be restricted to mean "no tuition", with some indirect costs remaining.[12] I do not have data on actual school costs and will only be able to measure responses to the arrival or (or removal of) announced FPE policies at the national level.

4.1 FPE in Historical Context

Many African countries have had at least two waves of Free Primary Education (FPE) policies. Broad access to free primary school was initiated by many African countries in the 1960s, following independence. These early FPE policies triggered surges in enrollment, expanding access to schooling as new countries started building more schools. However, initial enrollment gains began to erode in the 1970s and 1980s, and FPE policies came under increasing pressure as economic stagnation on the continent, debt crises, externally-imposed structural adjustment policies, and a growing youth population stretched public resources beyond what was feasible. National education budgets were slashed (The World Bank, 2009), governments begin to reintroduce user fees for public services, including public schooling.

[12]Evans et al. (2024).Klapper and Panchamia (2023) reports that over half of adults in SSA worry about how they will pay school fees. In Uganda, parents report that it costs on average USD168 (20% of annual income) to send a child to a public school.

and by the mid-1990s, the impact of the HIV/AIDS pandemic was making it even more difficult (and less worthwhile) for young people to access primary schooling.[13]

In 1990, the World Declaration on Education for All committed nations to the goal of meeting basic learning needs of all and emphasized education as a key human right. Pritchett (2025) notes that the Washington Consensus considered public spending on education (especially primary schooling) to be good. By the mid- to late 1990s, multilateral donors, NGOs, and specific developed countries renewed the push for universal primary education, and "Education for All" became a top priority in the first Millenium Development Goals of 2000. Soon after, resources from the World Bank's Education for All-Fast Track Initiative (in 2002) and the World Bank and UNICEF's School Fee Abolition Initiative (2005) (The World Bank, 2009) facilitated the initiation or re-initiation of free primary education (FPE) in many countries. This second late wave of FPE policies was supposed to push all children through to the end of primary school. Universal primary school completion by 2030 is now a Sustainable Development Goal.[14]

A critical implication of these policies that fluctuated over time is that in 5 of the 7 countries I consider here, all except Uganda and Malawi, FPE in the 1990s and 2000s was a *second* attempt to improve access to schooling and to promote human capital attainment. These different waves of FPE generate two sets of cohorts, separated by about a generation, that faced lower costs of accessing school. Cohorts in between faced the highest costs of education. In section 5, I explain how I use this introduction of FPE in both early and late periods to construct appropriate treatment and control groups in each country.

4.2 Effects of FPE on Education and Women's Work: Results from the literature

An existing body of work addresses questions around how FPE affects particular outcomes in specific countries. Development economists have tended to use difference-in-differences (DiD) designs or regression discontinuity (RD) designs to do this work, often implemented on Demographic Health Survey (DHS) data. DiD approaches often exploit spatial variation in pre-policy primary school completion rates (or historical enrollment, historical literacy

[13] Africa as a whole (excluding oil exporters) reduced the share of GDP spent on education by almost 25 percent between 1980 and 1983 (Tilak, 1990). Fortson (2011) estimates the impact of the HIV/AIDS pandemic on education in Africa, finding that post-1980s birth cohorts in areas with 10% higher levels of measured HIV had about 0.5 years less schooling relative to pre-1980s birth cohorts. Appendix D more formally discusses the implications of the HIV epidemic for the key results in my paper.

[14] As a point of comparison, universal primary schooling was in place in the US by the turn of the 20th century. Recent data indicates that still only 70% of adults in sub-Saharan Africa (excluding high-income countries), have completed primary school.

rates, or infrastructure backlogs) interacted with time variation in FPE law implementation to generate exogenous variation in exposure to FPE laws. Under such strategies, poor schooling outcomes in a pre-policy period have high implied exposure to national FPE laws. One challenge with this approach is that some regions may have historically higher or lower education levels because of earlier waves of FPE (in the 1960s and 1970s) that themselves had lasting effects on particular cohorts.

Alternatively, fuzzy regression discontinuity (RD) designs identify access to FPE for individuals who were just eligible relative to those who were just ineligible for free schooling. These designs run into trouble when samples are small, when there is a lot of overage enrollment or late entry to school (worsening the fuzziness) and hence a weak initial stage. Even when sharp RDs are possible, converting these local average treatment effects into average treatment effects requires extrapolating beyond the specific age cutoff.

Both designs have been used to examine how FPE affects proximate schooling outcomes (mostly for women) and fertility. RD designs typically find larger impact estimates of FPE laws on final schooling attainment for marginal individuals, relative to DiD papers that identify effects for an average individual. Point estimates for the impact of FPE on schooling attainment range from 0.1 to 1.5 years of additional education. Evans and Acosta (2020) provide an excellent summary of the existing work related to FPE impacts; most papers in this literature focus on single countries.[15] In some countries, additional female schooling induced by these FPE policies has been shown to reduce early fertility and child mortality.[16]

Despite the fact that African countries have had FPE in place since at least the 1960s, there are many outstanding questions about how the human capital accumulation induced by

[15]Estimates for females in using DiD approaches range from: 0.1 years (Andriano and Monden, 2019) to 0.15 years (Chicoine, 2021) in Malawi; to 0.2 years for males and females (Delesalle, 2021) in Tanzania (although Hoogevan and Rossi (2013) show that FPE crowded out quality education for poor rural families); to 0.13 years in Ethiopia Chicoine (2021); to 1.54 years in Nigeria (Osili and Long, 2008), and about 0.2 of one level for Nigerian men and women (Larreguy and Marshall, 2017). Papers using RD designs have found impacts of FPE for females of: 0.9 to 1 year in Ghana, after the 1990s FPE (Boahen and Yamauchi, 2018); and 0.72 years in Uganda (Keats, 2018). Several papers have examined how FPE within Kenya raised access, changed selection into primary school exam completion, and did not have negative impacts on test scores (e.g. see Lucas and Mbiti (2012) and Bold et al. (2013). Ozier (2018) examines the impact of free *secondary* schooling in Kenya using an RD design leveraging test-score cutoffs to generate eligibility for secondary school. He finds impacts on total learning outcomes for both genders.

[16]Andriano and Monden (2019) find that a one-year increase in schooling significantly reduces under-five child mortality in Malawi by 10% and in Uganda by 16%. Chicoine (2021) shows that one more year of schooling in Ethiopia reduced fertility by more than 0.4 births. Boahen and Yamauchi (2018) find that for each additional year of schooling in Ghana, fertility falls by 0.11 births or 65%; Keats (2018) shows that an additional year of schooling reduces the likelihood of having a first birth before age 20 by about 11% on average, but does not find any reductions in under-five mortality; (Osili and Long, 2008) find that increasing female education by one year reduces early fertility by 0.26 births in Nigeria; Ozier (2018) finds that increasing learning among females in high school almost eliminates the probability of early fertility (pregnancy before age 18) in Kenya.

these policies might have affected women's market work. Far fewer papers look at how FPE influences labor market outcomes later in life. Delesalle (2021) uses a DiD design to show that FPE-induced schooling raised the share of Tanzanian women working in agriculture - which contrasts with what the structural transformation literature suggests might have happened. Ozier (2018) uses an RD design to show that Kenyan secondary schooling reduces low-skill self-employment for men (with no significant results for women, see Table A.3), despite the fact that it increased human capital for both.

5 Empirical strategies

5.1 Seven Natural Experiments and Country-Specific DiDs

To identify the impacts of FPE on schooling and on later-life labor market outcomes, I implement country-specific difference-in-differences regressions that exploit time variation in exposure to early (pre-1990s) or late (1990s and beyond) FPE policies in each country interacted with cohort exposure variables that define eligibility for FPE. Intuitively, this approach compares outcomes across FPE age-eligible and age-ineligible individuals in a Census wave when FPE was in place, differencing out the same age gap in outcomes among individuals from a different Census wave when FPE was not in place. The Census waves are chosen so there is no overlap in birth cohorts.

The general empirical model for woman i of age a observed in Census wave t is therefore:

$$Y_{iat} = \beta_0 + \beta_1 TreatedGroup_a * TreatedWave_t + \beta_2 TreatedWave_t + \lambda_a + \eta_{ait} \qquad (1)$$

where Y_{iat} denotes the relevant human capital or labor market outcome, λ_a is a set of age fixed effects and η_{iat} is an idiosyncratic error term. Heteroskedasticity-robust standard errors are clustered on age and all regressions are population-weighted.[17]

$TreatedGroup_a$ captures whether or not an adult of a given age would have been eligible for FPE during childhood, if FPE had been in place. I use two variations of $TreatedGroup_a$:

[17] I use 10% samples of Census data taken from IPUMSI. Most observations have the same weight of 10. Clustering on age (regardless of which Census an individual is observed in) accounts for unobserved factors that affect education or labor market choices common to individuals of a specific age (e.g. if you need a driver's license to get a job, but can only obtain such license at age 18). Abadie et al. (2022) argue that clustering in this way may be too conservative in cases where each cluster has a large number of observations and where all individuals in the cluster are sampled. This is the case for the Census data used here. However, there is no improvement from using their proposed alternative estimator with less conservative variance, because treatment assignment is also perfectly clustered.

an indicator variable $Eligible_a$ and a fractional exposure variable, $Intensity_a$. $Eligible_a$ is a strict measure of eligibility, indicating whether a woman of age a (regardless of Census year) would have been age-eligible for FPE if FPE had been in place during her childhood. For example, suppose that FPE is implemented in a given year for everyone age 6 and younger. In a Census wave k decades after this FPE year, those aged $6 + k$ years and younger would have a value of $Eligible_a = 1$ while those $6 + k + 1$ and older would be $Eligible_a = 0$. This definition of eligibility is implemented for the same age groups in the control Census wave. I include $Eligible_a$ as a control in Equation (1), although this term is collinear with the age fixed effects.

One drawback of defining $Eligible_a$ in this way is that it presumes that FPE policies are strictly adhered to and that no-one already attending school is affected by FPE policies. This is not realistic in many of the settings considered here: age policies for school are not strictly enforced, many children enter school late, and high rates of grade repetition leave many children below their grade-for-age. For children already in primary school at the time of a new FPE, most would remain eligible for at least some free schooling, depending on their age at the time of FPE implementation.

To capture both types of "leakages" from treatment, I construct a second, broader definition of exposure to the FPE laws during childhood:

$$Intensity_a = \frac{\min\left(LowAge, |HighAge - (a - k)|\right)}{(HighAge - LowAge)} \tag{2}$$

$Intensity_a$ measures the share of primary school years for which an individual would have been age-eligible for FPE. For example, if FPE applied to all kids from ages 6 ($LowAge$) to 12 ($HighAge$) inclusive in a given FPE year, then k =twenty years after the start of FPE, $Intensity_a$ for adults ages 31 and younger is defined as:

$$Intensity_{31,20} = \frac{\min\left(6, |12 - (31 - 20)|\right)}{(12 - 6)} = \frac{1}{6} \tag{3}$$

while those aged 32 would have:

$$Intensity_{32,20} = \frac{\min\left(6, |12 - (32 - 20)|\right)}{(12 - 6)} = 0 \tag{4}$$

and anyone older than 32 would have $Intensity_a = 0$.

$Eligible_a$ and $Intensity_a$ differ in how the partially treated cohorts are categorized. Partially treated cohorts are part of the control group under the strict eligibility measure. Under $Intensity_a$, they are partially exposed to FPE in proportion to their exposure time. For example, consider someone aged 8 at the time of FPE introduction and then observed $k = 20$

years later:

$$Intensity_{28,20} = \frac{\min(6, |12 - (28 - 20)|)}{(12 - 6)} = \frac{2}{3} \tag{5}$$

This individual would have a value of $Eligible_a = 0$ but $Intensity_a = \frac{2}{3}$. Appendix Tables A1 and A2 define eligible cohorts by country, list early and late FPE countries, show which Census wave is a $TreatedWave_t$ and which not, and demonstrate how Treatment and Control groups are defined.[18].

$TreatedWave_t$ indicates whether a Census year is "treated", meaning that FPE laws were in place during childhood for at least some of the adults in that Census. In the late FPE countries – Ethiopia, Malawi, and Uganda – where FPE was implemented in the 1990s, $TreatedWave_t$ takes a value of 1 in the most recent Census and 0 in the earliest Census wave. For the early FPE countries – Ghana, Tanzania, Zambia, and Kenya – where FPE was implemented in the 1960s, 1970s, and 1980s, and turned off at some point, the $TreatedWave_t$ values are reversed.[19] $TreatedWave_t$ takes a value of 1 in the earliest Census and 0 in the more recent Census wave, when the FPE policy would no longer have been in place for adults appearing in that Census.[20] Intuitively, for Early FPE countries, the DiD design compares younger (eligible) to older (ineligible) cohorts in the early FPE period to younger (comparable eligible) versus older (comparable ineligible) cohorts in a later period after the early FPE policy had lapsed.

While the focus of this paper is on female schooling and work, for some outcomes I also test whether FPE has a larger impact on women relative to men using a triple difference design. I modify Equation (1) to include a female control variable, and interactions of female with each of $TreatedWave_t$, $TreatedGroup_a$, and $TreatedWave_t * TreatedGroup_a$. The parameter of interest in this specification is on the triple interaction term, $TreatedWave_t * TreatedGroup_a * Female_i$.

5.2 Identification of schooling and labor market impacts of FPE exposure

The parameter β_1 in Equation (1) captures the causal effect of removing tuition fees for primary schooling on the difference in average educational attainment between younger and older women over time. Since education levels may trend upwards, or in some cases down-

[18]More detailed definitions of treatment for each country are provided in Appendix B

[19]FPE was in place in Ghana for 5 years, in Tanzania for 10 years, in Kenya for 14 years and Zamibia for 15 years.

[20]Dynarski (2003) is an example of such an "on-off" differences-in-differences research design.

wards, over time, any simple difference could estimate a significant young-old *cohort* education gap. The DiD framework allows me to estimate the *differential* change in education across young and old cohorts that emerges after FPE comes into place, or that changes when FPE goes away. In other words, this specification isolates how FPE changes the *trajectory* of human capital attainment, relative to general increases or decreases that could otherwise have occurred.

Labor market impacts are also identified off of this variation in age-exposure to FPE policies across treated versus not treated Census waves. Because there could be life-cycle shifts in occupation and sector of work that are reflected in different occupational choices by age, the specification in Equation (1) nets out any stable life-cycle patterns in LFP or employment outcomes, by comparing younger versus older cohorts before and after FPE policies are introduced.

There are several nuances to these interpretations of β_1. First, β_1 is an intent-to-treat parameter. It measures how families and individuals react to the reduction in school fees when FPE comes along. Enrollment is unlikely to be universal in response to the FPE policies, and may differ across countries. Because I do not measure enrollment, but rather exposure to the policy, β_1 is likely a lower bound estimate of the FPE policy impact on schooling. Second, for the Early FPE countries, β_1 estimates the amount of schooling that was *lost* due to schooling costs being reintroduced. In contrast, for late FPE countries, β_1 gives us the differential gain in young people's cohort-level education that was facilitated by a permanent implementation of FPE. Comparing results from early and late FPE countries will show us how policies with limited scope may have different impacts, relative to more permanent policies.

5.3 Potential threats to identification

Staggered treatment timing is highlighted as a potential threat to internal validity in the DiD literature (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021). This concern is not relevant here, since FPE was implemented in each country at the level of the country. There is no staggered implementation across ages, or across regions within a country.

The key identification assumption required here is a parallel trends assumption: the cohort-profile of educational attainment (or the life-cycle of labor market changes) should be stable over time in the absence of FPE policies. To test for parallel trends in my setting, I would need the same life-cycle profiles of educational attainment (or labor market participation) 40 and 65 years before the most recent Census wave that is available for each country. Unfortunately, most African countries only began Census-taking in the 1970s.

Despite this limitation, it is worth noting that the research design itself minimizes the chances that non-parallel trends drive results. Equation (1) is implemented separately in each country, for policies with different timings, using both "off-on" and "on-off" treatment designs. It is highly unlikely that a common violation of the parallel trends assumption could invalidate impact estimates in all countries in the same way.

5.4 A Regression Discontinuity Approach

One way to get around concerns about non-parallel trends is to instead look at the effects of FPE using an RD approach. To isolate the direct effect of FPE on individuals at the margin of full eligibility for FPE, I implement a parametric specification of a differences-in-regression discontinuities (DiRD) estimator. Intuitively, this approach isolates the effect of FPE for the marginal female who is just age-eligible (relative to just age-ineligible) for FPE. Since there could be time-specific shocks to education (or labor market outcomes) tied to the age of her maturity, I difference out the same education (or labor market) effect at the same age discontinuity from an untreated Census wave. The DiRD is the result.

I specify the following OLS model where Age_RD_i is the running variable, a person's age relative to the earliest age at which the person can enter school (negative values of this variable are younger people), D_{it} is a dummy variable indicating whether the person is older than the earliest year the student could get FPE or not, and $Post_t$ is a dummy variable capturing the year of the Census:

$$Y_{it} = \delta_0 + \delta_1 D_{it} + \delta_2 AgeRD_i + \delta_3 D_{it} * AgeRD_i + \delta_4 Post_t * D_{it} + \delta_5 Post_t * AgeRD_i + \delta_6 Post_t * D_{it} * AgeRD_i + \epsilon_{it} \tag{6}$$

where:

$$AgeRD_i = Age_i - c \tag{7}$$

and

$$D_{it} = 1 \quad if \quad AgeRD_i > c; \quad else = 0 \tag{8}$$

Conditional on the running variable $AgeRD_i$, the parameter on D_{it}, δ_1 is the RD parameter and δ_4 is the DiRD (difference in regression discontinuity) parameter. It tells us how much more or less the outcome responded to crossing the cutoff age in the Treated Census wave ($Post_t = 1$), compared to the Control Census wave ($Post_t = 0$). I estimate this model for each country, looking at years of schooling and the probability of being in the labor force.

I interpret δ_4 as an estimate of the effect of FPE on the decision of the marginal woman (and their families) to get more schooling and to participate in the labor market. If these

DiRD estimates are in line with the DiD estimates, this is strong evidence against a violation of parallel trends confounding my DiD results.

5.5 External validity

Macroeconomic shocks could threaten the external validity of any one DiD estimate or RD estimate. Rosenzweig and Udry (2020) make this point when they note that macro shocks are problematic even in the case of carefully implemented randomized controlled trials with strong internal validity.

If I was to focus on FPE in a single country in a single period, I would need to pay close attention to whether the country in question was experiencing a boom or bust, whether prices for agricultural products were high or low, or whether the value of child labor was fluctuating during the time that the FPE policies came into place, or when they were reversed. Instead, by taking the same research design to multiple countries in different decades and with different timings, it is far less likely that stochastic, aggregate shocks could drive effects for all countries in the same way. If the research design produces similar impact estimates, especially for education outcomes, I can be more confident in the external validity of these results.

6 Data and key variables

6.1 Dates of FPE

Despite existing academic and policy literature on the effects of FPE in Africa, finding accurate and documented information on dates on which FPE was introduced in each country is not straightforward. Together with a team of research associates, I built a database of FPE dates and eligibility information for each country in Africa (54 countries plus the disputed Western Sahara) described in Dinkelman et al. (2026). I use the FPE dates for the seven countries of interest to define treated and control cohorts.

Of the seven countries included in my analysis, Malawi and Uganda had no FPE policies before introducing them in the mid/late-1990s. Ethiopia had an earlier wave, but publicly-available Census microdata only exist for analyses that cover the later waves of FPE. The other four countries (Kenya, Ghana, Tanzania, and Zambia), all had an early wave of FPE either in the 1960s or 1970s which then ended by the late 1980s or earlier. The second and later wave of FPE in these countries started in the early 2000s. In between these early and late FPE periods, there was no free primary schooling for about two to three decades – enough to stymie the education of at least one generation of African workers. I can only

measure late FPE impacts in these countries when there is a more recent round of Census data from the 2020s or beyond, which is the case for Ghana. In all countries, primary schooling is compulsory, and the age eligibility for free primary schooling is similar: starting at 6 or 7 years and ending at 11 to 13 years of age.

The reasons for the end of the early wave of FPE in each of the early countries vary, and are documented in Dinkelman et al. (2026). To summarize: the reasons appeared political in Ghana and Tanzania, and economic in Kenya and Zambia. A coup in Ghana in 1966 ended their first round of FPE; in Tanzania, FPE was abolished in 1985 when the president resigned. In Kenya, the government re-introduced fees in 1988 as part of attempts to bring national budgets under control; and Zambia ended FPE around 1980 as the government faced massive economic pressure from global commodity price shocks (especially copper).

6.2 Census Data

The main analysis uses two waves of Census data (either 10 or 20% samples) for each country, spanning decades from the 1980s through the 2010s. Within each country, I choose waves that are 20 to 30 years apart (see Appendix Table A1 for details) to avoid having the same birth cohorts in pre- and post-waves. All Census data are publicly available on IPUMSI (Ruggles et al., 2025).[21]

I use variables capturing individual-level demographics (age, gender, marital status, spousal demographics), human capital outcomes (years of schooling attained, highest level of school completed, literacy where available), and labor market outcomes (labor force participation, employment status, type of worker, and occupation and industry codes of work where available). Variables are standardized by IPUMSI or in my data cleaning process.[22]

The target analysis sample is women (and men) adults aged 20 to 48, with some adjustment of this age range to avoid including young adults who might still be in school, and to avoid overlapping cohorts across Census waves.[23] The relevant age samples for each country are: for Ghana in the early FPE Period, ages 29-48 (treated ages are 29-30); for Tanzania ages 21-40 (treated ages are 21-22); for Kenya ages 19-36 (treated ages are 19 and 21); for Zambia ages 23-36 (treated ages are 23 to 31); for Ethiopia ages 18-48 (treated ages are 19); for Malawi ages 20-48 (treated ages are 20-30); for Uganda ages 20-48 (treated ages are 20-23).

[21] IPUMSI website is: *https : //international.ipums.org/international/*

[22] I also use DHS data and a similar empirical strategy to verify some of my key results. See Online Appendix C for more details.

[23] In several of our countries, up to one in five of those aged 18 and 19 are still enrolled in some type of schooling. Duflo et al. (2025) shows that young adults spend a long time getting through higher education, among those who qualify.

Human capital outcomes are measured in four ways to capture both quantity and quality of schooling: completed primary schooling (0/1), years of complete schooling, and literacy (0/1). I also look at whether individuals enrolled in any secondary school (0/1). Except for literacy, all variables are available in all Census waves in all countries (see Data Appendix B for more details on variable construction).

Labor market outcomes focus on the extensive margin of labor force participation (0/1), sector of work (agriculture, manufacturing, or services), and type of work (wage work, unpaid work, self-employed). I standardize key labor market variables from each Census wave. Each variable is a dummy variable, and in general, the reference period for the employment questions is in the week (last seven days) before the Census period. [24] Labor force participation (LFP) captures the extensive margin of labor force activity and includes active workers as well as those not currently working but looking for a job.[25] Using international standard occupation (ISCO) codes, I look at whether someone reports working in agriculture, or non-agriculture.

In supplementary analysis, I also look at marital status (ever married (0/1)) and fertility (any children born (0/1)) as outcomes. Ashraf et al. (2020) provide evidence from a large school construction project in Zambia that families may choose to invest in education to increase a woman's return on the marriage market, at least in communities that practice traditional bride price. I will be able test whether FPE exposure changes marriage rates or fertility patterns in the aggregate for each of our countries.

6.3 Summary Statistics

Table 1 uses the Census data to show how educational attainment has changed for women in each country (results for men are in the Appendix). Average schooling among adult women in the 1980s ranged from a low of 0.5 years in Ethiopia to 2-2.6 years in Malawi and Ghana. Over the following twenty to thirty years, these average levels of schooling rose to almost 2 years in Ethiopia and to over five years in Malawi and Ghana. Large gains in years of schooling also boosted primary school completion, which is generally 6 to 7 years of education. Completion rates ranged from under 10% in Ethiopia and Malawi in the 1980s to over 70% in Kenya and Tanzania in later years. Literacy gains are on the order of 20 to 30 percentage points in Malawi, Uganda, and Tanzania. Despite these large education gains from low initial levels, human capital is still dismally low as others have noted (Evans and Acosta, 2020), and questions about quality remain Pritchett (2025). Not one of these

[24] In ongoing work, I compare Census measures of labor market outcomes with LSMS measures. Some of my results also reflect analyses of female labor market outcomes measured in the DHS.

[25] Those not in the labor force can be still in school, doing housework, retired, or otherwise unable to work.

countries has approached the level of universal secondary school attainment among working-age women.[26]

Means of labor market variables for women are also summarized in Table 1. Most countries have relatively high female LFP, with the female share in the labor market being around 73%. There is also very little gap between female LFP and employment rates, implying a low unemployment rate. Within a country over time, female LFP has fluctuated in different ways: falling in Ghana, Tanzania, and Malawi, rising in Zambia and Uganda, and remaining flat in Kenya. Some of these differences may reflect differences in the nature of labor markets across countries. They underscore the need for analyses that permit heterogeneity across countries.

Paid work typically captures formal sector work in SSA. Looking at the type of work variables, it is clear that wage work constitutes a very small share of women's work (except in Zambia).[27] Women are much more likely to report being self-employed, and in some countries, unpaid workers. As Table 1 shows, female jobs are also prevalent in the agricultural sector although there is much variation across countries in the share of women in farming. In Malawi in the 1980s this share is 95%, while in Ghana in 2010, only one-third of women are in farming. The share of women in manufacturing is low and the share in medium- and low-skilled service sector jobs is higher. These sectoral patterns reflect general trends across time in SSA (e.g. see Figure 1 in Dinkelman and Ngai (2022)).

7 Effects of FPE Exposure on Female Human Capital

7.1 FPE raises female human capital

Tables 2 to 4 present results from the differences-in-differences analyses of human capital accumulation for women in each country. Table 2 links exposure to Early FPE policies of the 1960s and 1970s to educational attainment outcomes and Table 3 present results for how the Late FPE policies of the 1990s affected educational attainment in more recent times. In each table, Panel A presents results using the $Eligible_{at}$ treatment variable and Panel B using the $Intensity_{at}$ treatment variable. All regressions control for Census wave indicators, age-at-interview fixed effects, the treatment variable (which is defined for all waves) and the interaction of treatment with treated wave. Regressions are population-weighted and robust

[26] As a useful point of comparison: In 2023-2024, 67% of working-age Indian women and 80% of working-age Indian men had completed primary school; women have on average 8 years of education and men have on average 9 years (survey weighted means, own calculations, India NSS (2023-2024).

[27] Similar summary statistics for men show that wage work is a much larger share of work for males, about one-third of all male jobs.

standard errors are clustered on age.

Table 2 clearly shows significant increases in total years of education and in primary school completion for women in all early FPE countries. In Panel A, effects range from 0.2 years of additional schooling (in Zambia) to almost 1.2 years in Tanzania and Ghana. Difference-in-differences results for the *Intensity* treatment in Panel B are larger than for the *Eligible* treatment: the coefficients on the *Intensity* measure range from 0.5 years in Zambia to almost 2 additional years of schooling in Tanzania. Differences between the estimates in Panel A and B reflect the fact that cohorts already in school at the time of FPE policies were themselves positively impacted by the elimination of tuition. Some of those individuals in the control group under the *Eligible* treatment definition could have stayed longer in school, contributing to a wider schooling attainment gap between those eligible for any FPE (under the *Intensity* measure) and those completely ineligible.

FPE policies also worked to keep more girls in school long enough for many more of them to complete primary schooling. Table 2 shows that in Zambia, primary school completion rose by 5.5 percentage points, with more than double that effect size in Ghana and Kenya – female primary school completion rose by more than 10 percentage points. Of these Early FPE countries, Tanzania saw most success, with an increase in female primary school completion of over 31 percentage points. Among strictly eligible cohorts, FPE in Tanzania quickly led to almost universal primary school enrollment. Effects on primary school completion are larger using the *Intensity* measure, although differentially so across countries. Primary school completion rose by about 13 percentage points in Ghana and by a much larger 14 to 43 percentage points in Zambia and Kenya. One reason for these differences is that FPE in Ghana and Tanzania lasted only 4 years, while FPE lasted 14-15 years in Kenya and Zambia.

Note that in Table 2, all the main effects of being in the treated wave are negative. This is because in the Early FPE countries, the treated wave is the earlier Census data, and education levels are generally increasing over time.

Table 3 shows that late FPE policies implemented in Ethiopia, Malawi, and Uganda had positive but somewhat smaller impacts on female educational attainment.[28] The late FPE policies increased years of schooling between 0.3 years (for Ethiopia) and almost 0.9 years in Malawi in Panel A of the table. Panel B estimates show larger impacts for the *Intensity* measure than for the *Eligible* measure, although these magnitudes shrink when scaling by mean intensity. For example, in Ethiopia, at mean intensity, the impact on years of schooling is about 0.17 more years of education (0.349*0.509), and between 0.29 and 0.34 more years

[28] Although Ethiopia had implemented some free access to schooling in earlier years (1976-1990, see Dinkelman et al. (2026) for details), there are no earlier Census data available to explore the impacts of these Early FPE policies. Uganda and Malawi had no early waves of FPE.

of education in Malawi and Uganda.[29]

Primary school completion rates before the introduction of FPE were significantly lower in Malawi, Ethiopia, and Uganda (under 30%, relative to 50 to 60%) than in early FPE countries, and primary school completion impacts are smaller than those for early FPE adopters in Table 2. Primary school completion rose by between 4 percentage points (in Ethiopia) and 10.5 percentage points (in Malawi) for girls exposed to the late FPE policies in these countries. In none of these countries has Late FPE raised primary school completion to the near 100% that Tanzania achieved under their early FPE.

Note that in contrast with Table 2, the main effects of being treated in Table 3 are all positive: this is because the treated waves are all later than the non-treated waves, and overall education is increasing in all countries over time.

Figure 2 collects together the DiD estimates for both years of schooling and primary school completion outcomes across all 7 countries, in the specification using the stricter *Eligible* measure of treatment. Each dot (whisker) represents an estimate for a different country, and the estimates are plotted alongside a dotted vertical line at zero. The mean of the outcome for each country is given in square brackets on the Y axis of each figure. On average, years of schooling increase by about 0.7 years and primary school completion rates increase by just over 10 percentage points.[30]

Table 4 shows how FPE policies translated into improvements in learning and positive pipeline effects into high school. Panel A in Table 4 indicates that early FPE policies significantly raised female literacy in places where FPE induced larger gains in education. In Tanzania, the country experiencing the largest increase in years of female schooling, literacy rates for women rose by 20 percentage points. In Zambia, literacy rose by 4.3 percentage points. Among the late FPE countries, only Ethiopia exhibits a significant almost 7 percentage point gain in literacy rates, from a base of about 20 percent literacy. Malawi and Uganda see no significant improvements in literacy among women. This lack of literacy gain in late FPE countries is consistent with concerns about low quality education (Evans and Acosta, 2020). Both Malawi and Uganda implemented FPE as a "Big Bang", with little

[29]The fact that the intensity measures yield smaller estimates at mean intensity levels relative to the strict eligibility measure for late FPE countries is likely because the contribution of the partially exposed cohorts to the treatment group is relatively small. This is in contrast to the early FPE countries, where the partially treated groups were a larger fraction of all treated individuals.

[30]The raw means of these outcomes across countries are 0.72 years and an 11 percentage point increase in primary school completion. In an auxiliary meta-analysis, I combine these cross-country results by estimating a random effects model. This analysis estimates that the average impact on years of schooling is 0.7 years (95% confidence interval is 0.37 to 1.01 years) and the average impact on primary school completion is 10 percentage points (95% confidence interval is 6 to 14 percentage points). I can reject the null of treatment effect homogeneity using this meta-analysis, which implies that there is significant cross-country heterogeneity in the treatment effects of FPE not driven by differences in the precision of estimates across countries.

preparation; and overcrowding in classrooms may have degraded actual learning in primary schools. However, the early FPE results show that it is possible for FPE to improve literacy. Moreover, these results show that *rolling back* FPE led to declines in literacy rates for women in later cohorts.[31]

Panel B of Table 4 shows that greater FPE exposure also increased high school entry in some countries. Although FPE policies did not generally extend to free secondary school during the analysis period, they may have impacted high school enrollment through several channels. Saving money on primary school tuition could facilitate families spending money on high school tuition. Or, just having FPE means more primary school graduates, and the best of those students may then have the option to continue to high school (whereas before FPE, that option was not feasible). On the other hand, if FPE policies encouraged more academically marginal children to complete primary school, there might be no impact on transition into secondary schools. And, at a systemic level, access to high school may actually fall if expansion of free primary schooling is funded at the expense of funding for high schools.

Table 4 shows that in Ghana, Kenya, Malawi, and Uganda, FPE-exposed cohorts show significant and large increases in entry into secondary school of between 9 and 12 percentage points. In Tanzania and Ethiopia, the effects of FPE exposure for females on high school entry are statistically *negative*. This may be because in these countries, governments funded primary school expansion by actively directing resources away from secondary education (e.g. see Delesalle (2021) for Tanzania.) In Zambia, there is no significant increase in entry into high school after FPE exposure.

7.2 FPE reduces gender gaps in education

In Table 5 (and in tables in the online appendix, A4, A6, and A5), I use a triple difference specification to show that FPE raised human capital substantially and significantly more for women compared to men. The coefficient on the *Female* indicator in Table 5 shows a 0.4 (in Kenya) to 2.5 year (in Malawi) gender gap (male minus female attainment) in years of schooling. Females are 1.2 to 24 percentage points less likely to complete primary school. Literacy gaps are also very large, up to 38 percentage points in Malawi. These are the types of gender gaps that have motivated the many randomized controlled trials that test different interventions to get and keep girls in school (e.g. Baird et al. (2011), Friedman et al. (2016) and Muralidharan and Prakash (2017) are just three well-known examples).

Against this backdrop of existing gender gaps, gender-neutral FPE policies had very large

[31] In contrast to the late FPE countries using Census data, the results using DHS data indicate improvements in literacy in most countries. See Online Appendix C

impacts. The gender gap in years of schooling (estimated by the coefficient on the triple difference term) fell by 0.76 of one year of schooling in Malawi, by half a year in Kenya, Zambia, and Ethiopia, and by 1.2 years of schooling in Tanzania. Literacy gaps fell in all countries except Uganda. Progression through primary school speeds up for girls and entry rates into high school are significantly higher for girls (relative to boys) exposed to FPE in all countries except Kenya. The results for the early FPE countries indicate that the cessation of FPE contributed to a slowing down of gender convergence in educational attainment in Ghana, Tanzania, Kenya, and Zambia.

7.3 FPE impacts on human capital in context

FPE policies act to stimulate the demand for education. Alternative strategies to increase human capital focus on the supply side, e.g.: school construction. The results of my analysis suggest that demand-side FPE policies are more effective at raising female educational attainment than supply-side school construction policies.

Duflo (2001)'s seminal paper on school construction uses a difference-in-differences strategy to show that a five-year 1970s program of building more primary schools in Indonesia raised schooling attainment of boys and led to higher male wages in adulthood. There are no results for girls in this paper. For each additional primary school built per 1,000 schoolchildren, schooling increased by 0.12 to 0.19 years among boys. If we assume that schooling was free after the additional school was built, for a child who was fully exposed to a new primary school, the estimated impact on years of attainment of 0.12 years below the schooling impacts estimated in Tables 2 and 3. Perhaps because boys already had a higher base level of education in Indonesia – 9 years among non-treated cohorts in low treatment districts – this particular school construction program had smaller impacts on total educational attainment than most of the FPE policies in the African countries studied here.

In a different setting, Khanna (2023) shows that more school construction funding in India raised attainment by 0.3 years among men and by only 0.09 years for women. He estimates difference-in-differences models to get at the average treatment effects of additional funding, showing that those most exposed to school construction in the *high school construction* districts experienced 0.3 more years of schooling, 4-5 percentage point increases in literacy and 3 percentage point increases in primary school completion. These are smaller impacts than generated by FPE policies in SSA. He also estimates a geographic RDD (where the local historical female literacy rate is the running variable) to estimate marginal treatment effects in districts eligible for primary school construction funding. The finding is that the marginal impact on education is larger than the average impact: total years of schooling rose

by about 0.7 years and primary school completion rose by 7 percentage points. Importantly, these results are for men. His results for Indian women are at the lower end of our estimates. This is in stark contrast to our setting, where FPE always raises girls' schooling, and always to a greater extent than for males.[32]

7.4 DiRD estimates of FPE on female human capital

To reassure that concerns about non-parallel trends are not driving the DiD results, Table 6 presents the DiRD results for female education outcomes using the specification in Equation (5). Panels A through D present results for each of the four key education outcomes (years of schooling, primary school completion, literacy, and any secondary schooling). Each cell presents the differences-in-regression discontinuity estimate of FPE exposure.

In all countries, the difference in RD estimates across treated and untreated Census waves is positive in panels A through C, although not significant for years of schooling in Zambia, or for literacy in Malawi and Uganda. Reflecting the DiD results, the DiRD estimates are positive in Ghana, Kenya, Malawi, and Uganda, and negative for Tanzania and Ethiopia.

Overall, this alternative research design corroborates what I find using the DiD results: that FPE raised human capital of females.[33]

8 Effects of FPE Exposure on Female Labor Market Outcomes

Tables 7 to 10 present results of the analysis of how FPE exposure impacts labor market outcomes across both early and late FPE countries. Table 7, Panel A and B both show that early FPE exposure significantly raises female LFP. In each of the four early FPE countries, Ghana, Kenya, Tanzania, and Zambia, female LFP rose by 2, 15, 8, and 4 percentage points respectively. These effect sizes are economically and statistically meaningful. Note that the trend in female LFP (the coefficient on $TreatedWave_t$) differs by country: in Ghana and Tanzania, female LFP is higher in the treated than untreated wave (meaning female LFP is falling over time), while in Kenya and Zambia, female LFP is lower in the treated than untreated wave (meaning female LFP is rising over time). Against these different background trends, early FPE raised female LFP in all four countries. In the late FPE countries of Malawi and Uganda, FPE exposure either reduced (in Uganda) or had no impact on (in Malawi)

[32]One reason for the smaller impacts in India could be the age of the sample: Khanna includes 15 to 75 years, while I restrict to about 20-40 years of age.

[33]Results using the same DiD design and DHS data also show very similar impacts on education outcomes for women. See Appendix C for results.

female LFP. The reduction in female LFP in Uganda is relatively small compared with the increases in LFP measured in the early FPE countries.[34]

Table 8 present the DiRD results that corroborate the DiD results: at the margin of being fully eligible for FPE, there is a positive jump in female LFP across the eligibility threshold for treated versus untreated waves in Ghana, Kenya, Tanzania, and Zambia, and a negative significant gap in Malawi and Uganda. Figure 3 collects the cross-country results for LFP in one figure, showing that where the impacts on female LFP are positive, the effects are large. In contrast, the late FPE countries show close to zero, or small negative, impacts of FPE exposure on female LFP.

When we consider employment (conditional on LFP) in Table 9, women exposed to FPE in the early period are more likely to work (Ghana, Tanzania, and Zambia), or have no change in employment probability (in Kenya). Note that the coefficient on $TreatedWave_t$ here is always positive, meaning that women's employment is falling over time in the early FPE countries. The increase in employment from FPE exposure is between 1.2 and 1.9 percentage points. In contrast, in Malawi and Uganda, greater FPE exposure significantly *reduces* employment. This reduction in women's work in Malawi is large, at over 12 percentage points; while employment probability falls by about 2.8 percentage points in Uganda.

Conditional on working, does FPE exposure shift women's work across sectors? Porzio et al. (2022) show that human capital is important for shifting men out of farming and into other sectors of the economy, and that this shift happens mostly across generations, rather than within a generation. In Table 10, I show DiD results for the outcome of whether a female worker works in non-agriculture. Regardless of whether women are entering or leaving the labor market because of FPE, there is some evidence that those women most exposed to FPE move towards the non-agricultural sector in Ghana (not significant), Tanzania and Uganda. There is not much change in the share of women working in non-agriculture in Zambia or Malawi.

9 Heterogeneity in the schooling-LFP relationship for women

In contrast with the findings for human capital, FPE exposure has more mixed impacts on women's market work. Early FPE increases female LFP and employment, while late FPE slightly reduces or has little impact on female LFP and employment. In some (but not all)

[34]In no country does greater FPE exposure reduce men's LFPR. See results for men in Appendix XX. FPE raises men's LFP in Uganda, Ghana, and Tanzania, and has no significant impact on men's LFP in Zambia or Malawi. There are no labor market outcomes for Kenya or Ethiopia.

countries, FPE exposure pushes women out of farming and towards the non-agricultural sector: however, given the size of the effects of FPE on educational attainment, the shifts out of agriculture seem relatively small. In this last part of the paper, I explore possible channels that might contribute to the differences in female LFP and employment impacts across Early and Late wave FPE countries.

9.1 Can family formation account for different impacts of FPE on female LFPR?

The first channel is tied to the income forces underlying the U-shape of female LFP. As Section 2 outlines, changes in female LFP over time tend to interact with changes in marriage markets and family formation through the development process. Along the U-shaped curve of LFP, female LFP falls when families get richer because income effects outweigh substitution effects at low levels of income. Higher levels of human capital in general (including for men) could raise incomes and lead to families buying more female leisure time. If more educated mothers produce higher quality children, then the relative value of women's time in the home versus the market will increase, affecting the relative substitution effect.[35] Income effects push women out of the labor market at low levels of income, while relative substitution effects pull women towards the home. However, as incomes grow further (and human capital increases even more), the opportunity cost of women's time spent in the home will increase to a large enough extent such that the income effect is dominated by the market substitution effect, and women (especially those with higher levels of schooling) start to work at higher rates.

For income and substitution effects to contribute to the changing pattern of female LFPR responses to FPE exposure, we should see lower marriage and family formation rates among women exposed to FPE in the early period (since these women are entering the labor market at higher rates) and higher marriage and family formation rates among those exposed in the later periods (since these women are leaving the labor force at higher rates).

Table 11 shows that this is not the case for these African countries. I estimate Equation (1) for a marriage indicator variable and for an "any children" dummy variable. The table shows that early FPE exposure increases marriage probabilities in Kenya and Tanzania, while reducing marriage probabilities in all other countries, whether early or late FPE adopters. These reduced form treatment effect magnitudes are very large: exposure to FPE in Kenya and Tanzania raises marriage rates among eligible cohorts by 6 to 8 percentage points,

[35] A recent paper by Duflo et al. (2024) uses RCT evidence to show that randomized access to free secondary schooling delays childbearing and marriage.

while depressing marriage rates by between 1 and 13 percentage points in Zambia, Ethiopia, Malawi, and Uganda. Without conditioning on marital status, Panel B of the table shows results for the indicator of whether the woman has ever had children. In Ghana and Tanzania, the number of children significantly increases with FPE exposure, while in all other countries, women with more FPE exposure reduce (potentially postponing) their fertility. I cannot measure completed fertility in Census data, and so the total impact on fertility is unclear.

These results show that a large income effect is unlikely to explain the increase in female LFP in early FPE countries, and the decline or limited change in female LFP in late FPE countries.[36] While FPE exposure does shift marriage and fertility outcomes, these channels cannot fully account for the patterns of impact of FPE exposure on female LFP.

9.2 How relevant are differences in available "clean" jobs for women?

The second channel which could contribute to different impacts of female schooling on female LFP is related to the structure of demand in each economy. As Goldin and others (e.g. Vidart (2023)) show, women with more schooling tend to move into the labor force when they can work in jobs that are considered "clean", e.g. office work and other white collar jobs.

In Table 12, I use detailed occupation-level data on five countries to describe the share of women and men who work in so-called clean jobs. I describe clean jobs as those occupations with ISCO codes in categories 1 through 4: Legislators, senior officials and managers; Professionals; Technicians and associate professionals; and Clerks.

For each country and in almost every Census wave, the share of women working in clean jobs is just 10% or less. In Malawi, one of the late FPE countries where schooling did not translate into higher female LFP, the share of women working in clean jobs is exceptionally low, at between 2 and 6%. In all countries, there is a higher share of men and women working in clean jobs in urban areas. In ongoing work, I will test for how the availability of clean jobs (measured by the share of men employed in such jobs in the local area) interacts with FPE exposure to affect how women with more education engage with the labor market. TBC.

10 Discussion and Conclusion

This paper uses Census data and seven natural experiments to examine the long-run impacts of FPE policies across a range of countries in Africa. There are two key findings. The first

[36]Results from the DHS are mostly consistent with Census results: women with more FPE exposure see higher marriage rates in Kenya, and lower marriage rates in the late FPE countries – Ethiopia, Malawi, and Uganda. There are no statistically significant results for Zambia and Tanzania.

is that FPE policies are effective at increasing girl's schooling attainment even when these policies end. I show that the introduction of FPE – regardless of when this occurred or for how long it was in place – significantly improved schooling outcomes of girls, shrinking gender gaps in education. The impacts of demand-side FPE policies are large relative to school construction episodes in other parts of the world, and they reduce education deficits to different degrees across countries: a 10 percentage point increase in primary school completion raised primary school completion in Ethiopia from 3.7 to 14 percent while in Tanzania, it raised primary school completion rates from 70% to 80%. Self-reported literacy rose in all of the early FPE countries. Moreover, the removal of early FPE policies retarded female human capital accumulation.

The second key finding is that increases in women's work alongside increases in schooling are not guaranteed along the development path. Despite these impressive gains in human capital for females in all countries, the impacts of FPE exposure on female labor market outcomes are mixed. Results indicate that women exposed to FPE in Early Wave countries increased their LFP but extensive margin participation fell, or did not change, in late FPE countries. Family formation channels cannot fully explain the different patterns of change in female LFP across countries. With very low shares of clean jobs in many countries, the structure of demand may limit possibilities for work among women with slightly higher levels of education.

These findings leave several questions open for future research. If countries continue to reduce barriers to female human capital attainment, and these educated women leave the labor market, how is aggregate productivity affected? To keep women and their daughters more connected to the labor market, are increases in female-specific labor demand required or is any increase in labor demand sufficient? For all of these questions, we need many more studies of individual countries in sub-Saharan Africa, to understand how local labor market conditions shape the choices that men and women can make.

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Figure and Tables Appendix

Figure 1: Correlation of LFP and Years of Schooling (Age-residualized) by Country

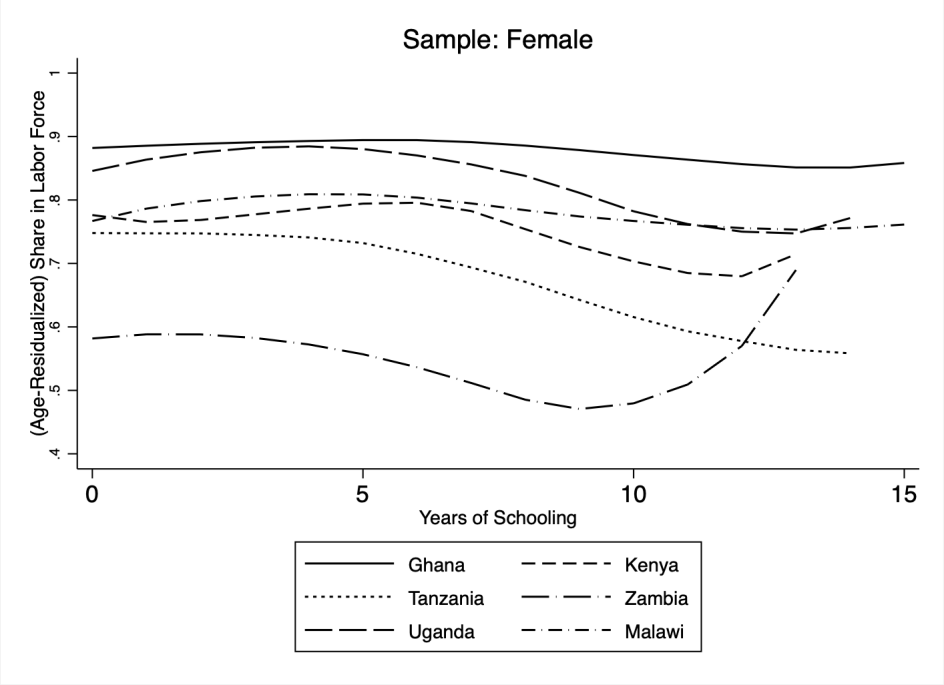
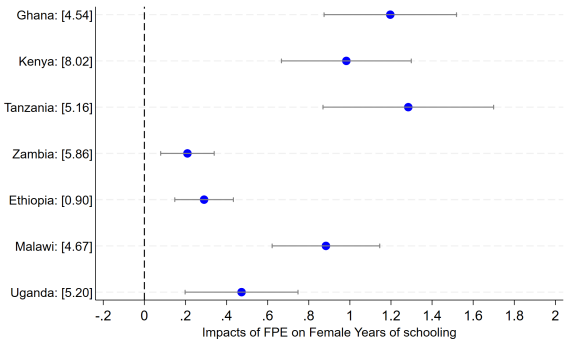
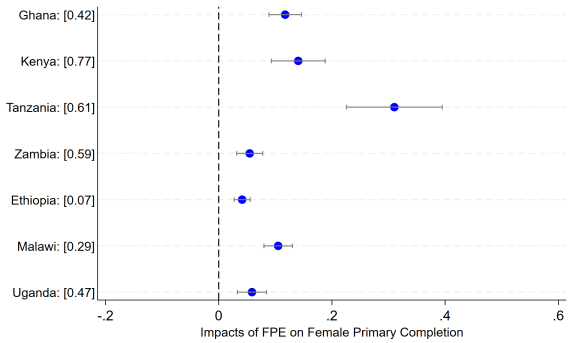


Figure shows age-residualized share of women in the labor force, by country, and level of schooling attained. Data are from the most recent Census wave in each country. Before plotting the figure, I add the mean female share in the labor force for each country to each age-residualized value of LFP.

Figure 2: DiD Estimates for Female Education Outcomes



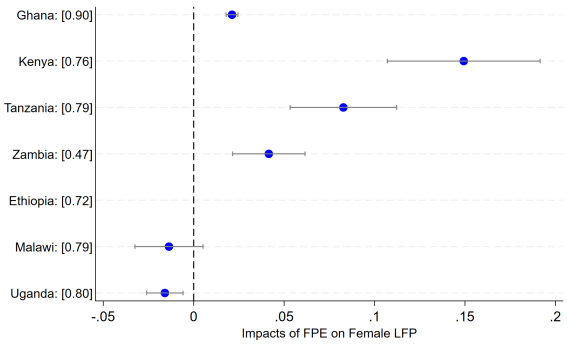
(a) Female Years of Schooling



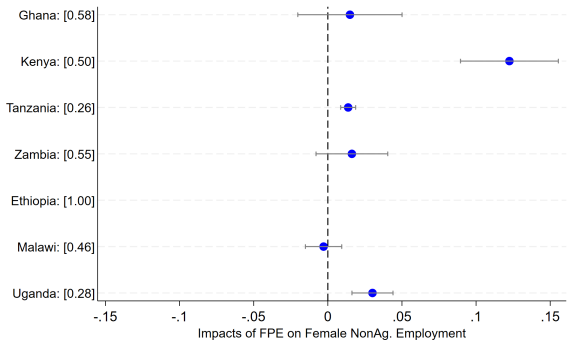
(b) Female Primary Completion

Notes: Each dot and whisker reports the estimated difference-in-differences coefficient and its confidence interval from Equation (1), estimated separately for each country. Numbers in square brackets report country-specific outcome means in the base (TreatedWave=0) year.

Figure 3: DiD Estimates for Female Labor Market Outcomes



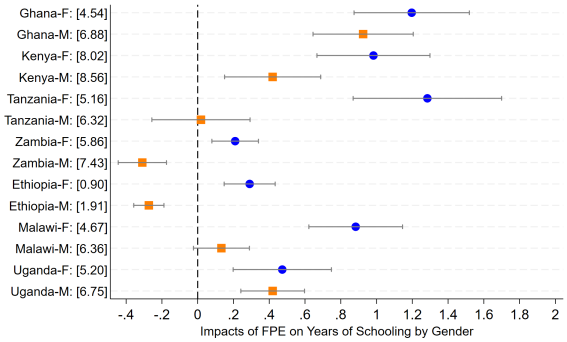
(a) Female LFP



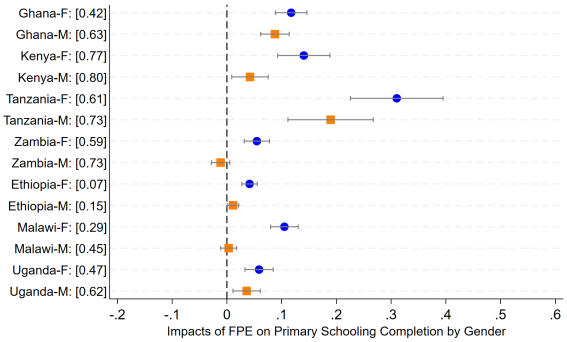
(b) Female Share in Non-Ag. Work

Notes: Each dot and whisker reports the estimated difference-in-differences coefficient and its confidence interval from Equation (1), estimated separately for each country. Numbers in square brackets report country-specific outcome means in the base (TreatedWave=0) year.

Figure 4: DiD Estimates for Education Outcomes: Males vs Females



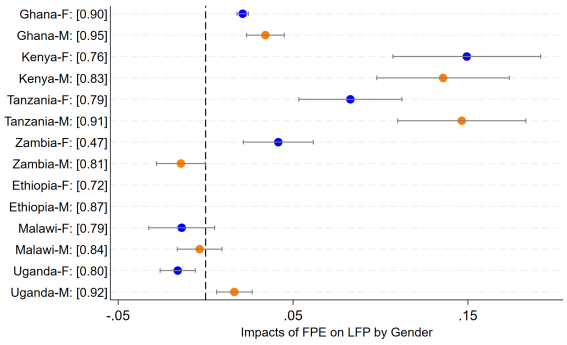
(a) Female vs Male Years of Schooling



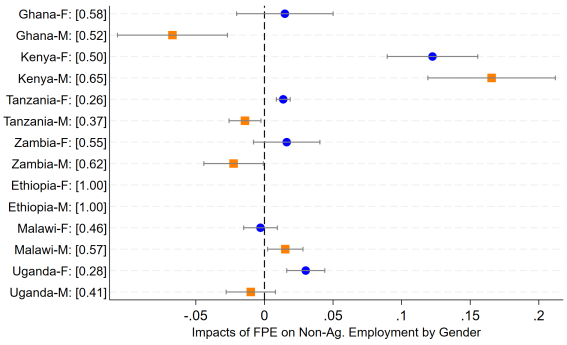
(b) Female vs Male Primary Completion

Notes: Each dot and whisker reports the estimated difference-in-differences coefficient and its confidence interval from Equation (1), estimated separately for each country and each gender. Blue is female, orange is male. Numbers in square brackets report country- and gender-specific outcome means in the base (TreatedWave=0) year.

Figure 5: DiD Estimates for Labor Market Outcomes: Males vs Females



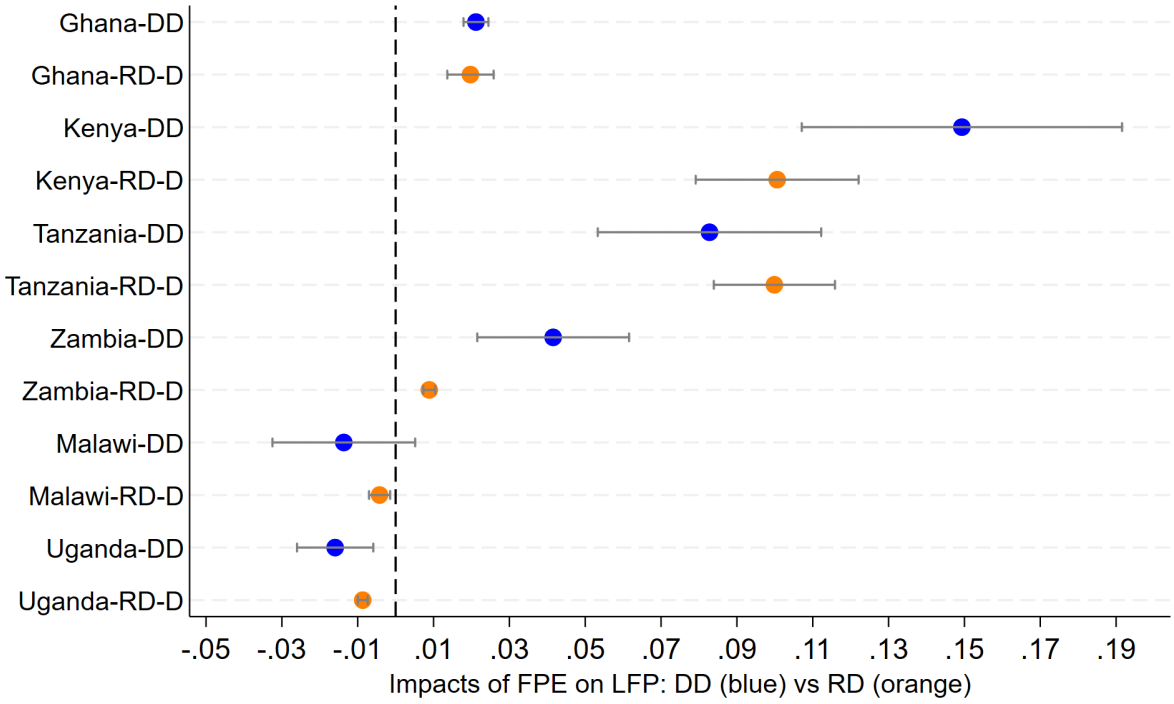
(a) Female vs Male LFP



(b) Female vs Male Share in Non-Ag. Work

Notes: Each dot and whisker reports the estimated difference-in-differences coefficient and its confidence interval from Equation (1), estimated separately for each country and each gender. Blue is female, orange is male. Numbers in square brackets report country- and gender-specific outcome means in the base (TreatedWave=0) year.

Figure 6: DiD and D-in-RD Estimates of Female LFP



Note: Each figure plots the difference-in-differences coefficient from equation (1) in blue and the differences-in-RD coefficient from equation (5) in orange for the outcome of female LFP.

Table 1

Summary Statistics: Females

	Ghana		Kenya		Tanzania		Zambia		Ethiopia		Malawi		Uganda	
	1984	**2010**	**1989**	**2019**	**1988**	**2012**	**1990**	**2010**	**1984**	**2007**	**1987**	**2018**	**1991**	**2014**
	Treated	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Treated**
Education Variables														
Years of schooling	2.697	5.665	5.390	8.535	3.523	5.959	4.773	6.519	0.574	1.672	2.070	5.938	3.173	6.100
Completed primary	0.250	0.515	0.567	0.804	0.405	0.718	0.502	0.636	0.037	0.143	0.097	0.386	0.289	0.552
Any secondary	0.226	0.480	0.300	0.714	0.044	0.162	0.244	0.413	0.032	0.077	0.035	0.268	0.112	0.320
Literacy	.	0.619	0.733	.	0.556	0.770	0.686	0.797	0.170	0.283	0.355	0.747	0.465	0.640
Labor Market Variables														
In labor force	0.920	0.885	0.755	0.757	0.883	0.741	0.339	0.549	.	0.724	0.806	0.776	0.683	0.848
Employed	0.995	0.963	0.932	0.861	0.996	0.972	0.947	0.910	.	0.969	0.996	0.818	0.995	0.972
Class of Work Variables														
Wage worker	0.078	0.117	.	0.473	0.081	0.094	0.360	0.206	.	0.092	0.042	0.085	0.069	0.397
Self-employed	0.783	0.766	.	0.271	0.886	0.865	0.266	0.397	.	0.540	0.954	0.753	0.306	0.530
Unpaid worker	0.138	0.116	.	0.242	0.026	0.039	0.374	0.397	.	0.314	0.004	0.136	0.624	0.073
Other worker	0.001	0.001	.	0.014	0.006	0.001	.	.	.	0.054	.	0.027	.	.
Sector of Work Variables														
Agricultural worker	0.544	0.354	0.508	0.495	0.879	0.665	0.342	0.490	.	.	0.949	0.305	0.783	0.695
Manufacturing worker	0.143	0.143	0.022	0.029	0.003	0.034	0.059	0.033	.	.	0.012	0.063	0.030	0.022
Mid and low service worker	0.274	0.426	0.310	0.275	0.080	0.185	0.196	0.292	.	.	0.025	0.572	0.142	0.187
High service worker	0.036	0.077	0.050	0.193	0.037	0.086	0.109	0.096	.	.	0.011	0.049	0.034	0.039
Unspecified	0.003	0.001	0.110	0.008	0.001	0.029	0.294	0.089	.	.	0.003	0.011	0.012	0.057

Table notes: Means of various outcome variables are provided for the main sample of females in selected age groups from the relevant census year for each country. . Means the variable is not available in the particular census wave.

Table 2: Effects of Early FPE on Female Human Capital Quantity

	Panel A: Strict Treatment							
	Ghana		**Kenya**		**Tanzania**		**Zambia**	
	YrsEdu	ComPrim	YrsEdu	ComPrim	YrsEdu	ComPrim	YrsEdu	ComPrim
Eligible*Treated Wave	1.197***	0.117***	0.983***	0.141***	1.284***	0.310***	0.210**	0.055***
	(0.164)	(0.015)	(0.161)	(0.024)	(0.212)	(0.043)	(0.066)	(0.012)
Treated Wave	-3.037***	-0.274***	-3.428***	-0.273***	-2.793***	-0.370***	-1.920***	-0.179***
	(0.158)	(0.015)	(0.127)	(0.024)	(0.212)	(0.042)	(0.046)	(0.009)
N	402,569	402,569	862,505	862,505	972,455	972,455	211,826	211,826
Adj R-Squared	0.115	0.099	0.131	0.086	0.155	0.155	0.042	0.021
Control Wave's Mean	4.535	0.424	8.022	0.766	5.161	0.615	5.862	0.586
	Panel B: Intensity Treatment							
Intensity*Treated Wave	1.306***	0.128***	1.256***	0.207***	1.915***	0.434***	0.572***	0.143***
	(0.224)	(0.020)	(0.145)	(0.021)	(0.269)	(0.049)	(0.113)	(0.020)
Treated Wave	-3.181***	-0.288***	-3.693***	-0.322***	-3.101***	-0.438***	-2.283***	-0.268***
	(0.187)	(0.017)	(0.107)	(0.017)	(0.164)	(0.029)	(0.072)	(0.014)
N	402,569	402,569	862,505	862,505	972,455	972,455	211,826	211,826
Adj R-Squared	0.115	0.099	0.132	0.090	0.160	0.169	0.042	0.021
Control Wave's Mean	4.535	0.424	8.022	0.766	5.161	0.615	5.862	0.586

Table notes: Estimates are person weighted, include cohort FE, and SE are clustered on age. * for significance levels.

Table 3: Effects of Late FPE on Female Human Capital Quantity

	Panel A: Strict Treatment					
	Ethiopia		**Malawi**		**Uganda**	
	YrsEdu	ComPrim	YrsEdu	ComPrim	YrsEdu	ComPrim
Eligible*Treated Wave	0.291***	0.041***	0.883***	0.105***	0.473**	0.059***
	(0.073)	(0.007)	(0.134)	(0.013)	(0.140)	(0.013)
Treated Wave	0.954***	0.092***	3.250***	0.225***	2.756***	0.242***
	(0.071)	(0.007)	(0.120)	(0.010)	(0.124)	(0.012)
N	867,611	867,611	455,612	455,612	859,029	859,029
Adj R-Squared	0.070	0.060	0.212	0.115	0.168	0.116
Control Wave's Mean	0.902	0.069	4.667	0.293	5.200	0.471
	Panel B: Intensity Treatment					
Intensity*Treated Wave	0.509***	0.059***	1.131***	0.127***	0.950***	0.102***
	(0.108)	(0.011)	(0.153)	(0.014)	(0.137)	(0.011)
Treated Wave	0.832***	0.078***	2.995***	0.199***	2.499***	0.217***
	(0.073)	(0.007)	(0.134)	(0.011)	(0.064)	(0.007)
N	867,611	867,611	455,612	455,612	859,029	859,029
Adj R-Squared	0.070	0.060	0.212	0.115	0.170	0.117
Control Wave's Mean	0.902	0.069	4.667	0.293	5.200	0.471

Table notes: Estimates are person weighted, include cohort FE, and SE are clustered on age. * for significance levels.

Table 4: Effects of Early and Late FPE on Literacy and Any Secondary Schooling: Strict Treatment

	Panel A: Literacy						
	Ghana	**Kenya**	**Tanzania**	**Zambia**	**Ethiopia**	**Malawi**	**Uganda**
Eligible*Treated Wave	N/A	N/A	0.209***	0.043**	0.067***	0.025	-0.014
	N/A	N/A	(0.031)	(0.012)	(0.010)	(0.016)	(0.009)
Treated Wave	N/A	N/A	-0.257***	-0.145***	0.084***	0.372***	0.168***
	N/A	N/A	(0.030)	(0.010)	(0.007)	(0.012)	(0.007)
N	N/A	N/A	972,449	211,826	867,611	455,612	859,029
Adj R-Squared	N/A	N/A	0.097	0.018	0.066	0.175	0.077
Control Wave's Mean	N/A	N/A	0.700	0.755	0.202	0.625	0.586
	Panel B: Any Secondary Schooling						
Eligible*Treated Wave	0.110***	0.096***	-0.143***	-0.012	-0.040***	0.121***	0.094***
	(0.014)	(0.013)	(0.021)	(0.007)	(0.004)	(0.014)	(0.016)
Treated Wave	-0.261***	-0.442***	-0.118***	-0.159***	0.043***	0.160***	0.186***
	(0.014)	(0.008)	(0.010)	(0.005)	(0.003)	(0.010)	(0.015)
N	402,569	862,505	972,455	211,826	867,611	455,612	859,029
Adj R-Squared	0.093	0.165	0.065	0.034	0.028	0.099	0.093
Control Wave's Mean	0.392	0.646	0.123	0.349	0.045	0.191	0.256

Table notes: Estimates are person weighted, include cohort FE, and SE are clustered on age. * for significance levels. N/A means outcome variable was missing in one or both Census years.

Table 5: Differential Effects of FPE on Human Capital of Women vs Men: Strict Treatment

	Strict Treatment: Yrs of Schooling						
	Ghana	**Tanzania**	**Kenya**	**Zambia**	**Ethiopia**	**Malawi**	**Uganda**
Female*Eligible*Treated Wave	0.255***	1.270***	0.540***	0.511***	0.570***	0.766***	0.072**
	(0.063)	(0.035)	(0.035)	(0.068)	(0.039)	(0.037)	(0.032)
Female*Treated Wave	-1.070***	-1.342***	-1.581***	-0.391***	-0.735***	0.593***	0.848***
	(0.027)	(0.012)	(0.016)	(0.060)	(0.012)	(0.027)	(0.015)
Female	-2.032***	-0.793***	-0.455***	-1.836***	-0.788***	-2.520***	-2.406***
	(0.015)	(0.007)	(0.009)	(0.035)	(0.010)	(0.023)	(0.013)
Treated Wave	-1.970***	-1.455***	-1.836***	-1.526***	1.687***	2.671***	1.906***
	(0.019)	(0.009)	(0.012)	(0.043)	(0.009)	(0.019)	(0.011)
N	762,767	1,793,708	1,648,979	397,766	1,651,375	868,965	1,667,574
Adj R-Squared	0.135	0.136	0.099	0.071	0.100	0.187	0.147
Control Wave's Mean	5.642	5.693	8.281	6.594	1.381	5.470	5.953

Table notes: Estimates are person weighted, include cohort FE, and SE are clustered on age. * for significance levels.

Table 6: Effects of Early and Late FPE on Female Human Capital: Difference-in-RD Estimates

	Panel A: Yrs of Schooling						
	Ghana	**Kenya**	**Tanzania**	**Zambia**	**Ethiopia**	**Malawi**	**Uganda**
D*Treat Wave	1.038***	0.623***	1.214***	0.022	0.288***	0.104***	0.147*
	(0.214)	(0.147)	(0.221)	(0.014)	(0.070)	(0.017)	(0.063)
N	402,569	862,505	972,455	211,826	867,611	455,612	859,029
Adj R-Squared	0.092	0.126	0.144	0.042	0.062	0.210	0.164
Control Wave's Mean	5.458	8.535	5.959	6.519	0.574	2.069	3.170
	Panel B: Primary School Completion						
D*Treat Wave	0.109***	0.080**	0.310***	0.008***	0.040***	0.014***	0.023***
	(0.020)	(0.024)	(0.045)	(0.002)	(0.007)	(0.002)	(0.006)
N	402,569	862,505	972,455	211,826	867,611	455,612	859,029
Adj R-Squared	0.079	0.080	0.142	0.020	0.054	0.114	0.113
Control Wave's Mean	0.507	0.804	0.718	0.636	0.037	0.097	0.289
	Panel C: Literacy						
D*Treat Wave	N/A	N/A	0.212***	0.007***	0.072***	0.000	-0.005
	N/A	N/A	(0.032)	(0.001)	(0.007)	(0.002)	(0.003)
N	N/A	N/A	972,449	211,826	867,611	455,612	859,029
Adj R-Squared	N/A	N/A	0.085	0.018	0.061	0.173	0.074
Control Wave's Mean	N/A	N/A	0.770	0.797	0.170	0.355	0.465
	Panel D: Any Secondary Schooling						
D*Treat Wave	0.105***	0.060***	-0.164***	-0.003	-0.042***	0.017***	0.037***
	(0.019)	(0.014)	(0.013)	(0.001)	(0.003)	(0.002)	(0.009)
N	402,569	862,505	972,455	211,826	867,611	455,612	859,029
Adj R-Squared	0.073	0.162	0.060	0.034	0.022	0.098	0.090
Control Wave's Mean	0.471	0.714	0.162	0.413	0.032	0.035	0.112

Notes: Estimates are person weighted. Strict equals one for individuals who are at or above the strict eligibility threshold for FPE, and zero otherwise. AgeRD is the running variable representing the distance to the eligibility threshold, centered at zero. N/A means outcome is missing for one or both Census waves. Robust standard errors are clustered by AgeRD. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Effects of Early and Late FPE Exposure on Female's LFP

	Outcome: LFP					
	Panel A: Strict Treatment					
	Ghana	**Kenya**	**Tanzania**	**Zambia**	**Malawi**	**Uganda**
Eligible*Treated Wave	0.021***	0.149***	0.083***	0.042**	-0.014	-0.016**
	(0.002)	(0.022)	(0.015)	(0.010)	(0.010)	(0.005)
Treated Wave	0.033***	-0.023*	0.148***	-0.243***	-0.022***	0.155***
	(0.002)	(0.010)	(0.007)	(0.005)	(0.004)	(0.002)
N	402,354	861,412	972,330	211,826	455,612	854,491
Adj R-Squared	0.005	0.077	0.046	0.044	0.022	0.045
Control Wave's Mean	0.895	0.756	0.787	0.470	0.786	0.798
	Panel B: Intensity Treatment					
Intensity*Treated Wave	0.023***	0.164***	0.097***	0.101***	-0.006	-0.012**
	(0.001)	(0.017)	(0.011)	(0.027)	(0.010)	(0.005)
Treated Wave	0.030***	-0.053***	0.135***	-0.303***	-0.025***	0.156***
	(0.001)	(0.004)	(0.004)	(0.021)	(0.005)	(0.002)
N	402,354	861,412	972,330	211,826	455,612	854,491
Adj R-Squared	0.005	0.078	0.047	0.044	0.022	0.045
Control Wave's Mean	0.895	0.756	0.787	0.470	0.786	0.798

Table notes: Estimates are person weighted, include cohort FE, and SE are clustered on age. * for significance levels.

Table 8: Effects of Early and Late FPE on Female LFP: Difference-in-RD Estimates

	LFP						
	Ghana	**Kenya**	**Tanzania**	**Zambia**	**Ethiopia**	**Malawi**	**Uganda**
D*Treat Wave	0.020***	0.101***	0.100***	0.009***	N/A	-0.004**	-0.009***
	(0.003)	(0.011)	(0.008)	(0.001)	N/A	(0.001)	(0.001)
N	402,354	861,412	972,330	211,826	N/A	455,612	854,491
Adj R-Squared	0.005	0.074	0.045	0.045	N/A	0.022	0.044
Control Wave's Mean	0.884	0.757	0.741	0.549	N/A	0.806	0.683

Notes: Estimates are person weighted. Strict equals one for individuals who are at or above the strict eligibility threshold for FPE, and zero otherwise. AgeRD is the running variable representing the distance to the eligibility threshold, centered at zero. N/A means outcome variable not available in one or both Census waves. Robust standard errors are clustered by AgeRD. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Effects of Early and Late FPE Exposure on Female's Employment

	Outcome: Employment					
	Panel A: Strict Treatment					
	Ghana	**Kenya**	**Tanzania**	**Zambia**	**Malawi**	**Uganda**
Eligible*Treated Wave	0.019***	-0.010	0.016***	0.012*	-0.121***	-0.028***
	(0.004)	(0.008)	(0.002)	(0.004)	(0.017)	(0.002)
Treated Wave	0.029***	0.081***	0.026***	0.029***	-0.119***	-0.017***
	(0.002)	(0.004)	(0.002)	(0.003)	(0.006)	(0.002)
N	360,252	651,496	765,617	99,492	358,016	681,995
Adj R-Squared	0.013	0.020	0.012	0.015	0.093	0.016
Control Wave's Mean	0.973	0.872	0.981	0.920	0.873	0.978
	Panel B: Intensity Treatment					
Intensity*Treated Wave	0.024***	0.004	0.021***	0.027***	-0.138***	-0.034***
	(0.004)	(0.008)	(0.002)	(0.008)	(0.017)	(0.001)
Treated Wave	0.026***	0.078***	0.023***	0.014**	-0.095***	-0.011***
	(0.002)	(0.005)	(0.001)	(0.006)	(0.005)	(0.001)
N	360,252	651,496	765,617	99,492	358,016	681,995
Adj R-Squared	0.013	0.019	0.012	0.015	0.093	0.016
Control Wave's Mean	0.973	0.872	0.981	0.920	0.873	0.978

Table notes: Estimates are person weighted, include cohort FE, and SE are clustered on age. * for significance levels.

Table 10: Exposure to FPE and Non-Ag. Employment Shares of Females: Strict Treatment

	Panel A: Non-Agric. Employment				
	Ghana	**Tanzania**	**Zambia**	**Malawi**	**Uganda**
Eligible*Treated Wave	0.015	0.023***	0.006	-0.003	0.030***
	(0.018)	(0.003)	(0.015)	(0.006)	(0.006)
Treated Wave	-0.186***	-0.233***	-0.059**	0.635***	0.039***
	(0.012)	(0.002)	(0.015)	(0.004)	(0.006)
N	350,509	751,125	91,567	312,671	667,035
Adj R-Squared	0.049	0.071	0.007	0.374	0.010
Control Wave's Mean	0.577	0.236	0.406	0.455	0.237

Notes: Estimates are person weighted. All models include cohort fixed effects. Robust standard errors clustered by age. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Effects of Early and Late FPE on Female Marriage Probability and Fertility: Strict Treatment

	Panel A: Ever Married					
	Kenya	**Tanzania**	**Zambia**	**Ethiopia**	**Malawi**	**Uganda**
Eligible*Treated Wave	0.080***	0.063***	-0.010*	-0.132***	-0.070***	-0.086***
	(0.012)	(0.007)	(0.004)	(0.016)	(0.015)	(0.014)
Treated Wave	0.088***	0.090***	0.001	-0.063***	-0.012***	-0.020**
	(0.009)	(0.006)	(0.004)	(0.015)	(0.001)	(0.006)
N	861,671	972,454	211,826	867,398	453,027	858,815
Adj R-Squared	0.175	0.084	0.042	0.199	0.105	0.095
Control Wave's Mean	0.690	0.816	0.858	0.909	0.920	0.877
	Panel B: Has Had Any Children					
Eligible*Treated Wave	0.174***	0.062***	-0.012***	-0.181***	-0.056***	-0.104***
	(0.021)	(0.012)	(0.003)	(0.016)	(0.013)	(0.011)
Treated Wave	0.094***	0.005	-0.111***	-0.067***	0.009***	-0.005
	(0.020)	(0.006)	(0.001)	(0.016)	(0.001)	(0.008)
N	847,971	965,843	211,826	766,918	453,638	852,935
Adj R-Squared	0.238	0.074	0.043	0.245	0.112	0.108
Control Wave's Mean	0.754	0.888	0.776	0.843	0.903	0.873

Table notes: Estimates are person weighted, include cohort FE, and SE are clustered on age. * for significance levels.

Table 12: Female Employment Shares in "Clean" Jobs by Urban and Rural area in each Country

Country	Rural	Urban
Ghana	.03	.14
Kenya	.10	.41
Malawi	.03	.18
Tanzania	.06	.17
Uganda	.02	.25
Zambia	.05	.34

Notes: Table shows the share of females who are employed in "clean" jobs. Clean jobs are those occupations with ISCO codes in categories 1 through 4: Legislators, senior officials and managers; Professionals; Technicians and associate professionals; and Clerks. Ethiopia and Uganda are not included in the table because they do not have ISCO-coded occupations in both Census waves.

A Online Additional Results Appendix - Not for Publication

Table A1: FPE Policy Dates, Age Eligibility Rules, and Analysis Data

	Early FPE		Late FPE		Analysis Data (Census)	
Country	Start/End	Age Elig.	Start	Age Elig.	Early FPE	Late FPE
Ghana*	1961/1965	6 to 11	2005	6 to 11	1984/2010	-
Kenya*	1974/1988	6 to 11	2003	6 to 11	1989/2019	-
Tanzania*	1974/1978	7 to 12	2002	7 to 12	1988/2012	-
Zambia*	1965/1980	7 to 13	2002	7 to 13	1990/2010	-
Ethiopia	-	-	1995	7 to 16	-	1984/2007
Malawi	-	-	1994	6 to 13	-	1987/2018
Uganda	-	-	1997	6 to 12	-	1991/2014

Table notes: Start and end dates of Early FPEs, as well as start dates for Late FPEs, are based on institutional documents and legal ordinances referenced in Dinkelman et al. (2026). Age eligibility corresponds to the age range in which individuals are eligible for primary schooling in each country. *Country has a late wave of FPE but it is too early to use Census data for analyses of outcomes in adulthood for exposed cohorts.

Table A2: Illustration: Malawi, Late FPE in 1994 using Birth Cohorts and Age at Census Time

	Panel A: Using Birth Cohorts	
	TREATED CENSUS (2018)	UNTREATED CENSUS (1987)
	Age 20 to 48	Age 20 to 48
Strictly Eligible	b. 1988-1998	b.1957-1967
Partially Eligible	b. 1981-1987	b.1950-1956
Not Eligible	b. 1970-1980	b.1939-1949
	Panel B: Using Age at Census Time	
	TREATED CENSUS (2018)	UNTREATED CENSUS (1987)
Strictly Eligible	20-30	20-30
Partially Eligible	31-37	31-37
Not Eligible	38-48	38-48

Table notes: FPE was introduced in Malawi in 1994. Panel A illustrates how age in years defines whether someone is in a strictly or partially eligible cohort, or control cohort, in the treated Census year. These same age definitions are applied to the same *ages* in the untreated Census wave. Panel B shows that these ages demarcating strict or partial eligibility, or control group, belong to different birth *cohorts* in the treated and untreated Census waves. We choose the Census waves and age range to ensure that birth cohorts do not overlap across Census waves.

Table A3: **Summary Statistics: Male Sample**

	Ghana		Kenya		Tanzania		Zambia		Ethiopia		Malawi		Uganda	
	1984	**2010**	**1989**	**2019**	**1988**	**2012**	**1990**	**2010**	**1984**	**2007**	**1987**	**2018**	**1991**	**2014**
	Treated	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Treated**
Education Variables														
Years of schooling	6.042	7.903	7.127	8.851	5.519	6.720	6.286	8.046	1.379	3.119	4.430	7.354	5.387	7.333
Completed primary	0.542	0.689	0.736	0.813	0.639	0.773	0.648	0.779	0.097	0.270	0.287	0.525	0.507	0.665
Any secondary	0.517	0.662	0.455	0.735	0.103	0.224	0.400	0.584	0.080	0.137	0.113	0.401	0.241	0.418
Literacy .	.	0.758	0.886	.	0.819	0.846	0.838	0.888	0.367	0.520	0.692	0.842	0.734	0.773
Labor Market Variables														
In labor force	0.982	0.935	0.889	0.815	0.966	0.881	0.715	0.861	.	0.867	0.900	0.820	0.912	0.925
Employed	0.990	0.970	0.924	0.830	0.991	0.960	0.931	0.895	.	0.975	0.979	0.867	0.988	0.979
Class of Work Variables														
Wage worker	0.322	0.302	.	0.618	0.222	0.160	0.499	0.368	.	0.144	0.339	0.205	0.231	0.437
Self-employed	0.618	0.659	.	0.205	0.757	0.815	0.298	0.472	.	0.701	0.658	0.623	0.611	0.514
Unpaid worker	0.049	0.038	.	0.168	0.010	0.023	0.203	0.161	.	0.114	0.003	0.126	0.158	0.049
Other worker	0.010	0.002	.	0.009	0.011	0.002	.	.	.	0.041	.	0.047	.	.
Sector of Work Variables														
Agricultural worker	0.597	0.402	0.284	0.360	0.690	0.597	0.347	0.401	.	.	0.716	0.270	0.620	0.575
Manufacturing worker	0.126	0.297	0.162	0.197	0.011	0.107	0.184	0.188	.	.	0.108	0.079	0.087	0.091
Mid and low service worker	0.163	0.163	0.331	0.248	0.210	0.168	0.160	0.248	.	.	0.119	0.561	0.202	0.228
High service worker	0.090	0.134	0.078	0.177	0.088	0.100	0.094	0.095	.	.	0.029	0.074	0.074	0.035
Unspecified	0.024	0.004	0.145	0.018	0.001	0.027	0.215	0.067	.	.	0.028	0.016	0.016	0.071

Table notes: Means of various outcome variables are provided for the main sample of females in selected age groups from the earliest census year for each country. Means the variable is not available in the particular census wave.

Table A4: Differential Effects of FPE on Primary School Completion of Women vs Men: Strict Treatment

	Strict Treatment: Primary School Completion						
	Ghana	**Tanzania**	**Kenya**	**Zambia**	**Ethiopia**	**Malawi**	**Uganda**
Female*Eligible*Treated Wave	0.028***	0.121***	0.096***	0.065***	0.032***	0.104***	0.025***
	(0.006)	(0.004)	(0.003)	(0.008)	(0.004)	(0.004)	(0.004)
Female*Treated Wave	-0.119***	-0.192***	-0.185***	-0.056***	-0.074***	-0.002	0.088***
	(0.003)	(0.001)	(0.002)	(0.007)	(0.001)	(0.003)	(0.002)
Female	-0.170***	-0.060***	-0.015***	-0.162***	-0.058***	-0.187***	-0.240***
	(0.001)	(0.001)	(0.001)	(0.004)	(0.001)	(0.003)	(0.001)
Treated Wave	-0.156***	-0.179***	-0.087***	-0.123***	0.165***	0.228***	0.154***
	(0.002)	(0.001)	(0.001)	(0.005)	(0.001)	(0.002)	(0.001)
N	762,767	1,793,708	1,648,979	397,766	1,651,375	868,965	1,667,574
Adj R-Squared	0.116	0.124	0.059	0.044	0.082	0.107	0.096
Control Wave's Mean	0.520	0.667	0.782	0.655	0.107	0.367	0.543

Table notes: Estimates are person weighted, include cohort FE, and SE are clustered on age. * for significance levels.

Table A5: Differential Effects of FPE on Any Secondary Schooling of Women vs Men: Strict Treatment

	Strict Treatment: Any Secondary Schooling						
	Ghana	**Tanzania**	**Kenya**	**Zambia**	**Ethiopia**	**Malawi**	**Uganda**
Female*Eligible*Treated Wave	0.025***	0.071***	0.000	0.044***	0.027***	0.056***	0.022***
	(0.006)	(0.003)	(0.004)	(0.008)	(0.003)	(0.004)	(0.004)
Female*Treated Wave	-0.108***	0.002*	-0.138***	-0.018***	-0.018***	-0.084***	0.019***
	(0.003)	(0.001)	(0.002)	(0.007)	(0.001)	(0.003)	(0.002)
Female	-0.178***	-0.061***	-0.032***	-0.206***	-0.047***	-0.081***	-0.137***
	(0.001)	(0.001)	(0.001)	(0.004)	(0.001)	(0.002)	(0.001)
Treated Wave	-0.154***	-0.120***	-0.303***	-0.141***	0.060***	0.244***	0.167***
	(0.002)	(0.001)	(0.001)	(0.005)	(0.001)	(0.002)	(0.001)
N	762,767	1,793,708	1,648,979	397,766	1,651,375	868,965	1,667,574
Adj R-Squared	0.113	0.068	0.129	0.061	0.034	0.108	0.080
Control Wave's Mean	0.490	0.151	0.667	0.429	0.070	0.247	0.309

Table notes: Estimates are person weighted, include cohort FE, and SE are clustered on age. * for significance levels.

Table A6: Differential Effects of FPE on Literacy Rates of Women vs Men: Strict Treatment

	Strict Treatment: Literate				
	Tanzania	**Zambia**	**Ethiopia**	**Malawi**	**Uganda**
Female*Eligible*Treated Wave	0.154***	0.058***	0.087***	0.025***	-0.010***
	(0.004)	(0.006)	(0.005)	(0.004)	(0.004)
Female*Treated Wave	-0.206***	-0.107***	-0.051***	0.230***	0.127***
	(0.001)	(0.006)	(0.002)	(0.003)	(0.002)
Female	-0.079***	-0.106***	-0.197***	-0.381***	-0.294***
	(0.001)	(0.003)	(0.001)	(0.002)	(0.001)
Treated Wave	-0.051***	-0.038***	0.136***	0.145***	0.041***
	(0.001)	(0.004)	(0.001)	(0.002)	(0.001)
N	1,793,698	397,766	1,651,375	868,965	1,667,574
Adj R-Squared	0.090	0.034	0.109	0.150	0.079
Control Wave's Mean	0.763	0.809	0.302	0.706	0.671

Table notes: Estimates are person weighted, include cohort FE, and SE are clustered on age. * for significance levels.

Table A7: Differential Effects of FPE on LFP of Women vs Men

	Outcome: LFP					
	Panel A: Strict Treatment					
	Ghana	**Kenya**	**Tanzania**	**Zambia**	**Malawi**	**Uganda**
Female*Eligible*Treated Wave	-0.014*	0.013**	-0.064***	0.056***	-0.004	-0.034***
	(0.005)	(0.005)	(0.005)	(0.008)	(0.015)	(0.002)
Female*Treated Wave	-0.012***	-0.075***	0.079***	-0.111***	0.051***	0.150***
	(0.002)	(0.003)	(0.004)	(0.003)	(0.005)	(0.002)
Female	-0.051***	-0.081***	-0.159***	-0.316***	-0.118***	-0.229***
	(0.001)	(0.007)	(0.004)	(0.003)	(0.005)	(0.004)
Treated Wave	0.045***	0.052***	0.069***	-0.132***	-0.074***	0.006**
	(0.002)	(0.012)	(0.010)	(0.004)	(0.002)	(0.002)
N	762,349	1,646,226	1,793,450	397,766	868,965	1,654,522
Adj R-Squared	0.019	0.138	0.082	0.164	0.055	0.076
Control Wave's Mean	0.922	0.790	0.843	0.629	0.813	0.858
	Panel B: Intensity Treatment					
Female*Intensity*Treated Wave	-0.013***	0.003	-0.071***	0.131***	0.006	-0.034***
	(0.004)	(0.008)	(0.003)	(0.029)	(0.015)	(0.004)
Female*Treated Wave	-0.011***	-0.075***	0.087***	-0.187***	0.044***	0.155***
	(0.002)	(0.003)	(0.002)	(0.025)	(0.006)	(0.002)
Female	-0.051***	-0.099***	-0.167***	-0.321***	-0.116***	-0.234***
	(0.001)	(0.004)	(0.004)	(0.015)	(0.008)	(0.005)
Treated Wave	0.041***	0.021***	0.048***	-0.116***	-0.071***	0.002*
	(0.001)	(0.002)	(0.004)	(0.011)	(0.003)	(0.001)
N	762,349	1,646,226	1,793,450	397,766	868,965	1,654,522
Adj R-Squared	0.019	0.140	0.084	0.164	0.055	0.076
Control Wave's Mean	0.922	0.790	0.843	0.629	0.813	0.858

Table notes: Estimates are person weighted, include cohort FE, and SE are clustered on age. * for significance levels.

B Online Variables Appendix - Not for Publication

B.1 Education Variables

I construct four main education variables:

- Years of Schooling: a continuous variable describing completed educational attainment
- Completed Primary: an indicator variable describing whether a person has completed X years of primary schooling, where X is country-specific and generally between 6 and 7 years.
- Literacy: an indicator variable describing whether a person has basic literacy, meaning that they can read and write in the language that they speak.
- Ever enrolled in high school: an indicator variable for whether someone moved on from primary school to at least start high school.

Country-specific notes on variable construction:

- Ethiopia 2007: a random 20% of the population was administered the long-form Census questionnaire, which included the education questions. Hence, there is no education information for the majority of the population. However, the 20% sample that I use is nationally representative.[37]
- Ethiopia: 1984-literacy is defined as completed Grade 1-3 and able to read and write, or Grade 4 and higher; 1994-completed Grade 1 or higher; 2007-able to read and write in spoken language.
- Ghana 2000, 2010: literacy is measured in English and/or Ghanaian and/or any other language
- Kenya 1989: literacy is yes/no, only available in one year. No data prior to 2000.
- Malawi 1987-literacy in English/Chichewa; 1998-literacy in English only; 2008 and 2018-literacy is in any language including English and/or Chichewa or other language.
- Tanzania: 2002 and 2012-can read and write in English, KiSwahili, or any language; 1988-person can read and write in KiSwahili
- Uganda, Zambia: All years: can a person read and write in any language?

[37] "In each EA of all regions a one in five (20 Percent) households are systematically selected for the administration of the long questionnaire." `https://international.ipums.org/international-action/variables/ET2007A_0032#questionnaire_text_section`

B.2 Labor Market Variables

- Is the person in the labor force? This includes workers, and those not currently working but looking for a job. Those not in the labor force are still in school, are doing housework, are pensioners, or are otherwise unable to work. On average across countries, about 80% of the sample of adults is in the labor force: this is 85% for men and about 73% for women. These rates of LFP are substantially higher than LFPR of men and women in many rich countries.

- Is the worker working for pay, profit, or family gain at all? This share of the labor force who reports working is 95% for both men and women. In all countries, men and women work at the same rates/are unemployed at the same rates, except for Malawi where female employment is somewhat higher than male employment, and in Zambia where that pattern is reversed.

- Among workers, which workers work for a wage? This is roughly capturing formal sector work. This variable does not exist for Kenya. At most, about one-third of workers work for a wage, and wage work tends to be concentrated among men relative to women in all countries.

- Among workers, which workers are self-employed? Unpaid family workers are not part of this category. This varies a great deal across countries, with some countries having a much larger share of men in self-employment and others having many more women in self-employment.

- For those workers who report being employed, but who do not work for a wage or are self-employed, the vast majority are unpaid family workers.

- Sector of work: using the occupation codes (ISCO codes), I generate a series of sector categories including: does the worker work in the agricultural sector (or not); if they work in non-agriculture, is this work in manufacturing, or in services; if the work is in services, is this high-skilled work or not.[38] There is some missingness in this variable; only those actively employed reported an occupation. [39]

[38] Kenya and Ethiopia are missing occupation data for at least one wave of Census data.

[39] The generic question is : "If active or unemployed but worked before, state: [Questions N and O are asked of persons who have ever worked] N: Occupation O. Industry.

B.3 Defining Treatment and Control Groups

Each country implements FPE in different years. The definition of eligible and ineligible cohorts (for the strict treatment) and partially treated cohorts (for the intensity measure) therefore varies across countries. The tables below describe how the full and partially treated age groups, and untreated age groups, are defined for the relevant treatment wave in each Census. Each table also shows the birth year cohorts of the full analyses samples in both treated and control waves.

Table B1: **Defining Treatment Groups for FPE Exposure: Malawi**

Year of birth	Age at FPE Initiation [1994]	Age in Pre-Wave [1987]	Age in Post-Wave [2018]	Treatment Group	Intensity
1939	55	48	.	Untreated	0
.	.	.	.	Untreated	0
1967	27	20	.	Untreated	0
.	.	.	.	.	.
1970	24	.	48	Untreated	0
.	.	.	.	.	.
1981	13	.	37	Untreated	0
1982	12	.	36	Partial	0.25
1983	11	.	35	Partial	0.375
1984	10	.	34	Partial	0.5
1985	9	.	33	Partial	0.625
1986	8	.	32	Partial	0.75
1987	7	.	31	Partial	0.875
1988	6	.	30	Full	1
1989	5	.	29	Full	1
.	.	.	.	Full	1
1997	-3	.	21	Full	1
1998	-4	.	20	Full	1

Table notes: Malawi sample is aged 20 to 48 inclusive. Treated cohorts are age 20-30; partially treated are 31-36; 37 to 48 are untreated. First column shows birth cohort. The green highlighted area shows age of fully treated individuals, in the Census wave they are observed in. The yellow highlighted area shows age of partially treated individuals in the Census wave they are observed in, alongside their partial treatment intensity (see text for details). The orange highlighted area shows the age of untreated individuals in the Census wave in which they are observed. The red highlighted area shows the comparable-aged individuals in the Census wave before the FPE-eligible become adults. Their treatment values correspond to the age-specific (not birth year specific) treatment values defined in the Treatment Group column.

Table B2: **Defining Treatment Groups for FPE Exposure: Uganda**

Year of birth	Age at FPE Initiation [1997]	Age in Pre-Wave [1991]	Age in Post-Wave [2014]	Treatment Group	Intensity
1949	48	42			
.	.	.			
1971	26	20			
1972	25		42	Untreated	0
.	.		.	Untreated	0
1985	12		29	Untreated	0
1986	11		28	Partial	0.17
1987	10		27	Partial	0.33
1988	9		26	Partial	0.50
1989	8		25	Partial	0.67
1990	7		24	Partial	0.83
1991	6		23	Full	1
1992	5		22	Full	1
1993	4		21	Full	1
1994	3		20	Full	1

Table notes: Uganda sample is aged 20 to 42 inclusive. Treated cohorts are age 20-23; partially treated are 24-28; 29 to 42 are untreated. First column shows birth cohort. The green highlighted area shows age of fully treated individuals, in the Census wave they are observed in. The yellow highlighted area shows age of partially treated individuals in the Census wave they are observed in, alongside their partial treatment intensity (see text for details). The orange highlighted area shows the age of untreated individuals in the Census wave in which they are observed. The red highlighted area shows the comparable-aged individuals in the Census wave before the FPE-eligible become adults. Their treatment values correspond to the age-specific (not birth year specific) treatment values defined in the Treatment Group column.

Table B3: **Defining Treatment Groups for FPE Exposure: Ethiopia**

Year of birth	Age at FPE Initiation [1994]	Age in Pre-Wave [1985]	Age in Post-Wave [2007]	Treatment Group	Intensity
1943	52	41			
.	.	.			
1964	31	20			
1965	30	19			
1966	29		41	Untreated	0
1967	28		.	Untreated	0
.	.		.	.	
1979	17		28	Untreated	0
1980	16		27	Partial	0.10
1981	.		.	.	
1982	13		25	Partial	0.40
1983	12		24	Partial	0.50
1984	11		23	Partial	0.60
1985	10		22	Partial	0.70
1986	9		21	Partial	0.80
1987	8		20	Partial	0.90
1988	7		19	Full	1.00

Table notes: Ethiopia sample is aged 19 to 41 inclusive. Treated cohorts are age 19; partially treated are 20-23; 24 to 41 are untreated. First column shows birth cohort. The green highlighted area shows age of fully treated individuals, in the Census wave they are observed in. The yellow highlighted area shows age of partially treated individuals in the Census wave they are observed in, alongside their partial treatment intensity (see text for details). The orange highlighted area shows the age of untreated individuals in the Census wave in which they are observed. The red highlighted area shows the comparable-aged individuals in the Census wave before the FPE-eligible become adults. Their treatment values correspond to the age-specific (not birth year specific) treatment values defined in the Treatment Group column.

Table B4: **Defining Treatment Groups for FPE Exposure: Ghana**

Year of birth	Age at FPE Initiation [1961]	Age at FPE Termination [1965]	Age in Pre-Wave [2010]	Age in Post-Wave [1984]	Treatment Group	Intensity
1936	25	29		48	Untreated	0
.	.	.		.	.	0
1948	13	17		36	.	.
1949	12	16		35	Untreated	0
1950	11	15		34	Untreated	0.17
1951	10	14		33	Partial	0.33
1952	9	13		32	Partial	0.5
1953	8	12		31	Partial	0.5
1954	7	11		30	Full	1
1955	6	10		29	Full	1
.						
1962			48			
			.			
1970			40			
.			.			
1980			30			
1981			29			

Table notes: Ghana: treated ages are 29 to 48 inclusive. Treated cohorts are 29 and 30, partially treated are 31-34, and control cohorts are 35 to 48. First column shows birth cohort. The green highlighted area shows age of fully treated individuals, in the Census wave they are observed in. The yellow highlighted area shows age of partially treated individuals in the Census wave they are observed in, alongside their partial treatment intensity (see text for details). The orange highlighted area shows the age of untreated individuals in the Census wave in which they are observed. The red highlighted area shows the comparable-aged individuals in the Census wave before the FPE-eligible become adults. Their treatment values correspond to the age-specific (not birth year specific) treatment values defined in the Treatment Group column.

Table B5: **Defining Treatment Groups for FPE Exposure: Kenya**

Year of birth	Age at FPE Initiation [1974]	Age at FPE Termination [1988]	Age in Pre-Wave [2019]	Age in Post-Wave [1989]	Treatment Group	Intensity
1953		.		36	Untreated	0
.	.	.		.	.	.
1963	11	25		26	Untreated	0.00
1964	10	24		25	Partial	0.17
1965	9	23		24	Partial	0.33
1966	8	22		23	Partial	0.50
1967	7	21		22	Partial	0.67
1968	6	20		21	Full	1
1969	5	19		20	Full	1
1970	4	18		19	Full	1
.						
1983			36			
.			.			
1990			29			
.			.			
2000			19			

Table notes: Kenya: Overall sample is aged 19-36. Treated ages are 19 to 21 in treated wave (1989); controls are 22-36. First column shows birth cohort. The green highlighted area shows age of fully treated individuals, in the Census wave they are observed in. The yellow highlighted area shows age of partially treated individuals in the Census wave they are observed in, alongside their partial treatment intensity (see text for details). The orange highlighted area shows the age of untreated individuals in the Census wave in which they are observed. The red highlighted area shows the comparable-aged individuals in the Census wave before the FPE-eligible become adults. Their treatment values correspond to the age-specific (not birth year specific) treatment values defined in the Treatment Group column.

Table B6: **Defining Treatment Groups for FPE Exposure: Tanzania**

Year of birth	Age at FPE Initiation [1978]	Age at FPE Termination [1978]	Age in Pre-Wave [2012]	Age in Post-Wave [1988]	Treatment Group	Intensity
1947				41	Untreated	0
1948	.	.		40	Untreated	0
.	.	.		.	.	0
1960	14	18		28	.	.
1961	13	17		27	Untreated	0
1962	12	16		26	Partial	0.2
1963	11	15		25	Partial	0.4
1964	10	14		24	Partial	0.6
1965	9	13		23	Partial	0.8
1966	8	12		22	Full	1
1967	7	11		21	Full	1
.						
1971			41			
.			.			
1980			32			
.			.			
1990			22			
1991			21			

Table notes: Tanzania: sample is aged 21-40. Treated ages are 21-26; Controls are 27-40. First column shows birth cohort. The green highlighted area shows age of fully treated individuals, in the Census wave they are observed in. The yellow highlighted area shows age of partially treated individuals in the Census wave they are observed in, alongside their partial treatment intensity (see text for details). The orange highlighted area shows the age of untreated individuals in the Census wave in which they are observed. The red highlighted area shows the comparable-aged individuals in the Census wave before the FPE-eligible become adults. Their treatment values correspond to the age-specific (not birth year specific) treatment values defined in the Treatment Group column.

Table B7: **Defining Treatment Groups for FPE Exposure: Zambia**

Year of birth	Age at FPE Initiation [1965]	Age at FPE Termination [1980]	Age in Pre-Wave [2010]	Age in Post-Wave [1990]	Treatment Group	Intensity
1954	11	26		36	Partial	0.29
1955	10	25		35	Partial	0.43
1956	9	24		34	Partial	0.57
1957	8	23		33	Partial	0.71
1958	7	22		32	Partial	0.86
1959	6	21		31	Full	1
.	.	.			Full	1
1967	-2	13		23	Full	1
.						
1974	-9	6	36			
.			.			
1986	-21	-6	24			
1987	-22	-7	23			

Table notes: Zambia: Sample is aged 23-36. Treated ages are 23-32 in the treated wave, controls are 33-36. The 37 and older are partially treated in the control wave because of the later FPE. First column shows birth cohort. The green highlighted area shows age of fully treated individuals, in the Census wave they are observed in. The yellow highlighted area shows age of partially treated individuals in the Census wave they are observed in, alongside their partial treatment intensity (see text for details). The orange highlighted area shows the age of untreated individuals in the Census wave in which they are observed. The red highlighted area shows the comparable-aged individuals in the Census wave before the FPE-eligible become adults. Their treatment values correspond to the age-specific (not birth year specific) treatment values defined in the Treatment Group column.

C Online DHS Results Appendix - Not for Publication

Census data use self-reported information on literacy. I cross-check my results for human capital accumulation using independently collected Demographic Health Surveys (DHS). The DHS collects years of education and assesses literacy by asking respondents to read a simple sentence written on a cue card in their local language. I select two waves of DHS data for each country that are closest in time to the Census data for that country, and implement the same analysis for women ages 20 to 49 inclusive. Summary statistics are shown in Table C1 and Appendix Tables C2 to C6 present the results.

The impacts of FPE exposure on female human capital measured in the DHS mirror the results using Census data: all countries exhibit positive impacts of FPE on years of schooling and primary school completion using both definitions of treatment (in Zambia, the point estimate is positive but not significant at conventional levels). All countries with literacy outcomes show positive impacts on literacy, including Uganda and Malawi, although the latter point estimate is not statistically significant. And entry into secondary schooling is significantly higher for females in all the countries that showed positive impacts in the Census analysis: Ghana, Zambia, Ethiopia, Malawi and Uganda. The magnitudes of the DHS data differ from Census results partly because the years of DHS do not perfectly line up with Census years.

To define whether a woman is in the labor force or not, a woman should report that they have done some work in the last week, or were absent from a job in the last week. They are defined as not currently in the labor market if they say they had a job in the last year, but not right now.[40]

Appendix Table C5 shows the difference-in-differences results for the outcome of currently employed. The results for Ghana, Kenya, and Tanzania (especially using *Intensity*) are similar to the Census results: in early FPE countries, women with greater FPE exposure are more likely to report being currently employed, or at least a null effect. Likewise, in Malawi and Uganda, late FPE exposure reduces current employment probabilities. Ethiopia

[40]These questions are asked of all women in the DHS, in this order: Aside from your own housework, have you done any work in the last 7 days? As you know, some women take up jobs for which they are paid in cash or kind. Others sell things, have a small business or work on the family farm or in the family business. In the last 7 days, have you done any of these things or any other work? Although you did not work in the last 7 days, do you have any job or business from which you were absent for leave, illness, vacation? Have you done any work in the last 12 months? What is your occupation? That is, what kind of work do you mainly do? Do you do this work for a member of your family, for someone else, or are you self-employed? Do you usually work throughout the year, or do you work seasonally, or only once in a while? Are you paid in cash or kind for this work or are you not paid at all? Sometimes, the question: Do you usually work at home or away from home? is also asked. I try to make this variable resemble the Census data as closely as possible.

shows a positive impact using the *Intensity* measure. This country has no comparison in the Census data. In Zambia, the DHS results differ from the Census results: instead of greater FPE exposure increasing employment probability as it does in the Census, the DHS results indicate a negative effect on employment.

The DHS results for marital status and fertility outcomes (ever had a child) also mirror the Census results (see Appendix Table C6).

Table C1: **Summary Statistics: Demographic Health Surveys**

	Ghana		**Kenya**		**Tanzania**		**Zambia**		**Ethiopia**		**Malawi**		**Uganda**	
	Ages 29-48		**Ages 19-36**		**Ages 21-38**		**Ages 23-36**		**Ages 22-48**		**Ages 20-48**		**Ages 20-48**	
	1988	**2008**	**1989**	**2022**	**1991**	**2010**	**1996**	**2013**	**2000**	**2011**	**1992**	**2015**	**1995**	**2013**
	Treated	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Treated**
Ever Married	0.992	0.964	0.814	0.715	0.902	0.896	0.926	0.899	0.996	0.923	0.966	0.931	0.944	0.894
Age at Marriage	17.987	19.608	17.530	19.857	17.830	18.500	17.776	18.454	15.758	16.442	17.461	17.981	17.210	18.483
Character Variables														
Years of schooling	3.812	5.729	5.745	10.208	4.279	5.580	5.999	6.882	0.817	2.191	2.433	5.846	3.639	6.544
Completed primary	0.373	0.537	0.518	0.879	0.491	0.660	0.516	0.571	0.035	0.123	0.193	0.535	0.220	0.460
Any secondary	0.057	0.490	0.264	0.645	0.048	0.117	0.284	0.418	0.050	0.099	0.044	0.257	0.128	0.332
Literacy	0.332	0.237	0.569	0.842	0.506	0.622	0.486	0.540	0.123	0.192	0.246	0.609	0.336	0.533
Labor Market Variables														
Currenlty Working	0.637	0.929	0.140	0.537	0.723	0.848	0.545	0.572	0.592	0.416	0.283	0.687	0.649	0.803
Work Exclude Housework,Agr	1.000	0.649	.	.	0.302	0.312	0.658	0.580	0.353	0.691	0.753	0.428	0.256	0.513
Work for Cash	.	.	.	.	0.610	0.489	0.862	0.676	0.369	0.801	0.913	0.410	0.523	0.783
Work in Agr	0.212	0.348	.	.	0.692	0.660	0.316	0.418	0.646	0.296	0.247	0.572	0.733	0.486

Table notes: Means of various outcome variables are provided for the main sample of females in selected age groups. There are no explitcit prefence periods for all Wave 1 while for wave 2 the preference period for is 7 days for currently working. Currently working in Ghana and Kenya in wave 1 only include those non-agr and non-household business workes. There's no direct occupation variable for Kenya in wave 1. Work types (non-arg, agr, cash) are conditional on being currently working.

Table C2: Effects of Early FPE on Female Human Capital Quantity: DHS

	Panle A: Strict Treatment							
	Ghana		**Kenya**		**Tanzania**		**Zambia**	
	YrsEdu	ComPrim	YrsEdu	ComPrim	YrsEdu	ComPrim	YrsEdu	ComPrim
Eligible*Treated Wave	2.169***	0.215**	1.193**	0.134***	0.831*	0.153*	0.717*	0.042
	(0.549)	(0.057)	(0.332)	(0.030)	(0.367)	(0.058)	(0.283)	(0.036)
Treated Wave	-2.188***	-0.188***	-4.674***	-0.387***	-1.541***	-0.209***	-1.307***	-0.084*
	(0.285)	(0.032)	(0.150)	(0.024)	(0.311)	(0.052)	(0.250)	(0.030)
N	4,138	4,138	23,470	23,470	10,278	10,278	10,396	10,396
Adj R-Squared	0.070	0.057	0.208	0.158	0.068	0.065	0.019	0.008
Control Wave's Mean	4.565	0.432	8.576	0.752	5.036	0.572	6.460	0.532
	Panel B: Intensity Treatment							
Intensity*Treated Wave	2.343***	0.237***	1.634***	0.212***	2.437***	0.421***	1.696***	0.148***
	(0.554)	(0.052)	(0.269)	(0.031)	(0.454)	(0.073)	(0.319)	(0.044)
Treated Wave	-2.812***	-0.252***	-4.979***	-0.430***	-2.357***	-0.349***	-2.225***	-0.175***
	(0.288)	(0.030)	(0.092)	(0.016)	(0.188)	(0.030)	(0.259)	(0.034)
N	4,138	4,138	23,470	23,470	10,278	10,278	10,396	10,396
Adj R-Squared	0.073	0.060	0.209	0.163	0.083	0.089	0.021	0.009
Control Wave's Mean	4.565	0.432	8.576	0.752	5.036	0.572	6.460	0.532

Table notes: Estimates are person weighted, include cohort FE, and SE are clustered on age. * for significance levels.

Table C3: Effects of Late FPE on Female Human Capital Quantity: DHS

	Panel A: Strict Treatment					
	Ethiopia		**Malawi**		**Uganda**	
	YrsEdu	ComPrim	YrsEdu	ComPrim	YrsEdu	ComPrim
Eligible*Treated Wave	1.932***	0.181***	0.846***	0.094***	0.848***	0.137***
	(0.301)	(0.038)	(0.216)	(0.025)	(0.215)	(0.024)
Treated Wave	0.868***	0.048***	2.978***	0.294***	2.695***	0.204***
	(0.128)	(0.009)	(0.177)	(0.018)	(0.188)	(0.019)
N	21,456	21,456	22,660	22,660	19,316	19,316
Adj R-Squared	0.080	0.058	0.171	0.124	0.164	0.099
Control Wave's Mean	2.268	0.139	5.584	0.515	5.666	0.389
	Panel B: Intensity Treatment					
Intensity*Treated Wave	0.929*	0.090*	1.211***	0.138***	1.382***	0.184***
	(0.545)	(0.049)	(0.222)	(0.022)	(0.204)	(0.022)
Treated Wave	0.745***	0.035***	2.666***	0.258***	2.332***	0.164***
	(0.142)	(0.012)	(0.176)	(0.015)	(0.159)	(0.016)
N	21,456	21,456	22,660	22,660	19,316	19,316
Adj R-Squared	0.075	0.052	0.172	0.125	0.166	0.101
Control Wave's Mean	2.268	0.139	5.584	0.515	5.666	0.389

Table notes: Estimates are person weighted, include cohort FE, and SE are clustered on age. * for significance levels.

Table C4: Effects of Early and Late FPE on Literacy: Strict Treatment - DHS

	Panel A: Literacy						
	Ghana	**Kenya**	**Tanzania**	**Zambia**	**Ethiopia**	**Malawi**	**Uganda**
Eligible*Treated Wave	0.200***	0.146***	0.123**	0.078	0.174***	0.026	0.105***
	(0.041)	(0.028)	(0.042)	(0.043)	(0.022)	(0.024)	(0.019)
Treated Wave	0.077*	-0.300***	-0.146**	-0.100*	0.019	0.343***	0.169***
	(0.031)	(0.025)	(0.039)	(0.037)	(0.014)	(0.020)	(0.015)
N	4,138	23,470	10,278	10,396	21,456	22,660	19,316
Adj R-Squared	0.035	0.098	0.035	0.007	0.046	0.112	0.058
Control Wave's Mean	0.266	0.722	0.565	0.509	0.221	0.570	0.474
	Panel B: Any Secondary Schooling						
Eligible*Treated Wave	0.053	-0.141***	-0.035	0.093**	0.122***	0.118***	0.122***
	(0.048)	(0.011)	(0.028)	(0.033)	(0.020)	(0.024)	(0.019)
Treated Wave	-0.438***	-0.361***	-0.068***	-0.190***	0.011	0.160***	0.171***
	(0.015)	(0.009)	(0.012)	(0.029)	(0.009)	(0.017)	(0.016)
N	4,138	23,470	10,278	10,396	21,456	22,660	19,316
Adj R-Squared	0.230	0.144	0.024	0.028	0.041	0.077	0.088
Control Wave's Mean	0.265	0.515	0.121	0.363	0.135	0.242	0.268

Table notes: Estimates are person weighted, include cohort FE, and SE are clustered on age. * for significance levels.

Table C5: Effects of FPE Exposure on Female's Probability of Currently Working: DHS

	Outcome: Currently Working						
	Panel A: Strict Treatment						
	Ghana	**Kenya**	**Tanzania**	**Zambia**	**Ethiopia**	**Malawi**	**Uganda**
Eligible*Treated Wave	0.113***	0.291***	0.001	-0.019	0.039	-0.067*	-0.061*
	(0.024)	(0.023)	(0.019)	(0.015)	(0.021)	(0.028)	(0.027)
Treated Wave	-0.309***	-0.441***	-0.121***	-0.009	-0.178***	0.433***	0.171***
	(0.018)	(0.023)	(0.017)	(0.012)	(0.011)	(0.010)	(0.012)
N	4,123	23,424	10,265	10,369	21,437	22,658	19,314
Adj R-Squared	0.135	0.168	0.029	0.012	0.032	0.112	0.042
Control Wave's Mean	0.796	0.430	0.768	0.588	0.491	0.622	0.771
	Panel B: Intensity Treatment						
Intensity*Treated Wave	0.022	0.323***	0.055*	-0.060***	0.044	-0.076***	-0.049*
	(0.052)	(0.023)	(0.030)	(0.017)	(0.026)	(0.027)	(0.028)
Treated Wave	-0.305***	-0.492***	-0.142***	0.027**	-0.189***	0.447***	0.174***
	(0.019)	(0.016)	(0.017)	(0.013)	(0.016)	(0.011)	(0.015)
N	4,123	23,424	10,265	10,369	21,437	22,658	19,314
Adj R-Squared	0.133	0.171	0.029	0.013	0.032	0.113	0.042
Control Wave's Mean	0.796	0.430	0.768	0.588	0.491	0.622	0.771

Table notes: Estimates are person weighted, include cohort FE, and SE are clustered on age. * for significance levels.

Table C6: Effects of Early and Late FPE on Female Marriage Probability and Fertility: Strict Treatment - DHS

	Panel A: Ever Married						
	Ghana	**Kenya**	**Tanzania**	**Zambia**	**Ethiopia**	**Malawi**	**Uganda**
Eligible*Treated Wave	0.009	0.166***	-0.018	-0.034	-0.016**	-0.059***	-0.103***
	(0.007)	(0.026)	(0.015)	(0.021)	(0.005)	(0.013)	(0.018)
Treated Wave	0.027***	0.079***	0.027*	0.051*	-0.008*	-0.008**	-0.018***
	(0.006)	(0.016)	(0.011)	(0.020)	(0.003)	(0.003)	(0.005)
N	4,138	23,470	10,278	10,396	21,456	22,660	19,316
Adj R-Squared	0.021	0.255	0.071	0.043	0.097	0.103	0.112
Control Wave's Mean	0.978	0.757	0.893	0.908	0.911	0.931	0.906
	Panel B: Has Had Any Children						
Eligible*Treated Wave	0.030***	0.169***	0.035	-0.002	-0.035***	-0.016	-0.095***
	(0.010)	(0.043)	(0.021)	(0.009)	(0.011)	(0.012)	(0.025)
Treated Wave	0.029***	0.083***	0.018*	0.002	-0.002	0.012***	0.009**
	(0.008)	(0.022)	(0.009)	(0.008)	(0.004)	(0.004)	(0.004)
N	4,138	23,470	10,278	10,396	21,456	22,660	19,316
Adj R-Squared	0.017	0.275	0.068	0.028	0.124	0.126	0.123
Control Wave's Mean	0.965	0.804	0.897	0.936	0.865	0.923	0.907

Table notes: Estimates are person weighted, include cohort FE, and SE are clustered on age. * for significance levels.

D Online HIV/AIDS Robustness Appendix - Not for Publication

From the early 1990s through the 2000s, the HIV/AIDS epidemic raised adult and infant mortality and morbidity across Africa. Researchers have posited that the epidemic reduced educational attainment of adults on the continent through two key channels: (1) by reducing the attainment of schooling in childhood, as families responded to heightened (and generalized) mortality risk and falling earnings by lowering human capital investments; and (2) by changing the composition of working age adults through selective mortality.

This appendix discusses how these documented HIV-induced changes in human capital and working age populations affect the interpretation of my key results on female education and female labor force participation. I show that even after accounting for any HIV/AIDS-related impacts on human capital accumulation and on selective mortality, the impacts of FPE on schooling are still strongly and significantly positive in all countries except in Zambia. The impacts on female LFP and employment outcomes are robust to including district fixed effects and district trends that account for differences in local HIV prevalence rates, although precision falls in the late FPE countries. Cohort sizes are not significantly difference in the difference-in-differences specification.

D.1 Background on HIV/AIDS in Africa

The HIV/AIDS epidemic was a major public health crisis that significantly impacted schooling, health, and labor markets in different African countries and to different degrees in the 1990s and 2000s. UNICEF estimated that Africa would have about 45 million AIDS' orphans (who had lost one or both parents to AIDS) by 2010 (The World Bank, 2009). Although the actual number of AIDS' orphans in 2010 was considerably lower than this, at about 12 million[41], the epidemic had vast impacts on health, earnings, savings, and investment decisions of millions of households. With the advent of PEPFAR funding from the mid-2000s, access to affordable anti-retrovirals increased dramatically, contributing to declining mortality rates after that time.

D.2 Impacts of HIV on schooling: Estimates from the literature

I look to the literature to find (where possible) country-specific estimates of the impact of the HIV/AIDS epidemic on overall schooling attainment and on labor market outcomes.

[41]Data are from UNAIDS in 2024. Cited by UNICEF 2024 in `https://data.unicef.org/topic/hivaids/orphanhood/`

Fortson (2011) provides the first key estimates of the aggregate impact of the HIV/AIDS epidemic on educational attainment in sub-Saharan Africa, using DHS data to show that areas with higher levels of HIV prevalence experienced relatively larger declines in schooling. Chicoine (2019) corroborates her results, finding somewhat smaller negative effects for slightly older cohorts.

Fortson's paper argues that at least part of this negative impact derives from families adjusting to the increased mortality risk (and hence lower lifetime returns to schooling) associated with higher HIV prevalence. Writing down a calibrated model in which mortality risk reduces the incentives to invest in education, she shows that a 10% increase in HIV prevalence accounts for about a one half-year (0.5 years) decline in schooling. Using pooled cross-sectional DHS data from 15 countries and a difference-in-differences research design, she estimates an empirical relationship between local HIV prevalence and schooling attainment of similar magnitude. Her research design compares younger cohorts born after 1980 (HIV-exposed) to older cohorts (unexposed) in districts with higher versus lower HIV prevalence, controlling for year of birth fixed effects and country-district fixed effects. The year of birth fixed effects allow education to be changing flexibly across cohorts, and her identification comes from departures from these cohort trends in high HIV prevalence regions. A key assumption that divides her sample into HIV-exposed and unexposed cohorts is that HIV had no effect on schooling for those born before 1980 (the unexposed cohorts).

The main schooling results relevant for my paper are that when within-district HIV prevalence increases by 10% (from a baseline of zero), completed schooling declines by 0.5 years and the probability of completing primary school declines by about 8 percentage points. These results are robust to focusing on non-orphans and on children living in households without any exposure to HIV positive adults (who may require caretaking), which suggests that HIV affects schooling in a broad way, not just for those in households with an affected member.

Using data for the same countries over a longer period of time (covering more cohorts), Chicoine (2019) find an almost identical negative relationship between HIV prevalence and schooling. When data from an additional 15 countries are added, the results from the expanded cohort sample show a smaller significant but still negative impact on education: for a 10% increase in prevalence, schooling declines by 0.3 years. These negative effects become statistically significant only for women born in 1987 or later.

Two other findings from Fortson (2011)'s paper are relevant for interpreting my results. First, Fortson finds negative impacts against a backdrop of *rising* education in most low-HIV prevalence countries, suggesting that HIV *attenuated* but did not reverse overall improve-

ments in education in these areas.[42]. Second, Fortson finds that the negative impacts of HIV prevalence on schooling are larger for men than for women. Taken together, these two results suggest that my FPE impacts may be downwards-biased, and smaller for women than men.

D.3 Impacts of HIV on labor market outcomes: Estimates from the literature

In contrast to schooling outcomes, there are few estimates of how the HIV epidemic affected labor markets in SSA.[43] For South Africa, where HIV prevalence was particularly high, Chicoine (2012) shows that group-specific AIDS mortality rates reduced wages by 3-6% and led to small reductions in employment for the least educated. This is consistent with negative spillovers from the epidemic, rather than a reduction in labor supply translating into higher wages and lower unemployment (which is the channel of impact suggested in Young (2005)).

Relevant for thinking about the composition of the adult workforce is Fortson (2008)'s result (again using DHS data but now only for Ghana, Tanzania, and Kenya) that for all but the most-educated respondents (e.g. college-level), higher HIV prevalence is correlated with higher schooling, even after conditioning on age, sector, and rural/urban status. The key result is that *better-educated individuals are more likely to be HIV-positive*. Even at 6 years of schooling, adults over 3 percentage points more likely to be infected with HIV than adults with no schooling. The channel Fortson emphasizes to explain these results is that education increases certain risk behaviors, such as premarital sex.

While the correlation between education and HIV prevalence has certainly evolved over-time, others in the literature have found evidence that more education does not lower the chances of STI exposure e.g. Duflo et al. (2015), and that there is no significant correlation between years of schooling and HIV positive status (in Tanzania, Kenya, and Ghana) de Walque (2009).

HIV-related selective mortality of the kind described in Fortson should have shifted the composition of the adult working-age population towards less educated adults. Survivor bias therefore leans towards finding smaller impacts of FPE on schooling in later waves of Census data. If those with more schooling are more likely to be working, this survivor bias could contribute to lower female LFP impacts, although it is still unclear what the GE impacts of this change in the labor market might be. However, if mortality was not selective along education lines, but rather affected all adults in specific age groups, it would be important

[42]In Rwanda, Kenya, and Zambia, education levels are falling over time, and in Tanzania and Lesotho, education levels are completely flat In the other countries, educational attainment is rising over time

[43]In a related paper, Fortson (2009) finds very little fertility impacts of the HIV epidemic in 12 African countries.

to know whether changing cohort size could affect the LFP results.

D.4 When and where was peak HIV/AIDS prevalence and mortality?

To provide informative bounds on how the HIV epidemic might affect the measured impacts of FPE exposure on female schooling and LFP in each of the countries in my analysis, it is useful to determine when, where, and which age groups in each country experienced peak HIV prevalence and mortality.

For the purposes of *schooling decisions*, Fortson defines HIV-exposed cohorts as those born between 1980 and 1991 and unexposed cohorts as those born between 1952 and 1979.[44] Chicoine extends the definition of exposed cohorts to include those born 1980-2001 and suggests that those most exposed would be born in the 1987-2001 range.

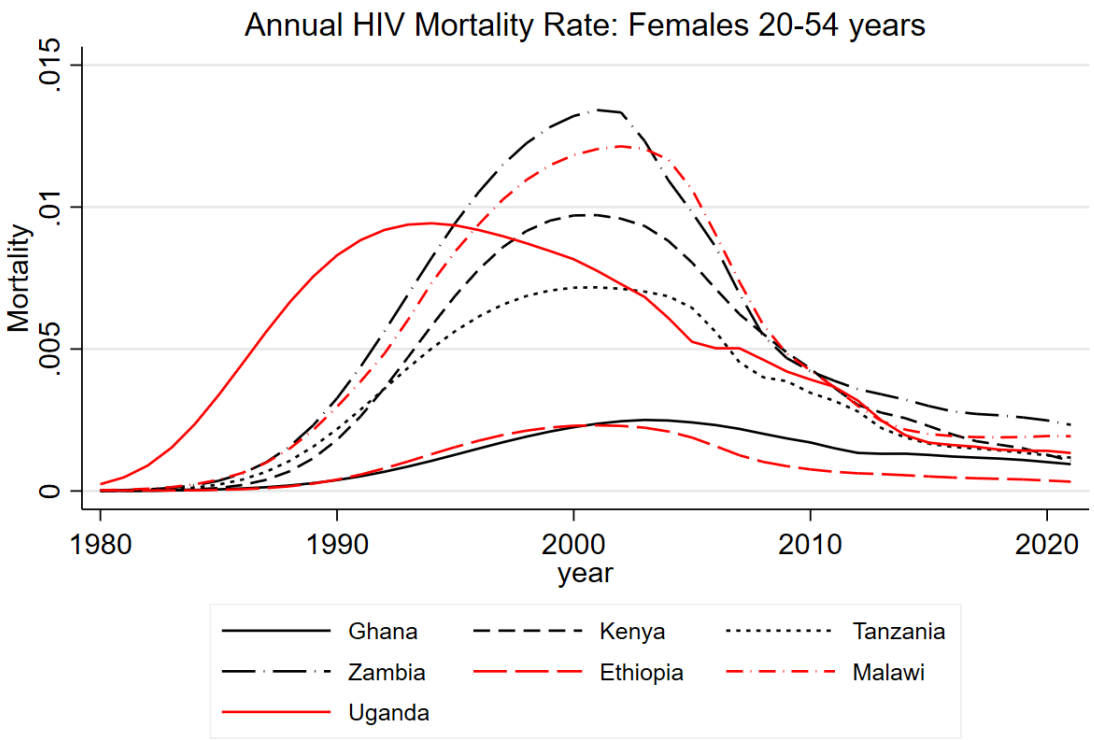
For the purposes of *selective mortality*, HIV-related mortality rose steadily from the early 1990s and peaked around the mid-2000s in most of the continent. I used data from the IMHE database (Institute for Health Metrics and Evaluation, 2024) to plot mortality rates from HIV for women aged 20-54 inclusive, in each year between 1980 and 2021. I plot age-specific mortality for each country separately in Figure D1.

The graph shows considerable variation *across* countries, as well as variation over time. Peak HIV mortality in Uganda was in the early 1990s while in other countries, the peak was more in the mid-2000s (with Ghana a bit later). As reflected in the above notes about HIV prevalence by country, Ghana and Ethiopia had relatively low mortality rates, Tanzania, Kenya and Uganda are in the medium range (at their peak), while Malawi and Zambia have the highest mortality rates. Note that there is substantial variation in high/low mortality rates across the Early and Late FPE country waves. Mortality in Zambia was the highest at 1.4% in the mid-2000s.

These figures echo the country-specific variation in HIV prevalence documented in the literature: Fortson (2011) documents high HIV prevalence in Malawi (11.6%) and Zambia (15.3%), low HIV prevalence in Ghana (2.13%) and Ethiopia (under 1.42%) and medium HIV prevalence in Kenya (6.74%) and Tanzania (6.85%). Estimates for Uganda in the late 1990s are around 15% Bloom and Canning (2006).

[44] The end points of these definitions are anchored by the dates of the DHS surveys that she uses, with the latest survey coming from 2006.

Figure D1: Annual HIV Mortality Rate by Country



Source: IMHE (2024)

D.5 Bounding the impacts of FPE exposure on schooling using country-specific HIV impacts

In my analysis, cohorts who are born in 1980 or afterwards – i.e. those most exposed to the impacts of HIV on schooling decisions – show up differentially in the treatment or control group or wave:

- Ghana: The sample is aged 29-48 and treated/control Census years are 1984/2010. Those born in 1980 or beyond show up for only 2 years of the control group in the control wave. None in the treated wave are exposed. If we take 1987 as the beginning of exposure to the epidemic (Chicoine et al., 2021), then no-one in the analysis sample is exposed to the epidemic.

- Kenya: The sample is aged 19-36 and treated/control Census years are 1989/2019. Those born in 1980 or beyond include everyone in the control group in the control wave. If we instead take those born in 1987 or later as most HIV exposed, this group would only show up as the 32-36 years olds in the control wave.

- Zambia: The sample is aged 23-36 and the treated/control years are 1990/2010. Those born in 1980 or beyond show up for most of the control group in the control wave. If we take 1987 as the beginning of exposure to the epidemic, then only one year of the control group (age 23) in the control wave is exposed to the epidemic.

- Tanzania: The sample is aged 21-40 and the treated/control years are 1988/2012. Those born in 1980 or beyond show up for most of the control group in the control wave. If we take 1987 as the beginning of exposure to the epidemic, then those aged 21-25 in the control wave would be exposed.

- Ethiopia: The sample is aged 18-48 and the treated/control years are 1984/2007. Those born in 1980 or beyond show up as all of the treated age group in the treated wave. If we take 1987 as the beginning of exposure to the epidemic, then those aged 18-20, i.e. the youngest of the treated ages in the treated wave, would be most exposed to the epidemic.

- Uganda: The sample is aged 20-48 and the treated/control years are 1991/2014. Those born in 1980 or beyond show up as most of the treated group in the treated wave, and a little of the untreated group. If we take 1987 as the beginning of exposure to the epidemic, then those aged 20-27, i.e. the younger end of the treated ages in the treated wave, would be most exposed to the epidemic.

- Malawi: The sample is aged 20-48 and the treated/control years are 1987/2018. Those born in 1980 or beyond show up as most of the treated group in the treated wave. If we take 1987 as the beginning of exposure to the epidemic, then those aged 20-31, i.e. would still be most of the treated ages in the treated wave most exposed to the epidemic.

For the early FPE countries, the HIV epidemic affects some portion of the control group in the control waves. By reducing educational attainment within the control group in the control waves, HIV could contribute/explain some of the schooling effects that I estimate are the result of the FPE policies. The question is whether the negative impacts of the HIV epidemic among the control group in the control waves are large enough to account for all of the schooling effect that I estimate? The answer is no.

Consider Fortson's estimate of a 0.5 year decline in schooling for each 10% increase in prevalence. In each of Ghana, Kenya, Tanzania, and Zambia, the control groups in the control wave will have about 0.5 years less schooling. This decline should be weighted by HIV prevalence in each country, and by the share of control group exposed. Therefore in Zambia the entire decline would have been felt, while in Ghana, the control group would have lost 0.25 years of education in a small portion of the control cohort, while in Kenya and Tanzania, the control group would have lost about 0.3 years of education (60 percent of the effect size*0.5). If we "remove" the impact of HIV and add back these lost years of schooling, the FPE impacts would shrink.

The point estimates for the strict definition of FPE in Table 2 suggest that the removing the effects of HIV would reduce schooling gains: In Ghana, schooling impacts would fall to about 0.94 years, in Kenya to 0.68 years, and in Tanzania, to about 0.98 years. These are still large positive impacts on human capital. In Zambia, the HIV impact could account for all of the FPE education gains, unless HIV exposure only begins for cohorts born after 1987 as Chicoine et al. (2021) suggests.

What about for late FPE countries? Because of the different timing of the FPE policies relative to 1980, the HIV epidemic affects some portion of the (once children now adults in the) treatment group in the treatment waves, reducing their schooling attainment by about 0.5 years for each 10% increase in HIV prevalence. If we added back the lost education from the impact of HIV, this would expand the positive gains to FPE even more: in Ethiopia, the schooling impact would rise by 0.5*20 percent or 0.1 years, in Uganda by 0.5*15 percent or 0.75 years, and in Malawi by 0.5 years. The positive impacts on schooling as a result of FPE would therefore be 0.39 years in Ethiopia, 1.38 years in Malawi, and 1.18 years in Uganda.

D.6 Bounding the impacts of FPE exposure on LFP using country-specific HIV impacts

To what extent did HIV mortality change the cohort size and composition of working-age adults in the treated and control waves?

First, I consider the implications of Fortson (2008)'s finding of higher prevalence among more educated cohorts. This would translate into a lower share of surviving women with less education in the labor market. This change in the composition of the working age population in some waves (after the early 1990s) more than others, and in some cohorts more than others (for those born earlier than the mid-1980s), may have contributed to the difference in labor market results that I estimate.

Taking the early FPE countries first and considering the mortality rates in the figure above:

- Ghana: peak HIV mortality for women of working age was 0.25 percentage points in 2003. The control wave would have been most affected. Adding this share of missing (educated, working) women back in would slightly dampen the positive LFP effect.
- Kenya: peak HIV mortality for women of working age was 0.97 percentage points in 2003. By 2019, the mortality rate was 0.1 percentage points. If we added back these missing (educated, working) women into the control group, this would hardly change the positive female LFP impact.
- Tanzania: peak HIV mortality for women of working age was 0.72 percentage points in 2001. If we added back these missing (educated, working) women into the control group, this would slightly dampen the positive LFP effect.
- Zambia: peak HIV mortality for women of working age was 1.34 percentage points in 2001. If we added back these missing (educated, working) women into the control group, this would shrink but not eliminate the positive female LFP impact.

Therefore, the positive impacts of FPE on female LFPR (and on employment) in Ghana, Kenya, Tanzania, and Zambia are still present even if we added back into the control group those women missing due to HIV mortality. Note that this is the case whether we assume these women were in the labor force (in which case our estimates would be slightly lower) or not (in which case estimates would be slightly higher. This is despite significant variation in HIV mortality across countries.

What about the late FPE countries? DiD estimates could be downwards-biased if treated ages in treated waves were most affected by selective mortality. This could contribute to the

negative impacts that I estimate. (But also recall that female mortality was lower than male mortality, and we do not see male LFP falling in the late FPE countries).

The DiD estimates of female LFP show a 1.4 percentage point decline in Malawi and a 1.6 percentage point decline in Uganda. Consider Malawi: the effects of selective mortality would have been largest in the treated wave of 2018, The highest rate of mortality from HIV for women in Malawi was in the mid-2000s and around 1.2 percentage points. If we assume that this mortality rate was highest among those women with highest education and high LFP, then adding them back to the DiD estimates would reduce the negative LFP result to about -0.2 percentage points. This is still a negative number, although less than before.

Uganda's epidemic was so much earlier than the other countries and both Census waves likely have some cohorts impacted by HIV mortality. The control wave of 1991 is likely to have shown the largest impact of selective mortality on composition of the working-age population, while the later treated wave would have seen smaller impacts. Applying the peak mortality rate (0.8 percentage points) to the control wave in 1991 would shrink the negative LFP result by about half, but would still not eliminate it.

In both of these late FPE countries, it is plausible that at least some of the negative female LFPR result is driven by the effects of selective HIV related mortality; however, it is clear from these estimates that there is still a negative relationship between FPE exposure and female LFP even after 'adding back' the missing women.

D.7 Robustness of LFP estimates to including spatial controls

Spatial variation in exposure to the HIV epidemic is a key source of variation used in Fortson (2011) and Chicoine et al. (2021). Within each country, some areas had higher levels of HIV prevalence (and mortality) in the 1990s and early 2000s, and others had lower levels. Both authors, as well as other researchers, have exploited this spatial variation in HIV prevalence rates to estimate the effect of HIV exposure on different outcomes. To leverage this spatial variation to test how the HIV epidemic may interfere with the labor market impacts of FPE exposure, I re-estimate the main LFP results including district fixed effects and the interaction of these fixed effects with a post indicator. As an alternative specification, I also include district fixed effects interacted with age dummies, since HIV prevalence had very strong age patterns in the 1990s and early 2000s. These are two different and general ways of controlling for high versus low HIV prevalence across space and across time, and any other local labor market shocks that could affect female LFP rates in a persistent way.

Table D7 and Table D8 present the results for the first set of controls. After including district-specific fixed effects and interactions with a post indicator the female LFP and

Table D7: Effects of Early and Late FPE Exposure on Female's LFP:Robustness

	Outcome: LFP					
	Panel A: Strict Treatment					
	Ghana	**Kenya**	**Tanzania**	**Zambia**	**Malawi**	**Uganda**
Eligible*Treated Wave	0.024***	0.152***	0.078***	0.041**	-0.019	-0.013**
	(0.002)	(0.021)	(0.014)	(0.010)	(0.009)	(0.005)
Treated Wave	0.040***	-0.028	0.269***	-0.387***	-0.067***	0.064***
	(0.005)	(0.017)	(0.013)	(0.010)	(0.007)	(0.006)
N	402,354	861,412	972,330	211,826	455,612	854,491
Adj R-Squared	0.023	0.094	0.099	0.059	0.062	0.087
Control Wave's Mean	0.895	0.756	0.787	0.470	0.786	0.798

Table notes: Estimates are person weighted, include cohort FE, district fixed effects, district X Post indicators, and SE are clustered on age. * for significance levels.

Table D8: Effects of Early and Late FPE Exposure on Female's Employment:Robustness

	Outcome: Employment					
	Panel A: Strict Treatment					
	Ghana	**Kenya**	**Tanzania**	**Zambia**	**Malawi**	**Uganda**
Eligible*Treated Wave	0.020***	-0.015*	0.016***	0.014*	-0.121***	-0.028***
	(0.004)	(0.007)	(0.002)	(0.005)	(0.017)	(0.002)
Treated Wave	0.020***	0.029***	0.046**	-0.029***	-0.080***	-0.005**
	(0.002)	(0.003)	(0.012)	(0.007)	(0.007)	(0.002)
N	360,252	651,496	765,617	99,492	358,016	681,995
Adj R-Squared	0.017	0.056	0.023	0.067	0.118	0.057
Control Wave's Mean	0.973	0.872	0.981	0.920	0.873	0.978

Table notes: Estimates are person weighted, include cohort FE, district FE and district FE X post indicator, and SE are clustered on age. * for significance levels.

Table D9: Effects of Early and Late FPE Exposure on Female's LFP:Robustness

	Outcome: LFP					
	Panel A: Strict Treatment					
	Ghana	**Kenya**	**Tanzania**	**Zambia**	**Malawi**	**Uganda**
Eligible*Treated Wave	0.017***	0.162***	0.075***	0.042**	-0.014	-0.012
	(0.003)	(0.023)	(0.014)	(0.010)	(0.010)	(0.006)
Treated Wave	0.033***	-0.036**	0.128***	-0.245***	-0.014**	0.162***
	(0.002)	(0.011)	(0.006)	(0.005)	(0.004)	(0.002)
N	402,354	861,412	972,330	211,826	455,612	854,491
Adj R-Squared	0.014	0.090	0.096	0.054	0.045	0.080
Control Wave's Mean	0.895	0.756	0.787	0.470	0.786	0.798

Table notes: Estimates are person weighted, include cohort FE, district FE, district FE X age dummies, and SE are clustered on age. * for significance levels.

Table D10: Effects of Early and Late FPE Exposure on Female's Employment:Robustness

	Outcome: Employment					
	Panel A: Strict Treatment					
	Ghana	**Kenya**	**Tanzania**	**Zambia**	**Malawi**	**Uganda**
Eligible*Treated Wave	0.018***	-0.022**	0.012***	0.017**	-0.119***	-0.024***
	(0.004)	(0.007)	(0.002)	(0.006)	(0.017)	(0.002)
Treated Wave	0.027***	0.064***	0.023***	0.032***	-0.114***	-0.014***
	(0.002)	(0.004)	(0.001)	(0.004)	(0.006)	(0.002)
N	360,252	651,496	765,617	99,492	358,016	681,995
Adj R-Squared	0.016	0.057	0.027	0.067	0.113	0.064
Control Wave's Mean	0.973	0.872	0.981	0.920	0.873	0.978

Table notes: Estimates are person weighted, include cohort FE, district FE, district FE X age dummies, and SE are clustered on age. * for significance levels.

employment results are virtually unchanged for the early FPE countries. Point estimates in the late FPE countries are also similar to my main results, but precision for Malawi falls, so that there is no longer a statistically significant decline in female LFP rates. In the more stringent test that includes district fixed effects interacted with age dummies in Tables D9 and D10, we see a similar pattern: the early FPE country results remain very similar, and the late FPE country results are similar in magnitude but precision falls in both Malawi and Uganda. Note that in these late FPE countries, the point estimates are still large and negative.

D.8 Testing for differential cohort sizes in the DiD framework

I investigate whether HIV-related mortality changed the overall cohort size of the most (or least) treated cohorts. I generate the log of adult female population in each country, year of Census, gender, age group, and district and then estimate the following, clustering standard errors on age:

$$LnPop_{art} = \beta_0 + \beta_1 Eligible_a * Post_t + \beta_2 Eligible_a + \beta_3 Post_t + \gamma_a + \lambda_t + \rho_r + \epsilon_{art} \quad \text{(D1)}$$

The results of this Table D11 show no significant differences in cohort size in the exposed cohorts of the treated wave in any country.

Table D11: Effects of Early and Late FPE Exposure on Cohort Size: Females

	Outcome: Ln Pop					
	Panel A: Strict Treatment					
	Ghana	**Kenya**	**Tanzania**	**Zambia**	**Malawi**	**Uganda**
Eligible*Treated Wave	0.228	0.245	0.032	0.068	-0.111	-0.095
	(0.155)	(0.117)	(0.055)	(0.062)	(0.072)	(0.085)
Treated Wave	-0.833***	-1.835***	-0.739***	-0.546***	0.868***	0.848***
	(0.062)	(0.068)	(0.055)	(0.052)	(0.061)	(0.055)
N	400	288	920	224	1,508	2,117
Adj R-Squared	0.904	0.932	0.959	0.960	0.952	0.902
Control Wave's Mean	6.579	7.485	6.467	6.654	5.361	5.562

Table notes: Estimates are person weighted, include cohort FE, district FE and district FE X post indicator, and SE are clustered on age. * for significance levels.