Mass schooling increases citizens' political capabilities but does not strengthen civic engagement: New evidence from Africa

Taryn Dinkelman*[1] and Virna Vidal-Menezes[2]

[1]University of Notre Dame , NBER, CEPR, BREAD

[2]University of Notre Dame

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Evidence from economically advanced Western democracies demonstrates that more schooling can raise civic engagement. We present new evidence from Africa that while mass schooling increases human capital, it does not generally create more politically active citizens. Exploiting variation in free primary education (FPE) policies and harmonized Afrobarometer and Demographic Health Survey data, we implement a consistent difference-in-differences research design in each of 17 African countries, comparing outcomes across cohorts with differential exposure to FPE in childhood, before and after FPE was introduced. FPE increased primary school completion by 2 percentage points, significantly raising human capital – in particular, literacy – by over 3.5 percentage points in 15 countries. Yet these increases in human capital do not significantly increase political engagement through voting nor strengthen pro-democratic political attitudes; in a number of countries, exposure to FPE significantly *reduces* political engagement. Results from a regression discontinuity design support our difference-in-differences analyses. Our results showing the disconnect between mass schooling and political engagement in Africa suggest that current theories may overstate the extent to which investments in human capital alone generate political participation.

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1 Introduction

The worldwide expansion of mass education is often viewed as a foundation of democratic development. Schooling creates literate citizens, building the ability to acquire and assess political information, evaluate governments, and participate in collective decision-making: what we might think of as enhancing individual political "capacity" or political capabilities. Yet, whether increases in human capital actually produce more politically active and engaged democratic citizens remains uncertain, especially in countries where rapid educational expansion has occurred alongside uneven democratic consolidation.

In Africa, the role of human capital in generating politically active citizens is of particular importance. Following two decades of rapid democratization beginning in the late 1980s/early 1990s (the so-called "Third Wave" of democratization), the last twenty years have seen significant rollbacks of democratic institutions and practices across the continent (IDEA, 2025). Today, more Africans live under fully or partially authoritarian states than under democratic governance (Nord et al., 2026). At the same time, human capital has increased in many sub-Saharan African countries since 2000. It is an open question as to whether this additional human capital played any role in affecting individual decisions about whether to vote, whether to engage with political leaders, or how to evaluate the functioning of democratic governments.

In this paper, we present new evidence on the relationship between mass schooling policies and political engagement of citizens differentially exposed to such policies in Africa. We exploit variation in the introduction of free primary education (FPE) policies across the continent from the 1960s to the early 2000s (documented in Dinkelman et al. (2026)) to generate plausibly exogenous variation in human capital attainment. Combining our policy timing data with multiple rounds of harmonized Afrobarometer and Demographic Health Surveys fielded across 17 countries, we implement a consistent difference-in-differences empirical strategy in each country, as well as a pooled difference-in-differences design for all countries. We compare schooling attainment, literacy rates, political attitudes, and political

actions across adult cohorts with more versus less exposure to FPE in childhood, before and after FPE was introduced.

A distinguishing feature and strength of our analysis is that it involves 17 low-, lower-middle-, and middle-income African countries. Each introduces FPE at a different time and implements the policy in different ways. Labor markets, levels of economic development, and baseline levels of democracy differ vastly across these countries. At the start of our analysis period, Malawi is the poorest country with GDP per capita of 224USD and Ghana the richest, with GDP per capita more than ten times higher. Uganda is the least democratic country while Cabo Verde is the most democratic, based on values from the V-Dem index. By implementing the same research design on the same type of data for each of the 17 countries, we learn which results generalize and which may be more context-specific.

Identification of our continuous treatment difference-in-differences estimates relies on a parallel trends assumption. Since neither the Afrobarometer surveys nor DHS surveys extend sufficiently far back in time, we cannot check for pre-trends. Instead, to bolster our main findings, we implement an RD design as a sharper test of the impact of FPE around the exact age of primary school eligibility, one which is insulated from concerns about non-parallel trends.

The first key result of the paper is that mass schooling policies generally raised education outcomes in most places. Our pooled analysis from the Afrobarometer data shows that FPE exposure increased primary school completion by 2 percentage points and raised educational attainment by about 4 percent. In 12 of our analysis countries, FPE policies significantly raised the probability of completing primary schooling by between 3 and 12 percentage points. These are economically meaningful gains, since baseline primary school completion rates range from 20% to 80%. Four of the countries without a significant positive relationship between FPE exposure and schooling attainment had each experienced long-running civil wars in the prior three decades – Burkina Faso, Liberia, Mozambique, and Zimbabwe, with

Lesotho experiencing a long-running guerilla war in the 1980s.[1] In addition, although most countries only implemented FPE without extending zero tuition polices into high school, we show large knock-on effects of FPE on high school completion in 10 countries, where the share of adults with completed high school also increases by between 3 and 13 percentage points.

Does the FPE-induced increase in human capital attainment represent real increases in the political capacity of citizens? We turn to using DHS data on literacy outcomes for 16 countries (there are no data for Cabo Verde) to show that in almost all countries, the answer is a resounding yes: FPE raised citizens' capabilities for participating in political processes by raising literacy. Our pooled analysis indicates that FPE exposure raised literacy by 3.5 percentage points on average (or 5 percent relative to overall mean literacy). Across countries, literacy rates increased by between 1.8 and 18.8 percentage points. Two countries stand out in this analysis: there are zero gains in literacy in Mozambique and in Zimbabwe.[2]

The second part of our analysis connecting schooling to political actions and attitudes restricts the sample to the 15 countries in which FPE generated significant gains in measured human capital and/or significant gains in literacy rates, or both.

In this subset of 15 countries, the pooled difference-in-differences analyses shows that greater FPE intensity has no significant impact on voting in national elections, while at the same time raising scores on a political action index, and on a pro-democracy index. These pooled estimates mask substantial heterogeneity across countries. Greater FPE exposure increases political actions in Malawi and Senegal without affecting actual voting, but reduces political action in Lesotho; those with more FPE exposure vote at higher rates in Zambia and Burkina Faso (6-7 percentage points higher), but at significantly lower rates in Ghana, Uganda, Benin, Liberia, and Mali. Greater FPE exposure either pushes attitudes towards

[1] Nigeria is an exception to our finding that FPE raises schooling in the majority of countries; we find it reduces total attainment in Nigeria. We discuss this further in our results section.

[2] The lack of first stage impacts of FPE on human capital in Mozambique and Zimbabwe is likely related to the respective 15 year civil wars that occurred in each country, ending in 1979 in Zimbabwe and 1992 in Mozambique. We discuss this exclusion later on in the paper.

the more pro-democratic, or has no significant impact on political attitudes at all.

Results from our (pooled) regression discontinuity design support these main results. Both the share of adults completing primary school and the share literate are higher at the age eligibility threshold, although not significantly so.[3] At that same FPE age eligibility cutoff, however, the share of adults who had ever voted is significantly *lower*.

Our work relates to several strands of work in economics and political science that link education with political institutions. Theoretically, Lipset's well-known modernization hypothesis emphasizes the positive causal link between education and democracy (Lipset, 1959): when general education levels increase as a country develops, the population grows more in favor of democracy. Putnam (1995) famously noted that education is *"the best individual-level predictor of political participation"*, although it is not clear that this participation need be in a democracy. Glaeser et al. (2007) argue that the probability of becoming a democracy, and the stability of a democracy once established, hinges on having many voters whose benefits of political participation are sufficiently high that they fight for it even in the absence of direct rewards. Mass schooling plausibly generates such supporters and thereby stabilizes democracy.

Empirically, education levels are highly correlated with democratic attitudes and political participation in democracies in both cross-section and panel regressions. The best econometric evidence – most of it from advanced western democracies – suggests that this effect is causal. For example, Milligan et al. (2004); Dee (2004) each present evidence that higher levels of schooling (induced by compulsory schooling laws) improve political interest in both the democracies of the US and the UK, yet only improve civic participation (voting) in the US. Some have suggested that the schooling-political engagement link operates through the labor market: Aaronson et al. (2026) provide evidence from the US that political activism grew faster in places where human capital of African Americans had increased rapidly and

[3]The lack of precision stems from the fact that the RD focuses just on a narrow bandwidth of ages around the FPE age eligibility cut off, so we lose power.

where Jim Crow laws continued to restrict black worker opportunity in the labor market.[4]

More limited evidence exists for the link between schooling and political engagement in low-income countries where democracies are often nascent and fragile. In Benin (Wantchekon et al. (2015)), Nigeria (Larreguy and Marshall, 2017) and Kenya (Lucas and Mbiti, 2012), more schooling increased civic engagement. In Senegal (Larreguy and Liu, 2024), Burkina Faso (Elodie and Kevane, 2023; André and Maarek, 2022) for men, and Zimbabwe (Croke et al., 2016), more schooling reduced political participation; while in Kenya, Friedman et al. (2016), education raises political awareness but not community participation, political efficacy, or intention to vote.

Our paper takes a big step forward in this literature. We measure adult engagement with politics in the period after the end of Africa's third wave of democratization, when countries are transitioning back to more authoritarian forms of governance. We expand the set of low- and lower-middle income countries for which we have evidence on the link between mass schooling and political participation. We use a common identification strategy and common data to test the link. Our results show that education expansion can successfully build citizens' skills and political capabilities *without* producing corresponding increases in democratic engagement that modernization theories predict. While there is a human capital return to mass schooling, there is little evidence of any democratic returns to mass education.

The dominant question has long been whether mass schooling produces more democratic citizens. Our evidence suggests that this may be the wrong question. Mass schooling clearly builds human capital. The unresolved question is why these gains fail to translate into greater democratic engagement. Explaining when – and why – human capital translates into civic engagement, especially in low-income countries with fragile democracies, emerges as a central unresolved question for the study of democratic development.

[4]Different strands of the literature focus on how political institutions, especially democracies, might have incentives to invest in mass schooling e.g. Bandiera et al. (2018). It is of course possible for governments to have incentives to invest in shoring up institutions by increasing access to mass schooling at the same time that individuals gaining new access to education are becoming more informed and more politically active. We consider drivers of FPE laws in Africa in a separate paper (Dinkelman et al., 2026) but focus on the impacts of FPE laws on individual attitudes and political behaviors in this paper.

We begin with a brief discussion of the background to FPE policies in African countries from the 1960s onwards and describe political regimes and the changing nature of democracy in Africa using commonly-used indices from V-Dem. We outline our empirical strategies and explain how we use Afrobarometer data to construct measures of FPE exposure and education and political outcomes, and DHS data to construct measures of literacy. The main results and discussion follow.

2 Democracy and mass schooling in Africa

There have been two key periods of democratization on the continent: the first coincided with independence movements of the 1950s and 1960s, when colonial powers ceded control to newly democratically elected governments. Many of these democracies turned towards authoritarianism soon after, and the period from the 1970s through 1990s saw a decline of democracy on the continent. Following the fall of the USSR in 1991, many African countries experienced a rapid second wave resurgence of democracy, as countries transitioned to democratic rule throughout the 1990s and into the 2000s (Grimmer, 2024). Since then, some countries have maintained stable democratic systems, while others have experienced more institutional instability especially with the occurrence of civil wars. Still others are now referred to as anocracies, simultaneously having characteristics of democratic governance (e.g. elections) and autocratic governance (e.g. without real competition in elections, and generally weak institutions). At present, over 60% of all Africans live in autocratic countries (Nord et al., 2026).

What does it mean to be a democracy in sub-Saharan Africa? To measure variation in the strength of democracies since 2000, we use the V-Dem Electoral Democracy Index ($v2x_polyarchy$), which combines different elements of society into a quantitative measure of democracy.[5] Figure 1 shows how values of this index of democracy change over time in each

[5] Lührmann et al. (2018) describe different ways to define democracy based on variables captured in the V-Dem database, https://v-dem.net/. Their electoral democracy requires three conditions: (1) a

of our countries. A binarized version of this index would define a democracy as a country with a score above 0.5 on this index.

Figure 1 shows wide variation in levels of electoral democracy across countries in our sample, and in some countries, large fluctuations in this measure over time too. Cabo Verde and Ghana tend to score the highest on the democratic index, while Uganda and Zimbabwe consistently score the lowest. Burkina Faso, Liberia, Madagascar, and Mali each have very large swings in values of the V-Dem index, suggesting much more volatile political institutions.[6] Over half of our countries would not be classified as democratic (their index values are below 0.5) relative to Western Democracies, despite the fact that most of them have regular election cycles.[7]

In many parts of Africa, political independence of the 1950s and 1960s was accompanied by a focus on mass education, often with a view to promote a new national identity. Different African countries invested in education in different ways: for example, Tanzania focused on expanding primary education while other countries began by expanding secondary and tertiary education in order to produce skilled workers who could fill positions in nascent civil services (including as teachers and medical staff) (Oketch and Rolleston, 2007). Enrollment in many countries increased steadily during these Early Waves of FPE in the 1960s and 1970s, and education budgets rose alongside.

Multiple global economic crises and political turmoil in the late 1970s and 1980s brought a halt to these increases in education levels. Many African countries had to accept externally-imposed structural adjustment policies (SAP) that traded IMF loans for stringent conditionalities that facilitated debt servicing, e.g. smaller budgets, cost-sharing and cost-efficiency measures implemented for government services in the health and education sectors. These

score above 2 on the V-Dem indicator for multiparty elections ($v2elmulpar_osp$); (2) a score above 2 on the V-Dem indicator ($v2elfrfair_osp$) for free and fair elections; and (3) a score above 0.5 on the V-Dem Electoral Democracy Index (EDI, $v2x_polyarchy$). We use the single Electoral Democracy Index to get a broad democratic "ordering" of our countries.

[6] Liberia has had a civil war since 2000; Madagascar has had multiple coups; while Mali and Burkina Faso have experienced both civil wars and coups in this period.

[7] A different measure of political institutions, the Polity IV project, classifies a majority of African countries as anocracies between 1990 and 2012. See Figure 2 in Gylfason (2013).

SAPs led to user fees being reintroduced in the education and other social sectors (e.g. health). It became much harder for families to enrol their children in primary school in the 1980s, and enrollment numbers dropped.

In 1990, the World Declaration on Education for All was signed in Jomtien, Thailand (Pritchett, 2025), after which the international community committed to supporting free primary schooling around the globe. Many of the same African countries that had been pushed into user fees for education were then pushed to eliminate user fees and expand access to universal and free primary schooling (Riddell, 2003). International donors provided funding to expand and improve primary education sectors in many places. We use the term Late Wave policies to describe FPE policies enacted during this period. See Dinkelman et al. (2026) for a detailed discussion of the collection of Early and Late Wave FPE Policies, and the background to these waves of policy reforms across the continent.

Figure 2 illustrates trends in primary school completion rates among adults 25 years and older over the last quarter century, for 16 of the 17 countries we will study.[8] Note that a 25 year old adult in 2002 would have been in primary school in the early 1980s, while a 45 year old adult would have been in primary school during early independence.

The figure shows large and persistent level differences between countries: Mali consistently has some of the lowest levels of primary school completion (below one in five adults) while Zimbabwe consistently has some of the highest levels (70% or higher). In all countries, adult cohorts see an increase in primary school completion rates between 2000 and 2024, although rates of change differ: Kenya and Lesotho, for example, seem to have steeper increases in primary school completion in some parts of the period. By 2024, at least half of our countries still have primary school completion rates below 50%. Because primary school completion is often synonymous with literacy, this figure underscores the massive literacy deficit that still plagues the continent.

[8]We use UNESCO data compiled by the World Bank Open Data. Not all countries have data on this variable. See: https://data.worldbank.org/indicator/SE.PRM.CUAT.ZS?end=2020&locations=TZ-UG-ZM-GH-KE-MW&start=1985.

3 Empirical strategy: Difference-in-differences

Our goal is to investigate whether more schooling leads to more political engagement and pro-democratic attitudes. Selection presents a key set of challenges to identifying causal relationships between schooling and civic engagement. Selection on wealth or ability makes it difficult to infer causality from differences in political attitudes and actions between those (and their families) who *choose* more versus less schooling.

To reduce concerns about selection into schooling, we isolate increases in schooling attainment driven by nationwide changes in free primary education (FPE) policies. Specifically, we exploit variation in the introduction and reversal of FPE policies across 17 countries. We use the legal year of FPE policies to define introduction of the policy and then isolate which cohorts in each country would have been exposed to free schooling during their elementary school years.

First, we restrict to a sample of those ages 18 to 60 years inclusive, and then identify years of data (survey rounds) where some adults interviewed in those rounds would have been exposed to FPE policies ($TreatedRound_t = 1$), and other years (survey rounds) where no adults would have been exposed (an untreated round, $TreatedRound_t = 0$) to those policies. In Early FPE countries, the treated rounds are early (rounds 1-5) and control rounds are late (rounds 6-8), while in Late FPE countries, the treated rounds are late (rounds 6-8) and control rounds are early (rounds 1-5).[9]

We construct a measure of exposure to FPE policies which we call $FPEIntensity_{it}$, to

[9]We further impose age restrictions on the samples of countries with multiple FPE waves, so that individuals in an early treated or control wave that would have been treated in a later wave of FPE are excluded from the analysis of early FPE effects; and similarly those who would have been exposed to an early FPE wave but show up as a control for a late FPE wave are excluded. In five countries, We deviate from the 18-60 age sample selection, first losing some of the youngest cohorts under Early FPE who would have been eligible for late FPE; and then losing some of the older cohorts under Late FPE who would have been exposed under Early FPE. This restriction also ensures that we don't use the same individuals as controls to evaluate the impact of each early and late wave. We always compare treated individuals with older individuals who did not have access to FPE. For the early FPE, these are individuals born before FPE took place; for the late FPE, these are individuals born between the two FPE waves. The samples used in the Early Wave analysis are: Ghana (age 43-60), Kenya (age 21-60), Nigeria (age 24-60), Tanzania and Zambia (age 30-60), Zimbabwe (age 19-60). The samples used in the Late wave analysis are: Ghana (age 18-38), Tanzania (age 18-47), Nigeria (age 18-46), Zambia (age 18-48), Zimbabwe (age 18-36).

capture the share of total years of an individual's primary schooling years that overlapped FPE years, by virtue of their birth year relative to the FPE years. $FPEIntensity_{it}$ is therefore a continuous measure of exposure. The idea is that the more years in childhood that an individual would have been eligible for FPE, once it was introduced, the more schooling they may have been able to attain.[10]

For individuals exposed to the FPE policy, our treatment variable for person i in round t is defined as:

$$FPEIntensity_{it} = \frac{YearsExposedFPE_{it}}{MaxExposureFPE_{t}} \tag{1}$$

Where $YearsExposedFPE_{it}$ is calculated based on the age of individual i at the time FPE was legally in place and the maximum age at which education is still free, or the age of the individual at the time the policy shut-down, for the early FPE analysis. $MaxExposureFPE_{it}$ is the maximum length of free education in the country, or the duration of FPE in early waves where the policy was terminated, often before the full length of primary education was reached.

To give an example of how this variable works, consider an Early Wave country that had an FPE policy in place for 5 years and in which children enter primary school at age 6 and complete primary school at age 11 (grades 1 to 6 inclusive). In this country, $MaxExposureFPE_{it} = 5$. For person i who was age 6 in the year the policy was introduced, their $YearsExposedFPE_{it}$ would be 5, and their $FPEIntensity_{it} = 1$. However, for someone who was age 9 when FPE was introduced, they would be eligible for only 3 years of free education, so their $FPEIntensity_{it} = 0.6$. And someone who was 12 when the FPE policy came into place would have $FPEIntensity_{it} = 0$; likewise someone who was just born.

The $FPEIntensity_{it}$ measure is therefore a function of age relative to the FPE policy dates and country-specific rules about age eligibility, as well as a function of the length of time the policy is or has been in place. The distribution of this variable within each country

[10]This is similar to the definition of treatment exposure used in Croke et al. (2016).

is shown in Figure A.1.

Note that our measure of $FPEIntensity_{it}$ is akin to an intent-to-treat measure of exposure to free primary schooling. Although we implement $FPEIntensity_{it}$ under the assumption that schools enforce age policies around FPE, it is highly likely most countries do not strictly enforce compliance with age eligibility policies. Non-compliance with FPE eligibility rules reduces the actual signal in $FPEIntensity_{it}$.

3.1 Seventeen natural experiments

Within a survey round, $FPEIntensity_{it}$ allows us to compare adults with more versus less exposure to FPE. With multiple treatment rounds ($TreatedRound_t = 1$), this intensity measure acts like a moving window, following older and older individuals into the future. Because this treatment variable is a function of age, we will tend to end up comparing younger to older adults within a round. Yet, education is also rising over time in all of these countries, as we saw in Figure 2.

To deal with the potentially confounding effects of age trends in educational attainment unrelated to the introduction of FPE policies, we introduce information from adults in untreated waves of data ($TreatedRound_t = 0$) for each country. These "control" adults allow us to estimate age profiles for the outcome variables in the absence of FPE policies. Under a country-specific parallel trends assumption, this provides us a good set of counterfactual outcomes for our treated individuals.[11]

We apply our measure of treatment intensity to adults in all untreated rounds to generate a measure of "potential intensity"; that is, we implement the same cohort definitions of $FPEIntensity_{it}$ to the same ages among adults in the untreated rounds. For example, if someone aged 30 in a treated round was aged 9 at the start of FPE policy, and as a result their $FPEIntensity_{it} = 0.6$, we assign 0.6 to 30 year-old individuals in all untreated rounds

[11] We will discuss how these assumptions need to be modified with this continuous difference-in-differences specification.

too.[12]

We use the following specification to estimate the effect of FPE on educational attainment and on political actions and attitudes. Equation 2 has the flavor of a differences-in-differences specification, where $FPEIntensity_{it}$ is a continuous treatment measure:

$$y_{ilt} = \beta_0 + \beta_1 FPEIntensity_{it} * TreatedRound_t + \beta_2 FPEIntensity_{it} + \rho_t + \beta_3 Female_i + \beta_4 Urban_i + \sum_{k=19}^{60} 1(Age_i = k) \cdot \gamma_k + \sum_{m=1}^{7} 1(Religion_i = m) \cdot \theta_m + \mu_l + \epsilon_{ilt} \quad (2)$$

Where y_{it} is the individual-level outcome of interest, ρ_t are round dummies, $Female_i$ equals one if individual i is female, $Urban_i$ equals one if individual i is resides in an urban locality, $1(Age_i = k)$ is a series of age dummies, $1(Religion_i = m)$ is a series of 7 religion dummies and μ_l are location fixed-effects.[13] We cluster standard errors at the location level.[14] We follow others in the literature who use Afrobarometer data in not using in-survey sampling weights in our analysis (Larreguy and Marshall, 2017; Hjort and Poulsen, 2019; Depetris-Chauvin et al., 2020).[15]

Importantly, for Late FPE waves, our untreated waves of data appear in rounds earlier than the treated waves. For the Early FPE waves, because FPE policies occurred in the 1960s through 1980s, and were then reversed, our untreated waves of data appear in rounds *later* than the treated waves (because the policy turns off). Our Equation 2 is applied in

[12]Note that in different treated rounds, a 30 year old might have different $FPEIntensity_{it}$ values because they were different ages in the same (historical) FPE year. This is the sense in which the intensity measure is a moving window, collecting older and older cohorts through time. To make a comparison with untreated individuals we have to choose a "placebo" value for similarly aged individuals in untreated rounds. In our current analysis, for each person i of age a in an untreated round, we conservatively impute the value of $FPEIntensity_{it}$ using the maximum value that $FPEIntensity_{it}$ takes within a country across treated rounds for individuals of age a. So, if a 30 year old in wave 3 had a value of $FPEIntensity_{it}$ =1, then we assign the intensity measure of 1 to 30 year olds in all control rounds.

[13]Based on what is consistently available across time within a country, we either use districts as locations, or sub-districts. See Data Appendix for more details.

[14]We do not cluster at the level of age; age indicators are already included and will account for fixed unobservable factors that may be related to specific ages. The seven religion categories are: None, Catholic, Protestant, Traditional Religion, Christian Other, Muslim, and Other; we include religion fixed effects and location fixed effects to follow the approach of (Larreguy and Marshall, 2017).

[15]Within-country sampling weights exist only from wave 4 onwards and we want to include earlier waves in our analysis for power reasons.

each country, regardless of whether they had only an Early FPE wave, only a Late FPE wave, or both.

The coefficient of interest in Equation 2 is β_1. If adults with greater exposure to FPE policies (high values of $FPEIntensity_{it}$) are differentially likely to have more education than those with less exposure (low values of $FPEIntensity_{it}$), even after controlling for differences in educational attainment between younger and older cohorts from non-FPE time periods, we will say that FPE policies raised schooling attainment. We estimate similar reduced-form relationships between the FPE policy exposure measure and political attitudes, political aspirations, and self-reported political actions outcomes. For each outcome, the estimate of β_1 tells us that moving from a treatment intensity of 0 to 1 is associated with a $\hat{\beta}_1$ larger (or smaller) change in the slope of y, under a linear dose-response treatment specification.

3.2 Pooled analysis: Aggregating across countries

By implementing the same research design across multiple countries, we generate 17 different estimates of the impact of FPE exposure on schooling and political aspirations and actions. Before presenting these results, we start with estimating a pooled version of Equation 2, modifying the specification to include a set of country fixed effects, δ_c:

$$y_{ilct} = \beta_0 + \beta_1 FPEIntensity_{ict} * TreatedRound_{ct} + \beta_2 FPEIntensity_{ict} + \rho_t + \beta_3 Female_i + \beta_4 Urban_i + \sum_{k=19}^{60} 1(Age_i = k) \cdot \gamma_k + \sum_{m=1}^{7} 1(Religion_i = m) \cdot \theta_m + \mu_{cl} + \delta_c + \epsilon_{ilct} \quad (3)$$

The estimate of β_1 in this pooled regression is the continuous difference-in-differences impact of FPE exposure on aggregate schooling, labor market, and political outcomes, after accounting for round, age, location, religion, and country fixed effects, and controlling for female and urban status. The pooled specification imposes common age, religion, round, and age effects across countries, as well as a common impact of FPE exposure.[16]

[16]We include country-specific weights in this approach to account for the fact that different countries

The pooled analysis tries to summarize what we learn "in general" about the impact of these FPE policies. However, the pooled estimates mask country-specific heterogeneity, which we will uncover in our main country-level empirical analyses.

3.3 Identification

3.3.1 Necessary assumptions

As with any difference-in-differences design, we assume parallel pre-trends across different levels of $FPEIntensity_{it}$. Showing parallel trends is difficult because we have many countries and because the Afrobarometer data do not stretch back far enough to allow checking of pre-trends in many countries.

Two aspects of our research design help to assuage concerns about potential violations of the parallel-trends assumption. First, we implement the design in Equation 2 across 17 different settings. Violations of the parallel trend assumption are unlikely to be at work in the same way in each country. Each country introduces FPE at different times, and implements the policy in different ways. To the extent that we find similar results for schooling across settings, we believe we have uncovered the true causal impact of these policies on educational attainment.

Second, in countries where both Early and Late Wave FPE policies were implemented, we reverse the order of the treated and untreated waves. In Early Wave countries, earlier rounds of data contain the exposed adults, while later rounds of data contain no exposed adults. In Late Wave countries, we use the more typical pre/post setup of later waves as treated waves, and earlier waves as untreated waves. If we find similar impacts of FPE policies within the same country using these two different designs, we will be more confident that non-parallel trends are not driving our results.

contribute different sample sizes to the pooled regression. See Data Appendix 7 for details.

3.3.2 Continuous treatment issues

$FPEIntensity_{it}$ is a continuous variable. As discussed in Callaway et al. (2025), when the treatment variable is continuous, a typical two-way fixed effects (TWFE) estimator can be problematic, unless a strong parallel trends assumption is invoked. In our setting, this means that we must invoke a parallel pre-trends assumption for each $FPEIntensity_{it}$ group. This would mean that, absent treatment (of any intensity), cohorts with different values of $FPEIntensity$ would have the same average change in outcomes, after controls.

Even if the strong parallel trends assumption holds, concerns about selection remain: if different groups select into more schooling depending on whether their $FPEIntensity_{it}$ is high or low, and if these different groups have different counterfactual outcomes, then our estimates are a mixture of an average treatment effect and a selection effect.[17] The basic problem is that the TWFE imposes a linear dose-response function, and it may not be, because of the selection happening at different values of the treatment.[18]

To maintain a sensible interpretation of β_1, we must add two more assumptions. First, linearity in the dose-response, so that moving from 0.1 to 0.2 has the same marginal effect as moving from 0.7 to 0.8. Second, no selection on dose-specific trends: higher-dose cohort-wave cells are not on different untreated trajectories than lower-dose cells. Finally, a dose must mean the same thing across cohorts and waves (although not across countries): a value like $FPEIntensity$ =0.4 is substantively comparable across cohorts/waves.

We conduct two tests to ensure that we likely have a linear dose-response function and that there is no selection on dose-specific trends. We binarize our treatment measure so that any non-zero value of $FPEIntensity_{it} = 1$, then we check the robustness of the main TWFE estimates when we replace the continuous treatment measure with this indicator of

[17] This is similar to the concern that IV estimates a Local Average Treatment Effect, and with heterogeneous treatment effects, the LATE is driven by groups that have large treatment effects and have the largest weights in the estimator.

[18] In our setting, after FPE, if the set of children choosing to get one more year of schooling from grade 1 vs from grade 6 is different, then we will have a hard time saying the treatment effects of FPE for those in grade 1 vs grade 6 is merely the effect of the treatment; it could also involve differential selection in each of the grades. This setting makes it tricky to compare different levels of treatment to each other.

treatment. Then, we bin the continuous treatment measure into low/medium/high intensity bins, interacting each bin with a $TreatedWave_t$ indicator to check for nonlinear relationships between the treatment and education outcomes. If we see similar results using a binary version of $FPEIntensity_{it}$, and fail to find evidence of nonlinear effects using the categories of treatment, we can be more confident that our main approach in Equation 2 estimates a meaningful average treatment effect.

3.4 Limitations

There are several limitations to our research design. First, we are unable to test for parallel trends in outcome variables prior to FPE policies being implemented, because the outcomes data do not extend far enough back in time. Second, there are some countries in which power could be an issue: where FPE policies are not sufficiently strong to get and keep kids in school, or where the quality of primary schooling is not high, the impacts on education or on literacy measured across all of those exposed may be positive but not significantly so.

Moreover, our measure of treatment intensity may be noisy; the actual take-up of education is still likely driven by families that value education more highly; there are potential spillovers (especially for the political opinion/aspirations/actions) between individuals with different levels of intensity. For example, if young people with more education are more likely to vote, perhaps they convince their older less-educated parents to vote too. Unfortunately, we can only identify the net impact of exposure to FPE policies on outcomes.

3.5 Alternative identification strategy: Regression Discontinuity Design

To address some of these concerns, we also implement a regression discontinuity design in the pooled sample. This design should be robust to concerns about non-parallel trends, a

key possible threat to validity.[19]

We specify the following RD model:

$$Y_{ict} = \alpha + \delta D_{ic} + h(Z_{ic} - c_c) + \eta_t + \epsilon_{ict} \tag{4}$$

where Y_{ict} are the relevant outcomes (either educational attainment outcomes or voting and political engagement/attitudes outcomes) for person i in country c in survey round t; $h(Z_{ic} - c_c)$ is a (in this case) linear function of the running variable and the country-specific cutoff age for earliest eligibility for FPE. $Z_{ic} - c_c$ is therefore the re-centered birth year, centered around the age of primary school entry. We control for survey round fixed effects η_t to account for time-varying shocks that may affect respondents differently across survey rounds. The estimate of δ_i is our estimate of the effect of being eligible for primary schooling on the relevant outcome variable.[20]

The variation we use in this RD differs from the variation in the difference-in-differences specification. In Equations 2 and 3, we compare outcomes across individuals with differential exposure to FPE policies based on country and year of birth, conditioning on urban status, female, religion variables, and age, location and survey round fixed effects. Age fixed effects are estimated off of individuals of the same age in different surveys. The treatment intensity measure depends on the year of birth in the specific round, relative to FPE introduction year. In our RD, we can narrow the focus to those individuals born close to the eligibility cutoff, based on year of birth alone.

The RD comparison is sharper than the difference-in-differences specification, and it insulates us against concerns about non-parallel trends. It does sacrifice sample size, though, in focusing on the narrow birth year window around the FPE eligibility age, and hence we sacrifice some precision. Nonetheless, finding similar results across both specifications should

[19]Limited sample size prevents us from implementing a separate RD for each country.

[20]Note that we follow (Croke et al., 2016) in using a 5 year window cutoff for the RD. We also follow them in excluding the partially treated cohorts – this with *Intensity* values above zero and below 1, retaining the full controls and the fully treated.

increase our confidence in our main findings.

4 Data

4.1 Afrobarometer surveys

The Afrobarometer survey collects data on an array of public attitudes questions, including topics such as democracy, economy, governance, and society. Data are available for 9 survey rounds, beginning in 1999, with the latest survey from 2022. The number of countries included in the surveys increased over the years, with 12 countries in the first round and 34 in the latest one.

Countries are included when we have enough data to observe outcomes for adults who were affected by FPE in childhood. The result is a sample of countries that are at different levels of economic development, with different levels of measured democracy, and different labor markets in the 2000s. All of these countries are in the lower- or lower-middle income country category of the World Bank. In 2005, GDP per capita (in current USD) in our sample ranges from 286USD in Malawi to 720USD in Zambia. There are also a wide range of geographies represented: for example, Benin and Ghana in West Africa; Kenya and Tanzania in East Africa, and Malawi and Zambia in Southern Africa.

The sample drawn for each country and round is intended to be a representative cross-section of all citizens of voting age in a country.[21] Relative to Census data (Census years do not always line up with Afrobarometer rounds), the Afrobarometer data is more rural in all of our countries. For instance, Ghana in 2010 was 54.4% urban in the Census and 44% urban in the Afrobarometer data. Within country, the Afrobarometer data varies quite substantially in the share of adults who are rural varies in rounds 1 and 5 relative to adjacent rounds. Specifically, the sample is overwhelmingly urban for Uganda (55.3% in round 5 compared

[21] See https://www.afrobarometer.org/surveys-and-methods/sampling/ for more discussion of sampling protocols in the Afrobarometer.

to 14.2% in round 4), Malawi (30% in round 5 compared to 14.3% in round 4) and Kenya (42.2% in round 5 compared to 20.6% in round 4), and rural for Ghana (26.3% in round 5 and 44% in round 4).

4.2 Sample choice

We use data from Afrobarometer rounds 1 through 9 in our analyses. In countries with Early FPE, rounds 1 through 5 are considered treated rounds, and rounds 6 to 9 are control rounds. For Late FPE countries, the opposite is true: rounds 1 to 5 are control rounds, and 6 to 9 are treated rounds. All adults ages 18 to 60 in every round are initially eligible to be included in our analysis.

The requirements of our research design lead us to have different age ranges in each country sample. We adapt our sample by age for each country, using the relevant date of FPE introduction/phase out in relation to the relevant survey round year. We do this to minimize the chances that anyone in untreated waves was exposed to any type of FPE, and ensure a "clean" set of comparison cohorts for each wave of FPE policy. We also do this to ensure that in the six countries with both Early and Late waves of FPE (Ghana, Kenya, Nigeria, Tanzania, Zambia, and Zimbabwe), individuals treated in the each of the FPE waves show up in our sample with the correct $FPEIntensity_{it}$ measure, and that those treated in the Early (Late) Wave do not show up as control cohorts in the Late (Early) wave.[22] By restricting samples in these policy-relevant ways, we avoid the issue of using the same individuals as controls in both the Early and Late waves in these six countries.

4.3 Key variables

We use data from the Afrobarometer survey to construct our key outcome variables:

Education outcomes: since there is no literacy variable, nor any variable that allows

[22]We create the measure of $FPEIntensity_{it}$ for each of the Early and Late waves in these six countries using the relevant treated rounds for each wave. Individuals in a given survey round can be treated with respect to the Early Wave, but untreated with respect to the Late Wave (or vice versa).

us to capture a measure of number of years of schooling attained, we construct three broad measures of attainment. First, we define an indicator variable for whether someone has completed at least primary education (or above). Since primary school completion rates are high in some countries, and especially in later waves, and because completed primary schooling is a necessary condition to proceed to secondary schooling, we then also use an indicator variable to capture whether someone has completed secondary schooling. Third, following Larreguy and Marshall (2017), we use the categorical education variable provided in the Afrobarometer to measure increases in educational attainment, ranging from 0 (no formal schooling) to 5 (higher education).

Political actions: Our main outcome related to political action is whether an individual reports having voted in the most recent national election. We also use principal component analysis to build a Political Actions Index out of six dummy variables that include: reported attendance in community meetings, raising issues with local leaders, protesting, discussing politics with friends and family, contacting local government councillers (LGC) and contacting members of parliament/national assembly (MP). Voting in the last election is a binary variable that equals one if the respondent voted, and zero if they did not, independent of the reason and provided that there were national elections taking place where in that country, and conditional on the individual being above the legal voting age in the country at the time of the elections. Attending community meetings equals one if the respondent reports attending meetings during the past year and zero otherwise. Raising an issue equals one if the respondent reports having raised an issue with a local leader in the past year, and zero otherwise. Protesting equals one if the respondent reports attending a demonstration in the past year, and zero otherwise. Discuss politics equals one if individuals report ever discussing politics with friends and family, and zero if never discussing or do not know. Contacting LGCs and MPs equals one if individuals report contacting these officials at any frequency, and zero if never contacted or do not know.

Political attitudes and opinions: The main outcome related to attitudes and opin-

ions towards democracy is a Political Attitudes Index that is comprised of preference towards democracy, support for multiparty systems, anti-military rule and anti-autocratic rule. We use principal component analysis to build the Political Attitudes Index out of the component questions. Preference towards democracy equals one if the respondent agreed with the statement *"Democracy is preferable to any other kind of government"*, and zero if they agreed to *"In some circumstances, a non-democratic government can be preferable"*. Support for multiparty system equals one if an individual disagrees or strongly disagrees with the statement *"There are many ways to govern a country. Would you disapprove or approve of the following alternatives: only one political party is allowed to stand for election and hold office?"*, and zero otherwise. Similarly, the variable no military rule and no one man rule equals one if an individual disagrees or strongly disagrees with the statements *"There are many ways to govern a country. Would you disapprove or approve of the following alternatives: the army comes in to govern the country?"* and *"There are many ways to govern a country. Would you disapprove or approve of the following alternatives: elections and [Parliament] are abolished so that the [president] can decide everything?"*, respectively, and zero otherwise.

In addition to these main outcome variables, we also use harmonized Afrobarometer variables that capture demographics including: age, gender, rural/urban place of residence, religion (7 categories) and district of residence.

4.4 DHS data

To characterize increases in human capital as increases in political capacity, it is important to know whether individuals exposed to FPE are more literate than those with no exposure. Unfortunately, the Afrobarometer data do not contain measures of literacy. Instead, we use DHS data from 16 countries (no surveys are available for Cabo Verde) for our analysis. We choose DHS survey waves closest in time to our Afrobarometer waves, and define treatment intensity in the same way, using information about the year of FPE implementation, the survey year, and the respondent's year of birth. DHS data contain most of the relevant

demographics we need in our analysis, including age, gender, rural/urban place of residence, district of residence and religion (for most countries except Tanzania). Appendix 7 contains further details of data construction.

5 Descriptive statistics

Table A.1 presents descriptive statistics for some of the main demographic characteristics of our Afrobarometer sample. Due to our sample restrictions, the mean age in each country across all rounds is around mid-thirties. The analysis samples range from an average age of 30 years old in Nigeria to 38 years old in Mali. Sample sizes range from around 5,600 in Mali to over 17,000 in Nigeria. Country samples are well-balanced on gender, but the share of urban population varies quite a lot across countries, as one might expect. Cabo Verde has the highest urban share, while Senegal has the highest share of women.

Our key measure of exposure to free primary schooling policies, $FPEIntensity_{it}$, varies from 59% in Zimbabwe to 88% in Benin. The variation across countries comes from a combination of how long the relevant FPE wave was in place in each country, the year of the Afrobarometer wave, and the age of adults in each sample. We show graphically the variation in the continuous measure of treatment in Figure A.1. Some countries have few 100% treated units (e.g. BFO, Zimbabwe, Liberia, Mozambique) while others have more of their sample with 100% exposure. All countries have some variation between 0 and 1.

Table A.2 contains similar information for the DHS samples. Because of differences in when surveys are administered and how many rounds of data exist for each country, the samples are slightly younger than the Afrobarometer samples; the female share is very similar; and urban shares vary a little between the DHS and Afrobarometer. Average intensity also tends to be on average higher in the DHS than in the Afrobarometer, ranging from 67% to 92%. This again is due to there being more earlier years of data in the Afrobarometer than in the DHS (and hence more older folks with 0 exposure to FPE).

6 Results

6.1 Civic engagement and pro-democracy attitudes rise with schooling: OLS results

Table 1 provides results from country-specific OLS regressions of political actions and aspirations on the level of an individual's schooling. We use an indicator for whether an an individual has completed primary schooling, and include all the other controls (female, urban, religion, location, and round dummies) as in Equation 2.[23]

In every country, adults who have completed primary school score significantly higher on both of the political action and political attitudes indices, and almost all of these relationships are statistically significantly different than zero. The pooled result is therefore also positive and significant: the common finding in the literature that more educated individuals are more pro-democratic, and more politically engaged, holds in this set of 17 African countries. Concerns about democracy are also significantly higher for those with more schooling in most countries (or not significantly different than zero). For the most active measure of political engagement, voting in a recent election, results vary more across countries: in 9 countries, those with more primary schooling are either significantly more likely to have reported voting, while in the remaining 8 countries, there is no significant difference in voting behavior between those with different levels of primary schooling.

Overall, the OLS results suggest a strong positive link between schooling and concerns about and engagement in democracy. This pattern is consistent with the idea that education is fundamental to political engagement. In the next section, we use our causal difference-in-differences framework to tease out whether schooling causes changes in these political outcomes.

[23]Results are similar if we instead use an indicator for completed secondary schooling, or the categorical variable for level of education attained.

6.2 Difference-in-differences: FPE exposure raises educational attainment

Figures 3 to 5 show the main impacts of exposure to FPE on educational attainment, or the point estimate from equation (3), $\hat{\beta}_1$ and standard error bars for completed primary schooling, completed secondary schooling, and overall level of schooling.[24] Tables A.3 to A.4 present the underlying regression results.

To interpret the coefficient on the intensity term, we can think of comparing someone who had no exposure to the FPE policies to someone who had maximum exposure to the FPE policies (moving $FPEIntensity_{it}$ from 0 to 1). Alternatively, we could multiply the coefficient estimates by the mean value of $FPEIntensity_{it}$ from Table A.1 in the treated sample.

The figures show that in the pooled sample, higher exposure to FPE policies increases educational attainment at both primary and secondary school level, and hence increases the final level of schooling attained. The pooled result indicates that complete exposure to FPE in childhood would have raised the primary school completion rate by 2 percentage points, or about 3% relative to the overall mean primary school completion rate. Greater FPE exposure raises primary school completion by between a statistically significant 3 (in Tanzania) and 12 percentage points (in Senegal), and by between 4 (in Zambia) and 13 percentage points (in Tanzania). These increases are economically meaningful, since baseline shares of primary school completion range from 0.2 to 0.8 in these countries.

We find larger and significant effects of FPE on the share of adults with completed high school, ranging from 3 to 13 percentage points. In the pooled sample, the impacts of FPE on completing high school are 50% larger than the impacts on primary school completion. The share of adults with completed secondary education is substantially lower (below 50%) in

[24]Results from the underlying regressions are in Appendix Table A.3. The first row of the table presents results for the pooled cross-country level regression, and remaining rows present results from individual level regressions of Equation 2 on each country sample. For reference, we show the mean of each education outcome for each country sample.

most countries, and more variable across countries. Recall that the FPE policies generally did not extend to free high school (a handful of countries including Ghana and Ethiopia did have free tuition at higher levels of schooling too, in some early years). Yet because completing primary school is a prerequisite to entering (and completing) high school, FPE policies may have led to more youth moving on to high school. The results in this table suggest that improving access to primary school through FPE programs had significant knock-on effects in many countries.

We also find consistent and in many cases significant increases in educational attainment for all of our countries using the categorical education variable, with the exception of Nigeria. Overall, the level of education increases by between 0.1 and 0.4 (categories) on a mean of 2.3.

In several countries, our empirical strategy identifies no significant impact of FPE on primary or secondary school completion: Burkina Faso, Lesotho, Liberia, Mozambique, and Zimbabwe fall into this group. Figures 3 and 5 show this clearly. In Nigeria, the impacts of FPE on schooling completion are statistically significantly negative. As noted earlier, Burkina Faso, Liberia, and Mozambique all had long-running civil wars that decimated infrastructure - including schools - over several decades. Lesotho experienced multiple coups in the late 1990s and significant political instability into the 2000s, which may have undermined investments in education.[25] While Zimbabwe, with the highest levels of overall education attainment (86% of adults having completed primary school and 44% having completed secondary school), had little room for large increases in educational attainment. Standard errors for Zimbabwe are also exceptionally large.

It is worth noting that the results for Nigeria contrast with what Larreguy and Marshall (2017) find in their paper examining the impact of FPE on schooling and political engagement. This is also the case for Zimbabwe, where Croke et al. (2016) find positive impacts of

[25]Using different data, Moshoeshoe et al. (2019) look at the short-run impacts of the introduction of FPE in Lesotho and show that although enrolment in primary schools skyrocketed by almost 20 percentage points in the two years around FPE introduction, grade for age attainment actually fell by 0.15 years. They argue this suggests that FPE reduced school quality in Lesotho.

FPE on educational attainment. While we use similar cohort-based empirical strategies, we define treated cohorts differently relative to each paper. We discuss these differences further in Appendix 7.

A key takeaway from these results is that FPE policies significantly raised educational attainment in many, but not all, countries. Any pooled analysis would miss these differences. Likewise, any focus on an individual country would miss the overall patterns, and the heterogeneity across countries.

6.3 Difference-in-differences: Linking FPE exposure to political capacity

Basic literacy is a key aspect of human capital that raises a citizen's ability to interact in the political process. Because Afrobarometer has no measure of literacy, and because recent debates about schooling in Africa are concerned about the low quality of education across the continent, we use DHS data to support our claim that FPE exposure did expand individual capacities to engage in political processes. We estimate the impact of FPE exposure on literacy outcomes.

Table 2 presents pooled results and country-specific results for literacy. Exposure to FPE policies did indeed raise literacy rates overall: the pooled estimate is a 3.5 percentage point gain in literacy. Literacy increases with exposure to FPE in almost all countries: in Nigeria, FPE raises literacy rates by 1.8 percentage points while in Burkina Faso it raises literacy by 18.8 percentage points.

Combining the information from the prior table with these results for literacy, we see that Mozambique displays no significant impacts for any of the human capital measures or for literacy. Zimbabwe has no impact on educational attainment, and negative results for literacy rates; while Nigeria has gains in literacy but negative impacts on human capital.

Since we are interested in the link between mass schooling, political capacity, and political actions and aspirations, we drop Mozambique, Zimbabwe, and Nigeria from the next stage

of our analysis of civic engagement. In the next section, we drill down into whether those adults who gained more schooling as a result of FPE also hold different political aspirations and engage in politics in different ways.

6.4 Difference-in-differences: FPE exposure and civic engagement

In contrast with the OLS results where we saw that higher schooling attainment significantly raised voting behaviors *and* political attitudes towards democracy, the causal analysis presented in Table A.4 suggests that greater FPE exposure does not systematically or significantly generate more active citizenry at the ballot box. Figures 6, 7, and 8 each present estimates for the difference-in-differences parameter in Equation 2, along with confidence interval whiskers.

Pooled results show that greater FPE intensity significantly raises the political action index and pro-democracy attitudes by 2 percentage points. However, FPE exposure reduces actual voting by a significant 1 percentage point. Pooled results conceal significant heterogeneity across countries. Greater FPE exposure increases political engagement in Malawi, and Senegal, but reduces it in Lesotho; it reduces voting in almost all countries (not always significantly so), except for Burkina Faso, Senegal, and Zambia Greater FPE exposure either pushes attitudes towards the more pro-democratic , or has no impact on political attitudes.

This heterogeneity across countries is also seen in Figures 7 and 8: For most countries, FPE exposure raises political engagement and attitudes towards democracy, although for a couple of countries, this relationship is statistically negative. This variation in impacts is remarkable, since FPE fairly uniformly raised schooling attainment in all places.

These results tell us that schooling does not everywhere translate into more political engagement, more pro-democratic attitudes, or more civic engagement. The results in Table A.4 contrast with the OLS regressions in Table 1: the strong positive correlation between education levels and political engagement does not seem to reflect a precise causal relationship. The bottom line takeaway is that neither a pooled analysis – even one exploiting the varia-

tion in FPE intensity – nor a focus on a particular country, would uncover this disconnect. Context matters, and much more context is needed for each of these African countries.

6.5 Robustness: RDD estimates of FPE impact on education and on voting behavior

Figures 9 and 10 show us the results from the alternative RDD design in equation 3.5. The first figure shows that if FPE is introduced in the year in which a person first becomes eligible for primary school, this raises their probability of completing primary school by 2.9 percentage points. This point estimate, while not statistically significant, is close to the point estimate from the pooled difference-in-differences analysis.

The second figure shows that the same birthyear cutoff significantly predicts a *decline* in the share of adults voting. The point estimate here is 5.9 percentage points, which is about three times as large as the pooled difference-in-differences estimate.

7 Discussion and conclusion

The modernization hypothesis states that as economies develop, education and literacy rise, and political systems shift towards stable democracies. Causal evidence linking higher levels of education to more democratic attitudes and greater civic participation is limited for less developed countries today, and the empirical evidence from these studies is often contradictory. We contribute to this literature by using a consistent research design and a common data source to present new evidence on these links for a large number of African countries. We investigate whether FPE-induced educational upgrading in Africa over the last several decades has changed political attitudes and civic engagement.

Our results suggest that the strongest signal of democratic engagement – that of voting in national elections – is negatively related to an individual's level of education: both on average, and significantly so in 5 countries. Civic engagement in the form of other political

actions (meetings, approaching local leaders) is more often higher, but also at times lower, for those most exposed to free schooling in childhood.

Our analysis generates two key questions for future research. First, how best can we understand the cross-country heterogeneity in the link between mass schooling and civic engagement? Our results push against relying on pooled analyses that aggregate across this heterogeneity, and at the same time dissuade us from placing too much weight on any single country being representative for the rest of the continent. Current theories may overstate the extent to which investments in human capital alone generate democratic participation.

Second, the level of overall education in many of our countries is very low and may still be too low to have consistently positive impacts on political engagement. Nigeria has the highest average secondary school completion rate in our sample of 17 countries (55%) while this rate is below 40% in much of the rest of our sample. In comparison, *mean* education in Arab Spring countries at the time of these uprisings was 7 to 9 years Campante and Chor (2012). FPE policies may not have gone far enough to raise average schooling to a level that might foment political protests and democratic demands. An important empirical question is then whether future expansions in access to secondary schooling in Africa will have different impacts on civic engagement in emerging democracies.

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Tables and Figures

Table 1: Education and Political Engagement: OLS correlations

	Voted (1)	Pol. Action Index (2)	Pol. Attitudes Index (3)
CVE			
Completed Primary	0.02**	0.07***	0.06***
	(0.01)	(0.01)	(0.00)
Ghana			
Completed Primary	-0.00	0.04***	0.06***
	(0.01)	(0.01)	(0.01)
Senegal			
Completed Primary	0.03**	0.05***	0.04***
	(0.01)	(0.01)	(0.01)
Benin			
Completed Primary	-0.00	0.05***	0.04***
	(0.01)	(0.01)	(0.01)
Lesotho			
Completed Primary	0.01	0.02***	0.08***
	(0.01)	(0.01)	(0.01)
Liberia			
Completed Primary	0.03**	0.07***	0.04***
	(0.01)	(0.01)	(0.01)
BFO			
Completed Primary	0.01	0.03***	0.07***
	(0.01)	(0.01)	(0.01)
Malawi			
Completed Primary	-0.01	0.03***	0.09***
	(0.01)	(0.01)	(0.01)
Mali			
Completed Primary	0.03	0.08***	0.09***
	(0.02)	(0.01)	(0.01)
Kenya			
Completed Primary	0.03***	0.06***	0.08***
	(0.01)	(0.01)	(0.01)
Nigeria			
Completed Primary	0.01	0.04***	0.05***
	(0.01)	(0.01)	(0.01)
Zambia			
Completed Primary	0.06***	0.04***	0.05***
	(0.01)	(0.01)	(0.01)
Tanzania			
Completed Primary	0.06***	0.06***	0.08***
	(0.01)	(0.01)	(0.01)
Madagascar			
Completed Primary	0.03**	0.05***	0.11***
	(0.01)	(0.01)	(0.01)
Mozambique			
Completed Primary	0.04***	0.03***	0.07***
	(0.01)	(0.01)	(0.01)
Uganda			
Completed Primary	0.02**	0.06***	0.05***
	(0.01)	(0.00)	(0.01)
Zimbabwe			
Completed Primary	0.02	0.02***	0.09***
	(0.02)	(0.01)	(0.01)

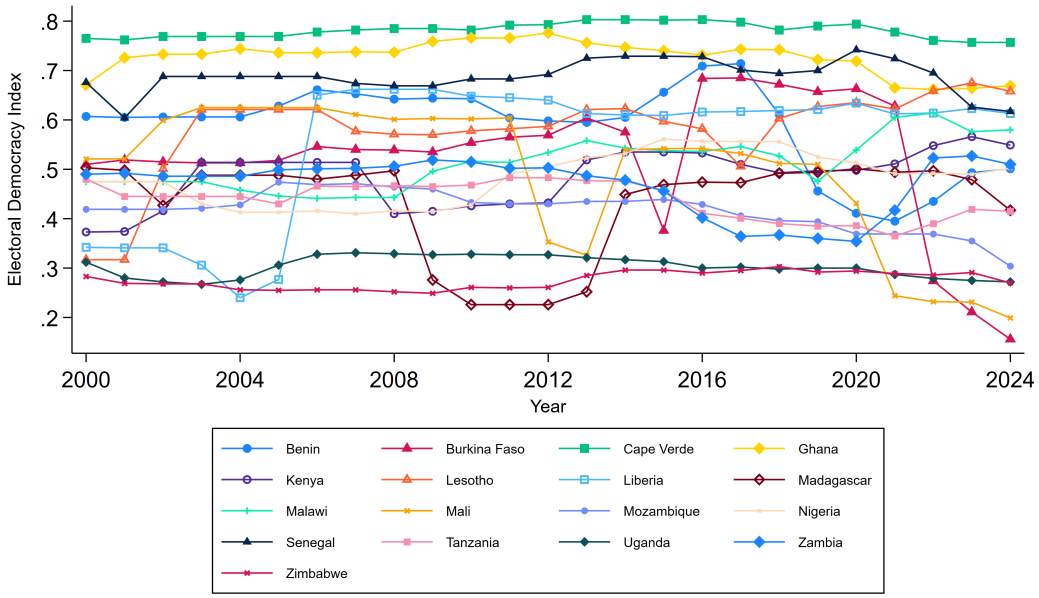
S.e. clustered at district level in parenthesis. All regressions include round, age, female, district, religion and urban dummies. Not all surveyed individuals responded to every question, hence observations vary slightly across outcomes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Effects of FPE on Literacy: Difference-in-differences estimates

	Mean Literacy (1)	Literacy (2)
Pooled	0.670	0.035***
		(0.004)
Ghana	0.615	0.154***
		(0.011)
Kenya	0.900	0.028***
		(0.004)
Nigeria	0.637	0.018***
		(0.006)
Tanzania	0.782	0.057***
		(0.007)
Zambia	0.719	0.065***
		(0.011)
Zimbabwe	0.957	-0.026***
		(0.007)
Benin	0.380	0.028
		(0.017)
Burkina Faso	0.363	0.188***
		(0.015)
Lesotho	0.902	0.028**
		(0.011)
Liberia	0.574	0.045***
		(0.016)
Madagascar	0.760	0.045**
		(0.015)
Malawi	0.719	0.012
		(0.014)
Mali	0.322	0.146***
		(0.023)
Mozambique	0.532	0.003
		(0.013)
Senegal	0.513	0.107***
		(0.016)
Uganda	0.701	0.070***
		(0.016)

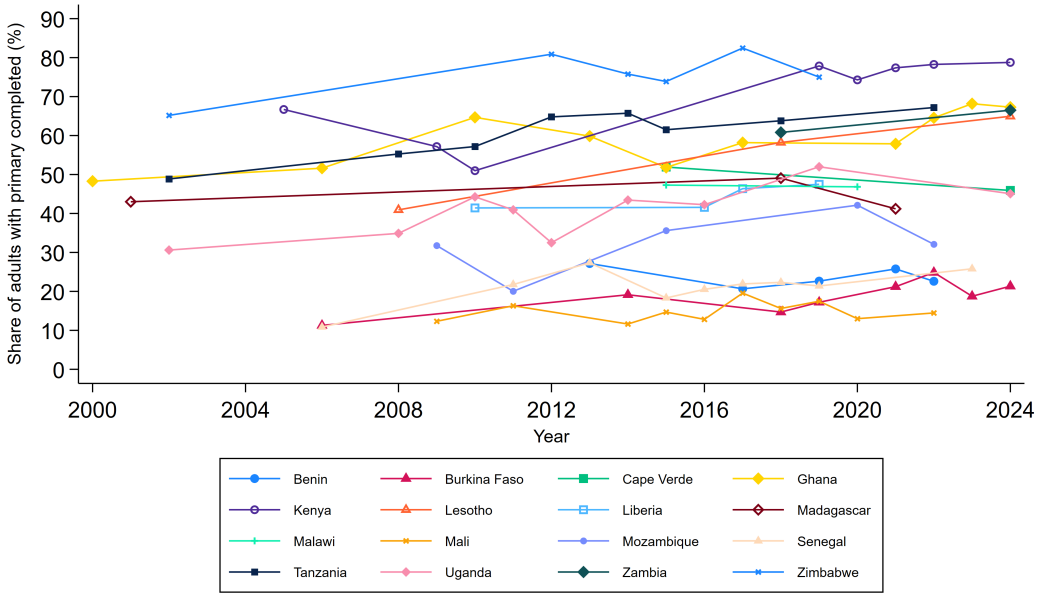
Notes: Mean literacy is weighted using de-normalized DHS sampling weights. Standard errors clustered at the location level are reported in parentheses. All specifications include age, female, urban residence, survey fixed effects, and location fixed effects. Religion controls are included except for Tanzania, where religion data are unavailable; Tanzania is included in the pooled regression by assigning all observations to one religion category and is estimated separately without religion controls. Country estimates pool Early and Late FPE samples where available. Zimbabwe uses the Early FPE sample only because Late FPE began in 2013 and the latest available DHS in our data is 2015. Regressions are weighted using de-normalized DHS sampling weights. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1: Electoral Democracy Index by Country and Year



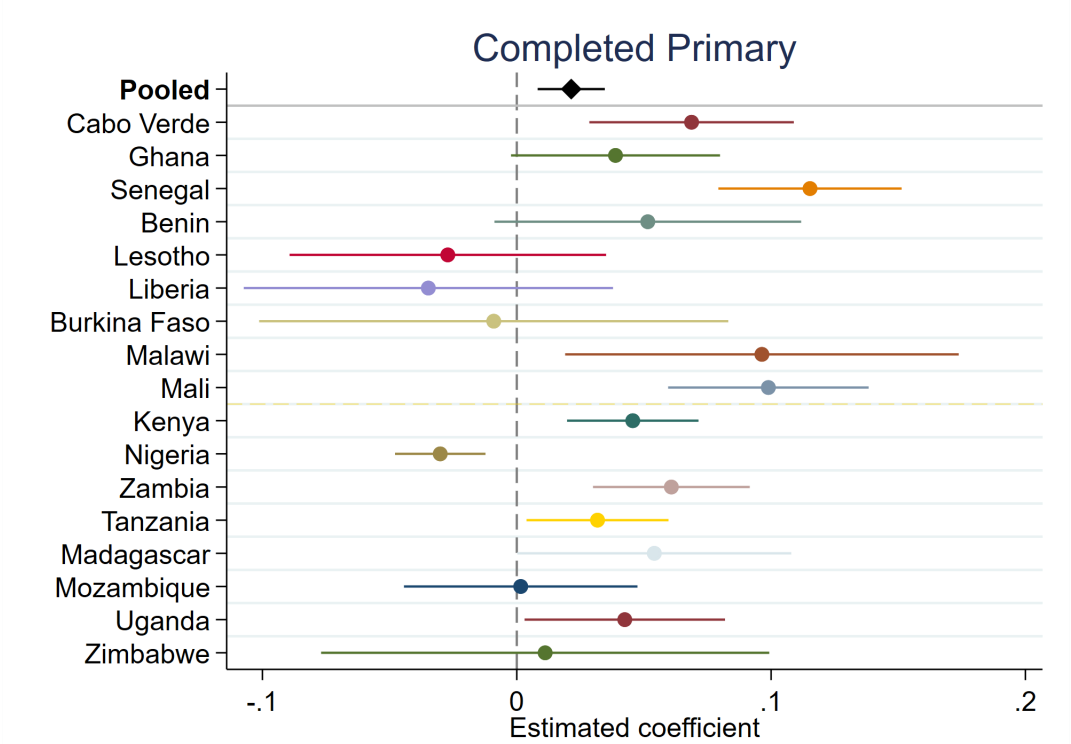
Note: Data are values of *v2xpolyarchy* from V-Dem.

Figure 2: Primary Schooling Completion Rate by Year and Country



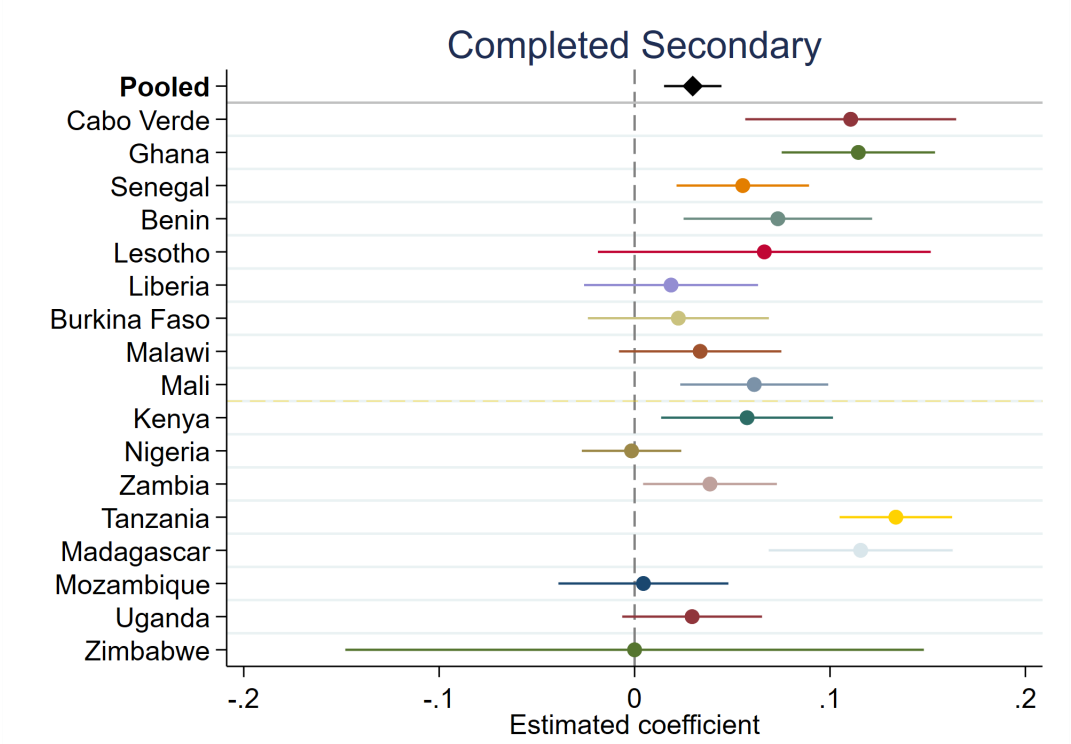
Note: Data comes from the Unesco Institute for Statistics compiled by the World Bank Open Data.

Figure 3: Completed primary



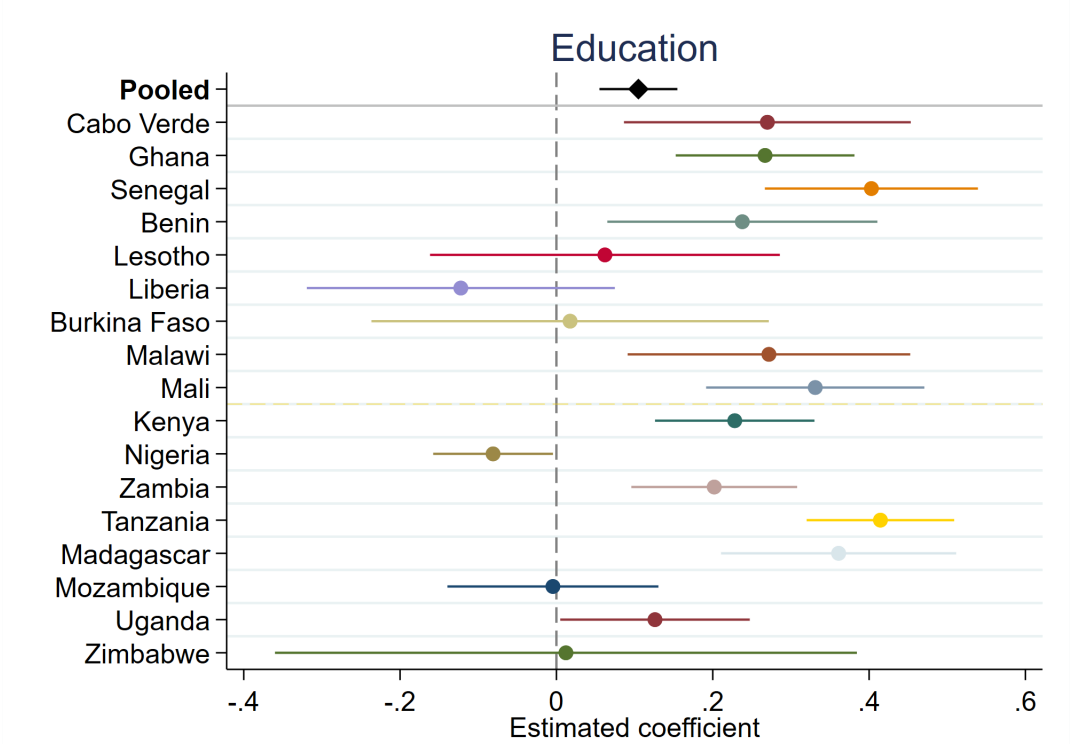
Note: Figure shows point estimates on the $Intensity * TreatedRound$ difference-in-differences term from each country-specific regression along with confidence interval whiskers.

Figure 4: Completed secondary school



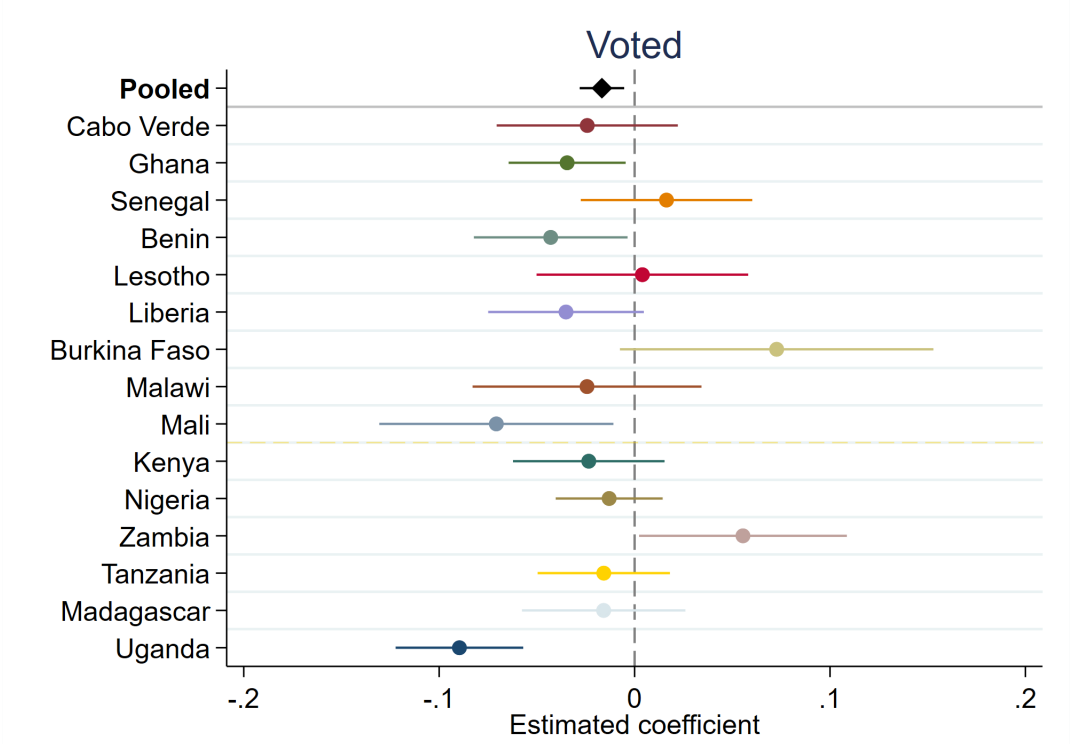
Note: Figure shows point estimates on the $Intensity * TreatedRound$ difference-in-differences term from each country-specific regression along with confidence interval whiskers.

Figure 5: Education level



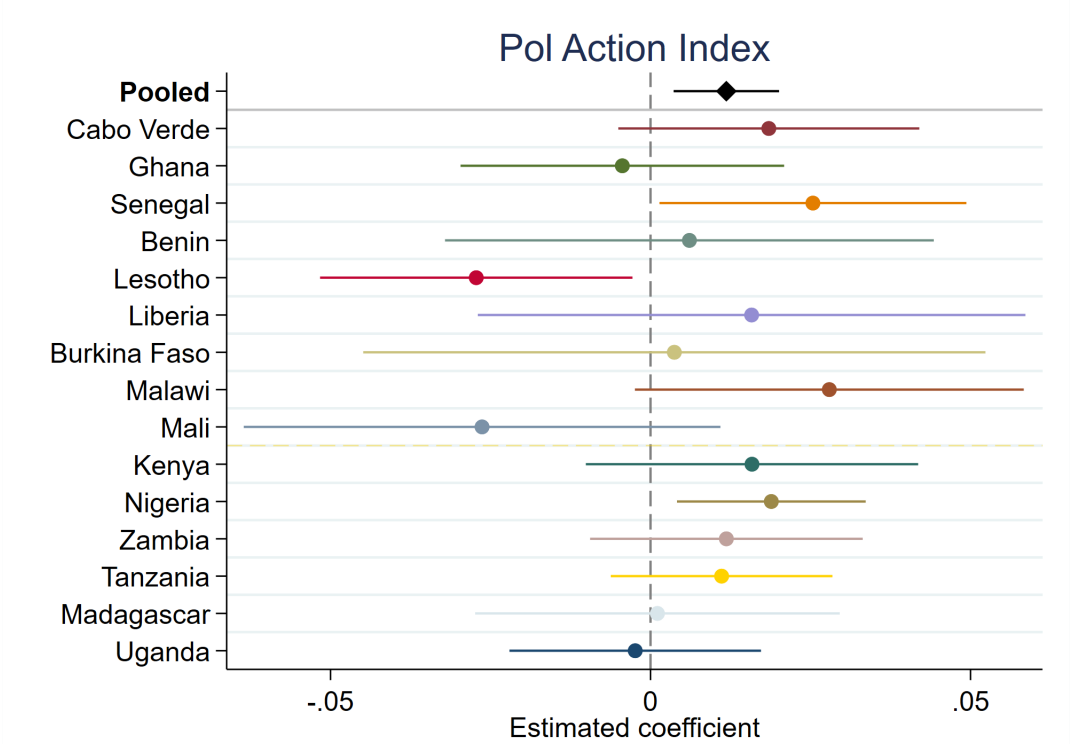
Note: Figure shows point estimates on the $Intensity * TreatedRound$ difference-in-differences term from each country-specific regression along with confidence interval whiskers.

Figure 6: Voted in Last Election



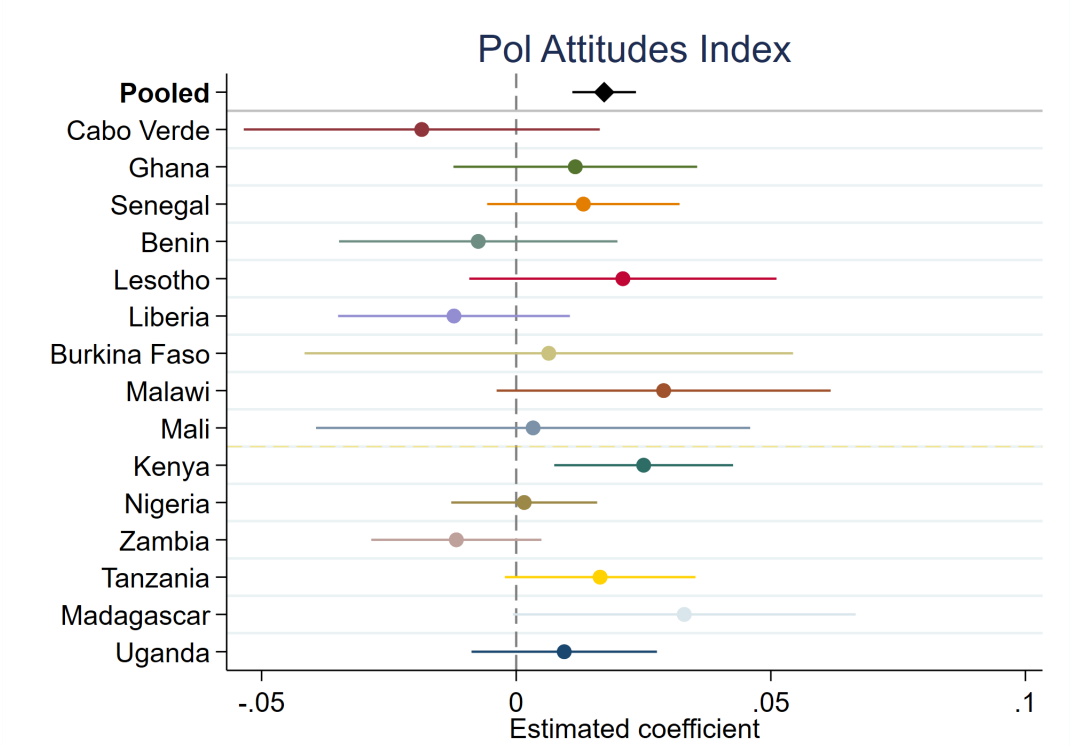
Note: Figure shows point estimates on the $Intensity * TreatedRound$ difference-in-differences term from each country-specific regression along with confidence interval whiskers.

Figure 7: Political Action Index



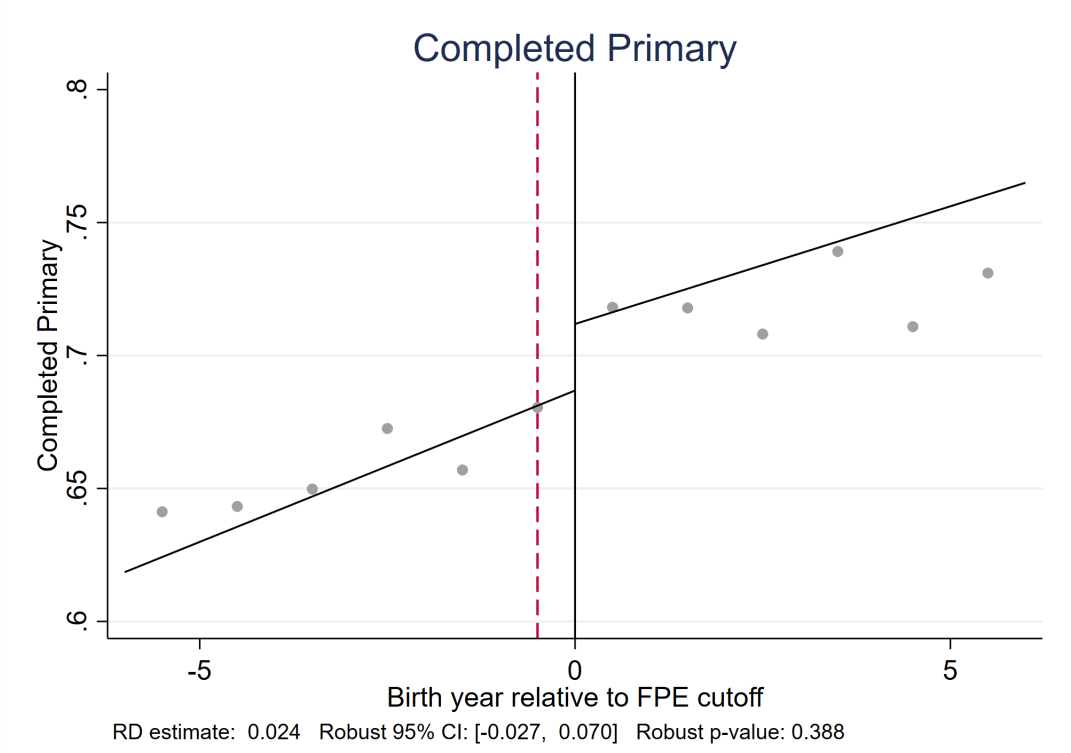
Note: Figure shows point estimates on the $Intensity * TreatedRound$ difference-in-differences term from each country-specific regression along with confidence interval whiskers.

Figure 8: Democratic support



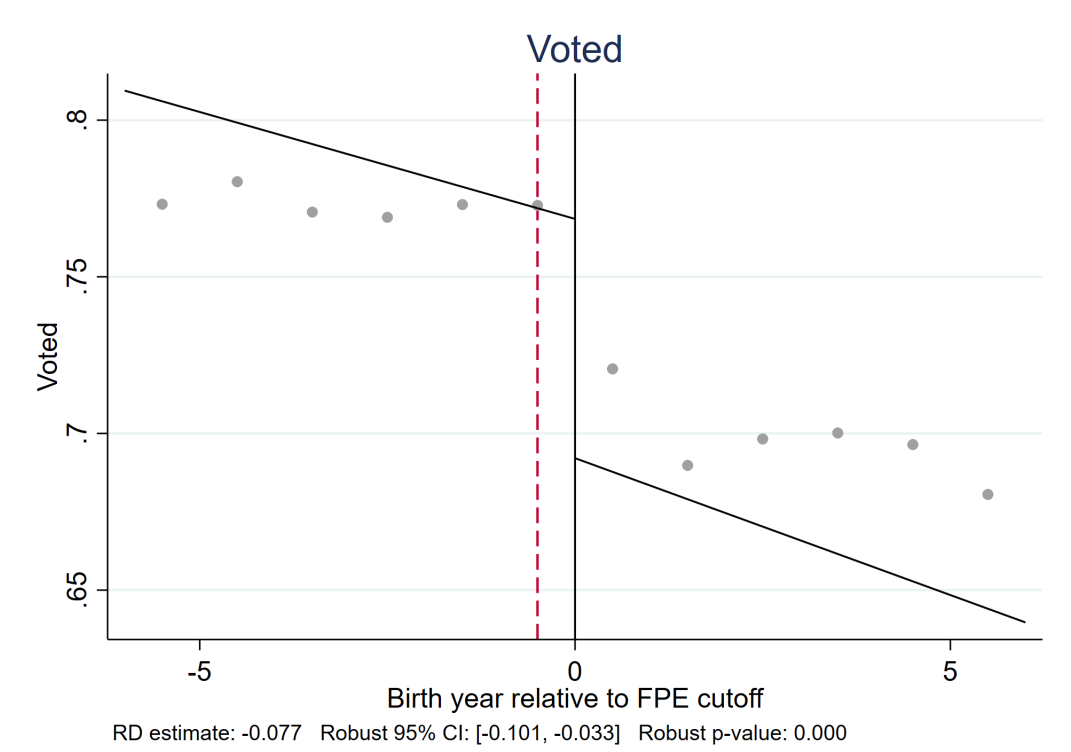
Note: Figure shows point estimates on the $Intensity * TreatedRound$ difference-in-differences term from each country-specific regression along with confidence interval whiskers.

Figure 9: Completed primary school: RD estimates



Note: Figure shows results from plotting the RD regression of completing at least primary education on birth year.

Figure 10: Voted Last: RD estimates



Note: Figure shows results from plotting the RD regression of voting in the last national election on birth year.

Appendix A: Additional Tables and Figures

Table A.1: Summary Statistics - Demographic Characteristics (Afrobarometer)

	N	Age	Avg. Intensity*	Female	Urban
	(1)	(2)	(3)	(4)	(5)
Benin	6,455	35.92	0.88	0.51	0.46
BFO	6,561	35.08	0.60	0.52	0.28
CVE	6,917	36.28	0.86	0.50	0.62
Ghana	12,298	35.55	0.80	0.52	0.53
Kenya	11,708	31.39	0.73	0.52	0.33
Lesotho	6,889	35.84	0.76	0.51	0.36
Liberia	6,731	33.60	0.73	0.50	0.49
Madagascar	5,822	37.88	0.85	0.51	0.22
Malawi	8,061	34.56	0.79	0.50	0.15
Mali	5,670	38.05	0.84	0.51	0.18
Mozambique	11,574	31.94	0.69	0.51	0.39
Nigeria	17,473	30.66	0.80	0.51	0.51
Senegal	8,139	35.06	0.79	0.53	0.48
Tanzania	13,099	33.98	0.74	0.52	0.37
Uganda	15,839	33.32	0.84	0.50	0.22
Zambia	7,661	31.56	0.76	0.51	0.42
Zimbabwe	8,147	32.95	0.59	0.51	0.38

Notes: Sample averages calculated for all individuals in all rounds. Average Intensity is calculated conditional on Intensity above 0.

Table A.2: Summary Statistics - Demographic Characteristics (DHS)

	N	Age	Avg. Intensity*	Female	Urban
	(1)	(2)	(3)	(4)	(5)
Ghana	38,575	31.27	0.85	0.51	0.53
Kenya	98,221	27.69	0.83	0.52	0.39
Nigeria	163,628	31.05	0.74	0.49	0.42
Tanzania	66,237	31.96	0.80	0.52	0.33
Zambia	65,752	32.12	0.72	0.51	0.45
Zimbabwe	28,342	26.87	0.67	0.55	0.41
Benin	38,028	33.78	0.87	0.50	0.43
Burkina Faso	41,510	31.06	0.71	0.51	0.32
Lesotho	30,938	31.01	0.84	0.52	0.35
Liberia	30,990	32.00	0.77	0.49	0.54
Madagascar	43,225	32.78	0.92	0.50	0.21
Malawi	48,177	32.50	0.76	0.52	0.19
Mali	58,862	32.88	0.91	0.48	0.28
Mozambique	41,956	30.65	0.81	0.53	0.38
Senegal	77,304	30.45	0.85	0.51	0.53
Uganda	40,907	31.76	0.76	0.51	0.21

Notes: N reports the unweighted number of observations. Age, average intensity, female, and urban are weighted using de-normalized DHS sampling weights. Average intensity is calculated among observations with positive FPE exposure intensity. The analysis sample follows the age and cohort restrictions used to construct FPE exposure. Where these restrictions extend beyond age 49, the sample is capped at 49 for both men and women, since 49 is the maximum age in the DHS female sample.

Table A.3: Effects of FPE on Educational attainment: Difference-in-differences estimates

	Completed Primary		Completed Secondary		Education	
	Mean	(1)	Mean	(2)	Mean	(3)
Pooled						
Intensity	0.633	0.02***	0.276	0.03***	2.309	0.10***
		(0.01)		(0.01)		(0.03)
CVE						
Intensity	0.663	0.07***	0.253	0.11***	2.469	0.27***
		(0.02)		(0.03)		(0.09)
Ghana						
Intensity	0.695	0.04*	0.323	0.11***	2.482	0.27***
		(0.02)		(0.02)		(0.05)
Senegal						
Intensity	0.412	0.12***	0.148	0.06***	1.542	0.40***
		(0.02)		(0.02)		(0.07)
Benin						
Intensity	0.395	0.05*	0.115	0.07***	1.512	0.24***
		(0.03)		(0.02)		(0.09)
Lesotho						
Intensity	0.671	-0.03	0.238	0.07	2.361	0.06
		(0.03)		(0.04)		(0.11)
Liberia						
Intensity	0.659	-0.03	0.355	0.02	2.546	-0.12
		(0.04)		(0.02)		(0.10)
BFO						
Intensity	0.315	-0.01	0.084	0.02	1.137	0.02
		(0.05)		(0.02)		(0.13)
Malawi						
Intensity	0.419	0.10**	0.135	0.03	1.758	0.27***
		(0.04)		(0.02)		(0.09)
Mali						
Intensity	0.216	0.10***	0.107	0.06***	0.945	0.33***
		(0.02)		(0.02)		(0.07)
Kenya						
Intensity	0.793	0.05***	0.453	0.06**	2.980	0.23***
		(0.01)		(0.02)		(0.05)
Nigeria						
Intensity	0.791	-0.03***	0.558	-0.00	3.109	-0.08**
		(0.01)		(0.01)		(0.04)
Zambia						
Intensity	0.763	0.06***	0.335	0.04**	2.807	0.20***
		(0.02)		(0.02)		(0.05)
Tanzania						
Intensity	0.811	0.03**	0.176	0.13***	2.231	0.41***
		(0.01)		(0.01)		(0.05)
Madagascar						
Intensity	0.580	0.05**	0.130	0.12***	2.069	0.36***
		(0.03)		(0.02)		(0.07)
Mozambique						
Intensity	0.572	0.00	0.192	0.00	2.073	-0.00
		(0.02)		(0.02)		(0.07)
Uganda						
Intensity	0.617	0.04**	0.227	0.03	2.311	0.13**
		(0.02)		(0.02)		(0.06)
Zimbabwe						
Intensity	0.866	0.01	0.444	0.00	3.142	0.01
		(0.04)		(0.07)		(0.19)

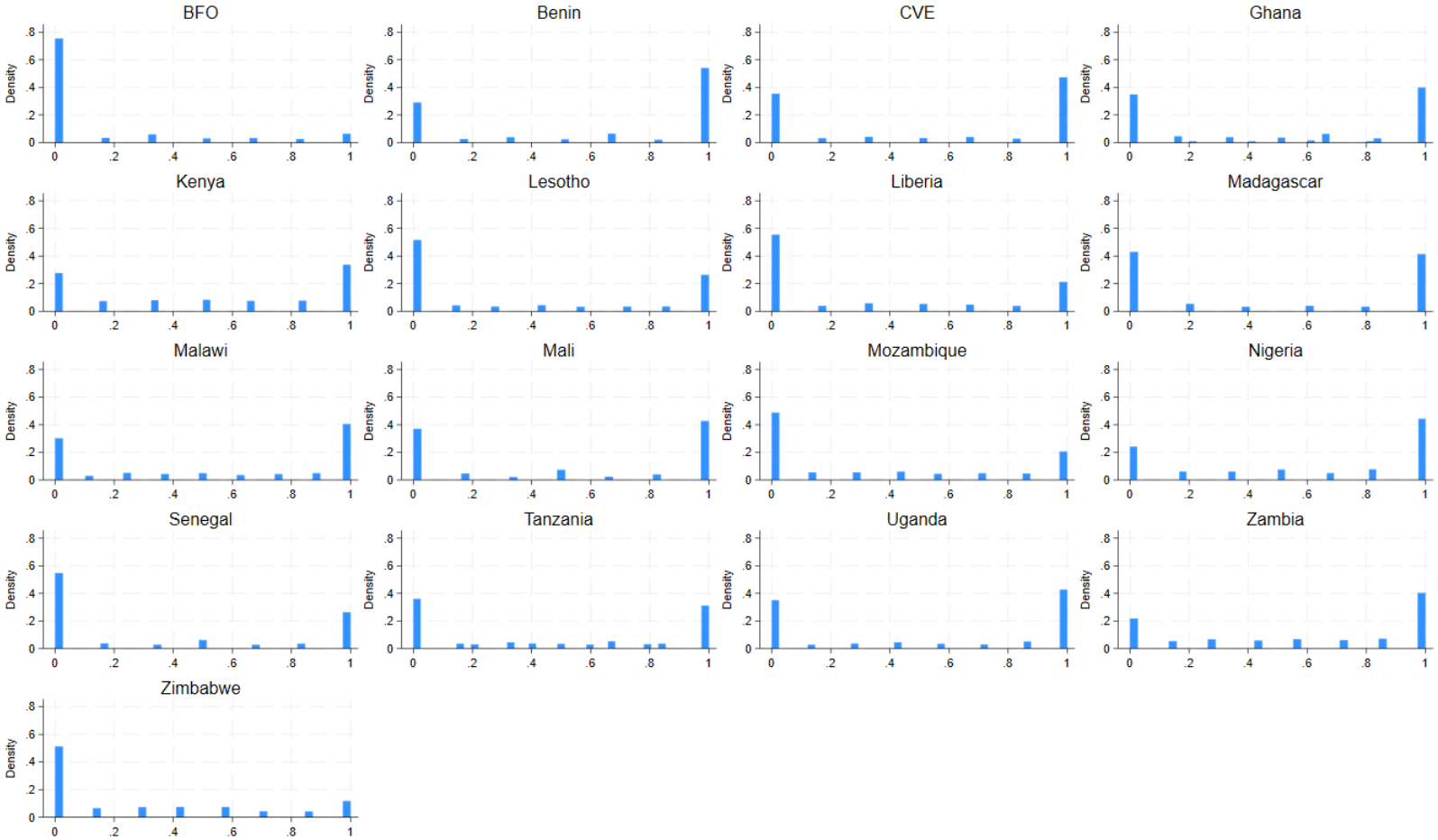
Notes: Standard errors clustered at the district level in parenthesis. Mean refers to the sample mean of the outcome in round 3. All estimations include round, age, female, district, religion and urban dummies. Not all surveyed individuals responded to every question, so the number of observations varies slightly across outcome variables. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Effects of the FPE on Political Action

	Voted		Pol Action Index		Pol Attitudes Index	
	Mean	(1)	Mean	(2)	Mean	(3)
Pooled						
Intensity	0.774	-0.02***	0.335	0.01***	0.781	0.02***
		(0.01)		(0.00)		(0.00)
CVE						
Intensity	0.793	-0.02	0.241	0.02	0.833	-0.02
		(0.02)		(0.01)		(0.02)
Ghana						
Intensity	0.838	-0.03**	0.273	-0.00	0.819	0.01
		(0.01)		(0.01)		(0.01)
Senegal						
Intensity	0.727	0.02	0.317	0.03**	0.857	0.01
		(0.02)		(0.01)		(0.01)
Benin						
Intensity	0.817	-0.04**	0.310	0.01	0.826	-0.01
		(0.02)		(0.02)		(0.01)
Lesotho						
Intensity	0.687	0.00	0.377	-0.03**	0.664	0.02
		(0.03)		(0.01)		(0.02)
Liberia						
Intensity	0.851	-0.04*	0.427	0.02	0.812	-0.01
		(0.02)		(0.02)		(0.01)
BFO						
Intensity	0.729	0.07*	0.345	0.00	0.689	0.01
		(0.04)		(0.02)		(0.02)
Malawi						
Intensity	0.825	-0.02	0.411	0.03*	0.759	0.03*
		(0.03)		(0.01)		(0.02)
Mali						
Intensity	0.714	-0.07**	0.310	-0.03	0.702	0.00
		(0.03)		(0.02)		(0.02)
Kenya						
Intensity	0.746	-0.02	0.346	0.02	0.842	0.03***
		(0.02)		(0.01)		(0.01)
Nigeria						
Intensity	0.750	-0.01	0.275	0.02**	0.779	0.00
		(0.01)		(0.01)		(0.01)
Zambia						
Intensity	0.686	0.06**	0.290	0.01	0.881	-0.01
		(0.03)		(0.01)		(0.01)
Tanzania						
Intensity	0.821	-0.02	0.363	0.01	0.815	0.02*
		(0.02)		(0.01)		(0.01)
Madagascar						
Intensity	0.768	-0.02	0.326	0.00	0.693	0.03*
		(0.02)		(0.01)		(0.02)
Uganda						
Intensity	0.832	-0.09***	0.419	-0.00	0.822	0.01
		(0.02)		(0.01)		(0.01)

: Standard errors clustered at the district level in parenthesis. Mean refers to the sample mean of the outcome in round 3. All estimations include round, age, female, district, religion and urban dummies. Not all surveyed individuals responded to every question, so the number of observations varies slightly across outcome variables. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A.1: Variation in FPE intensity across countries



Note: Each histogram shows the distribution of the treatment variable, FPE intensity, within each country.

Appendix B: Data and Variable construction

Table B.1: Free Primary Education Policies and Afrobarometer Coverage by Country

Country	Afrobarometer Rounds	Early FPE		Late FPE	Age Eligibility
		Start Year	End Year	Legal Start Year	
Ghana	2 (2002), 3 (2005), 4 (2008), 5 (2012) 6 (2014), 7 (2017), 8 (2019), 9 (2022)	1961	1965	1995	6–11
Kenya	2 (2003), 3 (2005), 4 (2008), 5 (2011) 6 (2014), 7 (2016), 8 (2019), 9 (2021)	1974	1988	2003	6–11
Tanzania	1 (2001), 2 (2003), 3 (2005), 4 (2008) 5 (2012), 6 (2014), 7 (2017), 8 (2021), 9 (2022)	1974	1978	2001	7–12
Zambia	2 (2003), 3 (2005), 4 (2009), 5 (2013) 6 (2014), 7 (2017), 8 (2020), 9 (2022)	1965	1980	2002	7–13
Malawi	2 (2003), 3 (2005), 4 (2008), 5 (2012) 6 (2014), 7 (2016), 8 (2019), 9 (2022)	—	—	1994	6–13
Uganda	1 (2000), 2 (2002), 3 (2005), 4 (2008) 5 (2012), 6 (2015), 7 (2016), 8 (2019), 9 (2022)	—	—	1997	6–12
Benin	3 (2005), 4 (2008), 5 (2011), 6 (2014) 7 (2017), 8 (2020), 9 (2022)	—	—	1990	6–11
Burkina Faso	4 (2008), 5 (2012), 6 (2015) 7 (2017), 8 (2019), 9 (2022)	—	—	2007	6–11
Cape Verde	2 (2002), 3 (2005), 4 (2008), 5 (2011) 6 (2014), 7 (2017), 8 (2019), 9 (2022)	—	—	1992	6–11
Lesotho	2 (2003), 3 (2005), 4 (2008), 5 (2012) 6 (2014), 7 (2017), 8 (2020), 9 (2022)	—	—	2000	6–12
Liberia	4 (2008), 5 (2012), 6 (2015) 7 (2018), 8 (2020), 9 (2022)	—	—	2002	6–11
Madagascar	3 (2005), 4 (2008), 5 (2013) 6 (2015), 7 (2018), 9 (2022)	—	—	1992	6–10
Mali	2 (2002), 4 (2008), 5 (2013), 6 (2014) 7 (2017), 8 (2020), 9 (2022)	—	—	1992	7–12
Mozambique	2 (2002), 3 (2005), 4 (2008), 5 (2012) 6 (2015), 7 (2018), 8 (2021), 9 (2022)	—	—	2004	6–12
Nigeria	1 (1999), 2 (2003), 3 (2005), 4 (2008) 5 (2012), 6 (2014), 7 (2017), 8 (2020), 9 (2022)	1974	1981	1999	6–11
Senegal	2 (2002), 3 (2005), 4 (2008), 5 (2013) 6 (2014), 7 (2017), 8 (2021), 9 (2022)	—	—	2001	6–11
Zimbabwe	2 (2004), 3 (2005), 4 (2009), 5 (2012) 6 (2014), 7 (2017), 8 (2021), 9 (2022)	1980	1991	2013	6–12

Note: '—' indicates that countries did not have an early wave of FPE. Start years refer to the legal start years of both early and late waves of FPE. Age eligibility refers to the age ranges that were eligible for FPE.

Table B.2: Free Primary Education Policies and DHS Coverage by Country

Country	DHS Survey Years	Early FPE		Late FPE Legal Start Year	Age Eligibility	Early FPE Analysis	Late FPE Analysis
		Start Year	End Year				
Ghana	2003, 2008, 2014, 2022	1961	1965	1995	6–11	T: 2003, 2008 C: 2014, 2022	C: 2003, 2008 T: 2014, 2022
Kenya	2003, 2008–09, 2014, 2022	1974	1988	2003	6–11	T: 2003, 2008–09 C: 2014, 2022	C: 2003, 2008–09 T: 2014, 2022
Tanzania	2004–05, 2010, 2015–16, 2022	1974	1978	2001	7–12	T: 2004–05, 2010 C: 2015–16, 2022	C: 2004–05, 2010 T: 2015–16, 2022
Zambia	2007, 2013–14, 2018, 2024	1965	1980	2002	7–13	T: 2007, 2013–14 C: 2018, 2024	C: 2007, 2013–14 T: 2018, 2024
Malawi	2004, 2010, 2015–16, 2024	—	—	1994	6–13	—	C: 2004, 2010 T: 2015–16, 2024
Uganda	2000–01, 2006, 2011, 2016	—	—	1997	6–12	—	C: 2000–01, 2006, 2011 T: 2016
Benin	2006, 2011–12, 2017–18	—	—	1990	6–11	—	C: 2006, 2011–12 T: 2017–18
Burkina Faso	2010, 2021	—	—	2007	6–11	—	C: 2010 T: 2021
Lesotho	2004, 2009, 2014, 2023–24	—	—	2000	6–12	—	C: 2004, 2009 T: 2014, 2023–24
Liberia	2007, 2013, 2019–20	—	—	2002	6–11	—	C: 2007, 2013 T: 2019–20
Madagascar	2003–04, 2008–09, 2021	—	—	1992	6–10	—	C: 2003–04, 2008–09 T: 2021
Mali	2001, 2006, 2012–13, 2018, 2023–24	—	—	1992	7–12	—	C: 2001, 2006, 2012–13 T: 2018, 2023–24
Mozambique	2003, 2011, 2022–23	—	—	2004	6–12	—	C: 2003, 2011 T: 2022–23
Nigeria	2003, 2008, 2013, 2018, 2024	1974	1981	1999	6–11	T: 2003, 2008, 2013 C: 2018, 2024	C: 2003, 2008, 2013 T: 2018, 2024
Senegal	2005, 2010–11, 2017, 2019, 2023	—	—	2001	6–11	—	C: 2005, 2012–13 T: 2017, 2019, 2023
Zimbabwe	2005–06, 2010–11, 2015	1980	1991	2013	6–12	T: 2005–06, 2010–11 C: 2015	C: 2005–06, 2010–11 T: 2015

Notes: The table reports the DHS surveys selected for each country to correspond as closely as possible to the Afrobarometer survey years used in the main analysis. DHS surveys conducted through 2013 are classified as corresponding to the Afrobarometer Rounds 1–5 period, while DHS surveys beginning in 2014 or later are classified as corresponding to the Rounds 6–9 period. Surveys beginning in 2013, including surveys spanning 2013–14, are assigned to the Rounds 1–5 period. **C** denotes control surveys and **T** denotes treated surveys. For countries with Late FPE reform, Rounds 1–5 period are classified as controls and Rounds 6–9 period are classified as treated. For countries with both Early and Late FPE reforms, the assignment is reversed across the two analyses.

B.1 Afrobarometer Data

We used data from Afrobarometer, a pan-African, non-partisan research initiative that conducts nationally representative surveys on political, economic, and social attitudes across African countries. The surveys are designed to capture the views of adult citizens on a range of issues, including governance, democracy, public service delivery, corruption, and civil liberties.

Since its inception in 1999, Afrobarometer has completed nine rounds of cross-national surveys, with Round 10 implemented in some countries during 2024–2025. Round 1 (1999–2001) covered 12 countries, and subsequent rounds progressively expanded to include 39 countries by Round 9 (2021–2023). Each round follows a standardized methodology based on stratified multistage cluster sampling. Within each country, samples are drawn to be nationally representative of the adult population (aged 18 and over), typically comprising between 1,200 and 2,400 respondents.

The sampling strategy involves the selection of enumeration areas (EAs) as primary sampling units, typically stratified by region and urban/rural residence. Within each selected EA, eight households are systematically chosen. A random starting point is first identified, and from this point, enumerators select the remaining households using a random walk procedure. In each selected household, one adult respondent is randomly chosen using computer-assisted personal interviewing (CAPI) software. To ensure gender balance, interviewers alternate between selecting male and female respondents across households. From Round 4 onward, weighting procedures were standardized across countries using post-stratification weights to adjust for deviations in population demographics by gender, age, and location. In Round 8 and subsequent rounds, weighting protocols further incorporated household-level adult population estimates.

All Afrobarometer data are publicly available and can be accessed through the project's official data portal: https://www.afrobarometer.org.

B.2 Weight Construction Across Rounds

Afrobarometer applies post-stratification weights to ensure national representativeness of its survey samples, correcting for unequal selection probabilities introduced by the sampling design. The construction of individual-level weights evolved over time, particularly after Round 3 (Afrobarometer, 2021, 2022).

Rounds 1–3. In the early survey rounds, Afrobarometer did not provide standardized sampling weights. While samples were designed to be nationally representative, these rounds

lack consistent post-stratification adjustments. Weighting in these rounds uses the area of location to correct for over- and underrepresentation of certain populations. In these rounds, EA and stratum population were not collected (Afrobarometer, 2021).

Rounds 4–7. Beginning in Round 4, Afrobarometer implemented a unified weighting strategy based on individual selection probabilities derived from the survey's multistage design. The probability of selection P_i for respondent i depends on whether the respondent resides in an urban or rural area, since rural areas involve an additional stage of sampling through Secondary Sampling Units (SSUs) (Afrobarometer, 2021, 2022).

For urban respondents:

$$P_i = P_t \cdot P_{\text{EA}} \cdot P_{\text{STA}}, \tag{5}$$

For rural respondents:

$$P_i = P_t \cdot P_{\text{EA}} \cdot P_{\text{SSU}} \cdot P_{\text{STA}}, \tag{6}$$

where:

- $P_t = \frac{n}{N}$ is the overall country sampling fraction, with n the sample size of data collected by Afrobarometer and N the national adult population, mostly from the most recent census of that country before a particular Afrobarometer round survey,
- P_{EA} is the probability of selecting an individual/household within a given enumeration area (8 interviews per EA),
- P_{SSU} is the probability of selecting an SSU (rural only),
- P_{STA} is the probability of selecting an EA within a stratum (e.g., region × urban/rural).

The within-country weight is computed as the inverse of the total selection probability:

$$\texttt{WITHINWT}_i = \begin{cases} \frac{1}{P_t \cdot P_{\text{EA}} \cdot P_{\text{STA}}}, & \text{if urban,} \\ \frac{1}{P_t \cdot P_{\text{EA}} \cdot P_{\text{SSU}} \cdot P_{\text{STA}}}, & \text{if rural.} \end{cases} \tag{7}$$

Round 8 and Beyond. Starting in Round 8, Afrobarometer refined the weighting approach by incorporating household-level corrections based on the number of adults in the respondent's household. This adjustment accounts for the unequal probability of selecting a respondent from households of varying size (Afrobarometer, 2022). Let A_{h_i} denote the number of adult citizens in respondent i's household. The household-adjusted weight is then:

$$\texttt{WITHINWT_HH}_i = \begin{cases} \frac{A_{h_i}}{P_t \cdot P_{\text{EA}} \cdot P_{\text{STA}}}, & \text{if urban,} \\ \frac{A_{h_i}}{P_t \cdot P_{\text{EA}} \cdot P_{\text{SSU}} \cdot P_{\text{STA}}}, & \text{if rural.} \end{cases} \tag{8}$$

This final weight, identified in the dataset as `withinwt_hh`, is recommended for all descriptive and inferential analyses. In this round and subsequent rounds, the variable previously labeled `withinwt` is now identified as `withinwt_ea`.

B.3 Comparison of Afrobarometer with Population Census Data

To assess how well Afrobarometer reflects key national trends, we compare its estimates with those from national population census data obtained from the Integrated Public Use Microdata Series (IPUMS) International, using the two most recent census years for each country. We focus on two indicators: the share of the population living in urban areas and educational attainment, specifically the completion of at least primary and secondary education. Our analysis centers on Ghana and Tanzania, examining whether the gap between Afrobarometer and census data widens as more time passes since the last census.

To do so, we first merge the census and Afrobarometer datasets. In preparation for the merge, we categorize the Afrobarometer data into two groups based on the survey year: 2000s (if the survey year is before 2010) and 2010s (if the survey year is 2010 or later). We generate a corresponding group-year variable in both the Afrobarometer and census datasets. The two most recent censuses, as our time of collecting the data from IPUMS, are between 2000–2009 and 2010–2019, respectively.

We then merge the datasets using three variables: Country, the variable in long format containing our indicators, and the newly created Group-Year. After merging, we compute the difference in means between Afrobarometer and census estimates for each indicator and regress these differences on the number of years since the most recent census, calculated as the Afrobarometer survey year minus the census year. The estimated coefficients from these regressions indicate whether Afrobarometer systematically overstates or understates each indicator as the time since the last census increases.

B.3.1 Urban Share

We looked at how closely Afrobarometer matches census data on the share of people living in urban areas. In Ghana, we found that the further a survey is from the last census, the more Afrobarometer tends to overestimate the urban population by about 1 percentage point per year, a statistically significant difference. In Tanzania, the pattern is similar but weaker and not statistically significant. These suggest that discrepancies in urban share estimates seem to get worse the further out from the last census. Including wave fixed effects may help account for national-level changes over time.

Table B.3: Urban Share

	Ghana	Tanzania
	(1)	(2)
Years Since Census	0.010**	0.006
	(0.004)	(0.007)

Notes: Robust standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.3.2 Education

As part of our effort to understand how well Afrobarometer captures real trends in education over time, we compared its estimates of primary and secondary school completion to national census data from Ghana and Tanzania. In Ghana, Afrobarometer estimates for primary completion tend to rise slightly above census figures over time by about 0.5 percentage points per year, while in Tanzania, the estimates drift slightly downward by 0.3 percentage points per year. Neither of these trends is statistically significant. However, when we look at secondary completion, the gap between Afrobarometer and census data grows significantly in Ghana, by about 1.5 percentage points per year, suggesting that Afrobarometer increasingly overstates secondary completion the further we get from the last census. In Tanzania, the pattern is again small and not statistically significant.

Although Afrobarometer data reports education across multiple detailed categories, including no schooling, incomplete and complete primary, incomplete and complete secondary, and university, its structure still emphasizes educational thresholds. This makes it well suited for identifying extensive-margin effects, such as whether a policy increases the number of individuals who at least complete primary or secondary education. At the same time, the inclusion of incomplete categories also allows for some insight into intensive-margin changes,

like improvements in partial attainment. These features make Afrobarometer data a useful source for evaluating first-stage effects of policy on educational outcomes.

Table B.4: Educational Attainment: Atleast Primary and Secondary Completion

	Atleast primary completed		Atleast secondary completed	
	Ghana	Tanzania	Ghana	Tanzania
	(1)	(2)	(3)	(4)
Years Since Census	0.005	-0.003	0.015**	0.002
	(0.006)	(0.004)	(0.004)	(0.003)

Notes: Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.4 DHS data

DHS data are publicly available from https://dhsprogram.com/data/, upon registration. We obtained DHS data for 16 of our 17 countries (no data exist for Cabo Verde). We chose years closest to the years of Afrobarometer that are used in our main analysis. The DHS contain most of the variables we need to implement our empirical strategy, in harmonized form: age/year of birth, self-reported literacy, gender, location/subdistrict of residence, urban/rural, religion categories and DHS round dummies. We also used the DHS weights.

For including location fixed effects, we needed to create a crosswalk between the DHS clusters and Afrobarometer districts/locations. For each country and wave, we used the DHS GPS shapefiles and matched them into the most recent administrative boundaries available for each country. This way, each cluster in each wave was mapped to a fixed set of updated administrative boundaries in that country. We use these district boundaries to include fixed effects and cluster standard errors at the location level.

We age-restricted both male and female DHS samples at 49, since the female sample only goes up to 49. This differs from our sample in the Afrobarometer.

For Tanzania, there is no religion data for at least one of the DHS rounds. We retain Tanzania in our analyses but exclude religion fixed effects in the country-level analysis. For the pooled analysis of literacy, we absorb the religion dummies (which are all set to zero, except for one which is set to one for all individuals) into the Tanzania fixed effect.

The DHS Sampling Manual recommends de-normalizing weights for pooled analyses involving more than one survey and does not restrict this recommendation to pooling surveys within the same country. We therefore de-normalize the country-specific weights in the following way: we downloaded single-age group population data from the UN Population

Division to construct the relevant target populations: women aged 15-49 and men aged 15-49, 15-54, 15-59, or 15-64, depending on the upper age bound of the male sample in each DHS survey country. Then we normalize the individual-level weights to reconstruct the population totals. (We apply the common age restriction up to age 49 later before our analyses).

C. Appendix: Some comments on Nigeria and Zimbabwe

In two important papers, Croke et al. (2016) and Larreguy and Marshall (2017) examine the impact of FPE on schooling outcomes, and on political participation in Zimbabwe and Nigeria respectively. This appendix discusses why we find somewhat different results to these papers.

C.1 Nigeria

In our main analysis of the Afrobarometer data, we find that FPE exposure has a negative impact on primary school completion and on the educational attainment variable for Nigeria, while in the DHS data, we find that exposure to FPE raises literacy in adulthood.

In contrast, Larreguy and Marshall (2017) use Afrobarometer data to find that FPE exposure raises educational attainment and raises political engagement in Nigeria.

Although we use the same data, our empirical strategies differ in ways that affect outcomes of educational attainment. Larreguy and Marshall (2017) consider all of the cohorts born after the introduction of FPE (1974) as eligible for free schooling. They treat this early wave of FPE as the only FPE policy that was always in place and hence never ended.

Our measure of FPE intensity imposes that this early wave of FPE did end. According to **?**, FPE was phased out in 1981-1983. Our Intensity measure excludes individuals who were eligible to go to primary school after the end of this early FPE period.

Which measure is preferable depends on how one thinks about what the treatment is. In Larreguy and Marshall (2017), the introduction of the FPE policy is a key way to capture changing access to schooling. To the extent that school infrastructure was built as a result of the policy, and those facilities remained even when tuition was reintroduced, students could therefore continue to attend primary school, at a cost, post-FPE.

In our paper, the treatment is the number of age-eligible years that a child could have attended primary school without tuition costs. Hence, once FPE turns off, the treatment goes to zero. Our Intensity measure used in both the Afrobarometer and DHS data capture this variation.

The negative impacts we find on educational attainment and positive impacts on literacy can be reconciled by thinking about what treatment regimes students in our control group were facing. The control group in our Intensity measure includes older adults who were school-age elibile in the period after the end of early FPE (1974-1983) and before late FPE (2004). These older adults would have had better physical access to schools, which

could contribute to their higher educational attainment. However, with shrinking resources available for maintaining school quality, this enhanced access did not translate into learning – individuals treated during the "formal" FPE periods still have better literacy outcomes.

C.2 Zimbabwe

In our main analysis of the Afrobarometer data, we find that FPE exposure (for policies begun in 1980) has no significant impacts on human capital in Zimbabwe and a negative impact on literacy outcomes for those most exposed to FPE.

Croke et al. (2016) show that the initiation of education reforms, including FPE, in 1980 had little to no effect on primary school attainment, partly because primary school enrollment was already at 80% among black Zimbabweans. But their donut-hole RD specification shows that the reforms significantly raise secondary school enrollment and high school completion. The paper notes that since the reform was implemented so fast, school quality suffered, and many classrooms were overcrowded. This may account for our negative impacts on literacy.