THE EFFECT OF EDUCATION ON CIVIC AND POLITICAL ENGAGEMENT IN NONCONSOLIDATED DEMOCRACIES: EVIDENCE FROM NIGERIA

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Abstract—Developing democracies are experiencing unprecedented increases in primary and secondary schooling. To identify education’s long-run political effects, we use a difference-in-differences design that leverages variation across local government areas and gender in the intensity of Nigeria’s 1976 universal primary education reform—one of Africa’s largest ever educational expansions—to instrument for education. We find large increases in basic civic and political engagement: better educated citizens are more attentive to politics, more likely to vote, and more involved in community associations. The effects are largest among minority groups and in fractionalized areas, without increasing support for political violence or own-group identification.

I. Introduction

DEVELOPING democracies are typically among the world’s most corrupt and politically unequal. Many lack the effective political institutions required to support democratic consolidation and economic development (Acemoglu & Robinson, 2006). However, establishing and sustaining such institutions is not easy to achieve, especially in contexts where civic and political engagement are low. Interventions have consistently found that community-driven development programs struggle to encourage broad-based political participation (Casey, Glennerster, & Miguel, 2012; Gugerty & Kremer, 2008; Humphreys, Sanchez de la Sierra, & van der Windt, 2012), and civic education programs have produced mixed results (Finkel & Smith, 2011; Finkel, Horowitz, & Rojo-Mendoza, 2012). Similarly, political information interventions across the developing world have generally failed to increase political engagement (see Lieberman, Posner, & Tsai, 2014).

Although the Millennium Development Goals (MDGs) are primarily concerned with eradicating poverty, the push for universal primary education (UPE) and equal access to secondary education could simultaneously empower politically weak and disengaged citizens in developing democracies. Primary school enrollment in sub-Saharan Africa rose from 59% in 1999 to 78% in 2011, while secondary school enrollment rose from 26% to 41% over the same period (UNESCO, 2014). These newly educated generations might be crucial for democratic consolidation, particularly as many of sub-Saharan Africa’s nascent democracies are failing to consolidate (Opalo, 2012) or hold governments to account beyond the voting booth (Bratton & Logan, 2006), while reducing educational inequalities could reduce political inequalities.

The optimistic modernization literature suggests that education lays the groundwork for democratic consolidation (e.g., Dahl, 1971; Glaeser, Ponzetto, & Shleifer, 2007; Lipset, 1959). However, the causal interpretation of the positive cross-country correlation between education and democracy remains disputed (e.g., Acemoglu et al., 2005; Campante & Chor, 2012a; Glaeser et al., 2004). Recent studies have sought to address the difficulty of identifying the political effects of education by exploiting individual-level variation. In particular, studies in consolidated democracies have found that primary and secondary education can substantially increase political participation (Dee, 2004; Miller, Moretti, & Oreopoulos, 2004; Sondheimer & Green, 2010).

However, education’s participatory effects in nonconsolidated democracies—polities that hold relatively fair and competitive multiparty elections but also experience unequal political representation and limited elite competition—could be significantly less benign than in consolidated democracies. If increases in political voice and economic opportunities for the newly educated do not accompany educational expansion (Huntington, 1968) or if the benefits of the reform do not reach the politically disadvantaged (Casey, Glennerster, & Miguel, 2012; Gugerty & Kremer, 2008), then increased education could accentuate institutional capture (Bardhan, 2002; Gugerty & Kremer, 2008) and support for political violence (Friedman et al., 2016). This could ultimately lead to violent uprisings and democratic breakdown (Campante & Chor, 2012b; Huntington, 1968). These concerns are especially pertinent where local ethnic and religious divisions are salient (Horowitz, 1985).

Although surveys in developing countries provide unprecedented opportunities to examine civic and political behavior, previous research examining the effects of education on political and community engagement has been stymied by selection bias concerns (see Kam &...
II. Politics and Primary Education in Nigeria

Despite its political instability, Nigeria has experimented with some of Africa’s most ambitious nationwide education reforms. In 1976, Nigeria implemented its Universal Primary Education (UPE) program, providing six tuition-free years of primary education to all 6-year-olds. This increased primary enrollment by more than 7 million by 1981 (Osili & Long, 2008; Oyelere, 2010), which in turn substantially increased secondary involvement. Using Afrobarometer survey data from 1999 to 2013, we exploit variation in the intensity of the UPE reform, captured by variation in prereform primary school enrollment rates, across local government areas (LGAs) and by gender using a difference-in-differences strategy to instrument for an individual’s level of education (see also Bleakley, 2010; Dufo, 2001).

Our reduced-form and instrumental variables (IV) estimates show that Nigeria’s UPE program substantially increased civic and political engagement, up to 37 years after individuals were educated, for a large group of citizens who would have remained essentially uneducated without UPE. In particular, education significantly increases interest in politics, political knowledge, and basic forms of political participation, including voting and contacting local government councilors. However, such an increase in education does not cultivate more costly forms of political activity like contacting national-level representatives or participation in peaceful political demonstrations. Furthermore, the education induced by the UPE reform substantially increases the likelihood of attending community meetings and actively participating in local associations.

A range of checks validate our identifying assumptions and demonstrate robustness to alternative operationalization choices. Supporting our parallel trends assumption, our results are robust to including LGA-specific cohort trends, while placebo tests indicate no differential pretrends by UPE intensity. Furthermore, we present several checks suggesting that our results are unlikely to be explained by selective migration. We support the exclusion restriction by also showing that the UPE reform did not affect those too old to be eligible for additional schooling. Finally, we obtain similar results using alternative measures of reform intensity and when classifying older students already eligible to attend primary school in 1976 as partially treated.

Aggregating the reduced-form estimates suggests that the political impact of UPE in Nigeria may have been considerable. The reform increased turnout by 3 percentage points (or by 5% of total turnout), community meeting attendance by 4 percentage points (or by 14% relative to the sample average), and frequent discussion of politics by 3 percentage points (or by 14% relative to the sample average). Beyond its economic and social benefits, these large political effects of a major universal primary education reform suggest that fulfilling this MDG has the capacity to significantly increase informed political participation.

We find no evidence that participation increases institutional capture by the locally dominant ethnic or religious groups, own-group identification, or support for violence. Rather, the effects of education are greatest in relatively religiously fragmented areas and among religious minorities, while there is no indication that education increases support for violence, ethnic identification, or segregated participation. Our results thus suggest that publicly provided primary education fosters prodemocratic civic and political engagement, even in nonconsolidated democracies.

Nigeria contains one-fifth of sub-Saharan Africa’s population, features considerable ethnoreligious diversity, and, despite experiencing relatively contested elections, has struggled with democratic consolidation. The external validity of this quasi-experiment is thus unusually high and offers hope to reformers supporting both increased educational enrollment and prodemocratic participation. However, given recent evidence that education stimulated disengagement in Zimbabwe until elections became more competitive in 2008 (Croke et al., 2016)—albeit still far below the levels of competition in our Nigerian sample—it seems that education’s proparticipation effects may materialize only when politics is sufficiently competitive for voters to be able to meaningfully express their preferences.

The paper proceeds as follows. Section II provides an overview of education policies in Nigeria, focusing particularly on the 1976 UPE reform. Section III details the data and empirical strategy. Section IV shows our main results and robustness checks. Section V concludes.

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1 Cross-national correlations suggest that public schooling increases turnout, voter registration, protest, and contacting political leaders (e.g., Campante & Chor, 2012a; MacLean, 2011). Donor-sponsored programs providing less conventional educational opportunities are similarly correlated with increased political knowledge and local-level participation, but they show mixed associations with behavioral outcomes like voting (Bratton et al., 1999; Finkel & Ernst, 2005; Finkel et al., 2012; Finkel & Smith, 2011).

2 Existing work examines the effects of higher levels of education on participation in electoral authoritarian regimes (Croke et al., 2016) or on involvement in electoral campaigns (Wanichtekon, Klašnja, & Novta, 2015).
policies. This section provides a brief historical overview of Nigerian politics and education provision, before detailing the 1976 educational reform underpinning our identification strategy.

A. Social and Political Context

Africa’s most populous nation, containing 162.5 million people in 2011, Nigeria is a major oil and gas producer but ranks low in terms of GDP per capita. It is also one of the continent’s most ethnically, linguistically, and religiously diverse nations, containing more than 300 ethnic and linguistic groups. Religious and ethnic divisions remain the basis of violent political conflict (Adesoji, 2010; Lewis, 2006), and are sufficiently contentious that the 2016 Census is the first since independence to ask about religion or ethnicity.

Following independence in 1960, Nigeria has oscillated between military and democratic rule, experiencing ten military coups. However, federal elections for the presidency, Senate, and House have been held regularly since 1999. Polity IV rated Nigeria’s democracy as 4 in 2014 (on a scale from –10 to 10), rising from –6 in the 1990s. Consistent with this rating, democratic institutions are relatively fragile, and voter demand for political accountability remains low (Bratton & Logan, 2006). Moreover, the People’s Democratic Party (PDP) retained the presidency and legislative majorities until 2015, when the All Progressives Congress (APC) party won the presidency for the first time. Although recent elections have generally been fair, electoral campaigns nevertheless have featured vote buying, polling irregularities, and violence (Bratton, 2008; Collier & Vicente, 2014).

Nigeria’s government is divided across the federal government: 36 states and the capital Abuja, and 774 local governments. The federal, state, and local governments share national revenues and policy responsibilities. Local governments administer LGAs and are run by an elected chairman and local councilors. The first 301 LGAs were established in 1976. Using representative LGA survey data from 2001; Oyefusi, 2008). Furthermore, the costs of running for local office often ensure that politics is dominated by “political godfathers”—powerful and violent economic and political elites controlling state institutions—who install local politicians who depend on their patronage for reelection (Albin-Lackey & Rawlence, 2007).

B. Pre-1976 Education

Prior to independence, Great Britain divided Nigeria into three semiautonomous administrative regions: the predominantly Muslim North, Christian East, and mixed West regions. European-style education was introduced under colonial rule in the 1840s but was provided by Christian missionaries supported financially by the British government (Fafunwa, 1974). While the western and especially eastern regions were relatively densely populated with missions, the North was poorly served because the British government was reluctant to incite local religious leaders by interfering with Islamic practices prohibiting Western-style education (Achor, 1977; Csapo, 1983; Fafunwa, 1974; Osili & Long, 2008).

Missions served as the primary source of education until governments began implementing universal education in the 1950s. In 1955, the western region implemented a program of free six-year universal primary education. After doubling enrollment within a year (Csapo, 1983), this was extended to Lagos and the East in 1957 with similarly dramatic enrollment increases (Abernethy, 1969), and the North in 1958 (Bray, 1981; Fafunwa, 1974). However, these programs varied considerably in the length of education provided, how they were financed, and their success in enrolling students (Bray, 1981). The East experienced severe financial problems, lacked trained teachers, and faced considerable opposition from the local-majority Catholic church (Achor, 1977). Compounding preexisting educational disparities, enrollment in the North remained low due to traditional attitudes toward women (Csapo, 1983) and low funding (Achor, 1977). After independence, most primary education programs were reduced, with Nigeria’s newly designed regions differing in their willingness to fund education (Osili & Long, 2008; Oyelere, 2010).

Despite frequent shifts between military and democratic government, civil society and local politics in Nigeria are relatively vibrant. A large number of nongovernmental labor, professional, and religious associations actively engage in politics and supported the return to democracy (Ikelegbe, 2001; Lewis, 2006). However, participation is not equal across the population, and such groups do not always promote good governance. Political engagement is typically concentrated among Nigeria’s professional and educated middle class (Ikelegbe, 2001), while the internal disorganization of civil groups has allowed some to be co-opted by militant extremists emphasizing ethnic divisions (Ikelegbe, 2001; Oyefusi, 2008).
Buoyed by its oil revenue boom, Nigeria’s postindependence military federal government led by Olusegun Obasanjo announced in 1974 “one of the most ambitious education projects in African history” (Bray, 1981, 1). Starting September 1, 1976, the government implemented its nationwide UPE program, providing 6 years of free primary education starting from six years of age for all students. UPE targeted 100% primary enrollment by 1981 (Csapo, 1983). The government set seven grand objectives for the UPE curriculum: inculcating literacy, numeracy, and communication; a sound basis for effective thinking; citizenship education; character and moral training; developing adaptability; skills to function in the local community; and preparation for further educational enhancement (Achor, 1977).

Significant government investment, especially in classroom capacity, teacher training, and teaching equipment, was required to implement UPE. Investments varied substantially across the country and were determined at the state level according to the investment required to reach 100% enrollment. Osili and Long (2008) show that federal budgetary allocations for primary school construction across states, totaling 700 million naira, reflected differential prior enrollment, with per capita funding disproportionately distributed to eastern and, particularly, northern states. This funding intended to construct 150,995 new classrooms by 1980, of which 106,505 were to be built in the North (Csapo 1983), in addition to 80,000 new teachers and 6,699 new classrooms for teacher training (Nwachukwu, 1985). Figure 2 shows that public school construction spiked around 1976 to accommodate UPE, while private school construction was relatively unaffected.

Despite efforts to increase formal teacher training, understaffing and unqualified teachers were initially prevalent, especially where the required investments were greatest. In 1977, there was only one teacher per 41 pupils and only 60% of teachers were qualified in even the best-performing states (Asagwara, 1997). However, continuing investments substantially increased teacher supply and the proportion of qualified teachers in all states by the early 1980s (Asagwara, 1997).

The reform resulted in a dramatic increase in enrollment. The number of students in primary school rose by 214% from 4.4 million in 1974 to 13.8 million in 1981 (Osili & Long, 2008; Oyelere, 2010). This exceeded government expectations based on the 1963 Census of 11.5 million in 1980.
(Bray, 1981; Csapo, 1983). Given the huge disparities, differences were also pronounced across regions, with the largest enrollment increases in the North. Nevertheless, as many as 25% of students in Benue and Plateau dropped out before completing primary school after UPE’s introduction (Csapo, 1983); such incomplete primary schooling is reflected in our empirical analysis.

Due to a decline in oil revenues, the civilian government handed power in 1979 decided to end the UPE program in 1981. Although universal education remained a goal, most states then reintroduced school fees—excluding the western states, dominated by the United Nigeria Party—as the federal government ceased to provide grants for teacher salaries and training (Osili & Long, 2008). As figure 4 shows below, enrollment barely changed, and with time, it continued to increase. This suggests that school availability and better inputs, rather than fees, principally drove later enrollment decisions. Supporting this claim, Ozigi and Ocho (1981) find that the experience of UPE raised northern parents’ willingness to pay for schooling.

III. Data and Empirical Strategy

This section first describes the Nigerian survey data containing political responses, before detailing our difference-in-differences and IV identification strategies.

A. Survey Data

Our main variables draw from the Afrobarometer, which samples the economic, political, and social attitudes of citizens aged 18 or above.8 We pool all five available rounds (seven survey years in total),9 which cover 1999 to 2013. Excluding cohorts born before 1950—who are few in number and less appropriate comparisons than cohorts born closer to the reform—produced a maximum sample of 16,289 respondents from 582 of Nigeria’s 774 LGAs.10 Summary statistics and detailed variable definitions are provided in table A1 of the online appendix.

Outcome variables. We consider three main categories of outcomes: interest in politics, political participation, and community participation. Interest in politics is measured by three variables. First, Discuss politics often is a dummy for the 20% of the sample that report frequently discussing politics with friends or family. Second, News scale is a standardized scale averaging 5-point ordinal scales asking how frequently respondents follow the news on television, by radio, or in newspapers. The scale has a Cronbach’s alpha of 0.69.11 Third, Political knowledge scale is a standardized scale combining indicators for whether the respondents could correctly name their vice president, finance minister, House representative, state governor, and local government chairman or chairwoman (Cronbach’s alpha of 0.54). This knowledge measure is important because respondents cannot overstate their knowledge of politics.

We measure political participation using two basic and three less frequent self-reported behavioral indicators. Registered voter is a dummy for the 78% of respondents registered to vote, and Voted is a dummy for the 64% of the sample who reported voting in the most recent election. While social desirability bias is an important concern for self-reported turnout (Karp & Brockington, 2005), our sample closely maps the 60% sample-weighted average of national turnout in the 1999, 2003, 2007, and 2011 presidential elections.12 We also consider three more demanding forms of participation. Attend demonstration is a dummy for the 14% of respondents who participated in a peaceful protest within the past year. Contact local councilor and Contact representative are, respectively, dummy variables for the 16% and 6% of respondents who contacted each type of political figure in the last year.

Active community participation is measured by group membership and attendance. We code dummy variables for Attend community meeting in the past year and active participation in local associations—Active association member—over the past year.13 In our sample, 48% of respondents have attended a community meeting, and 29% are active association members.

Education. Our main explanatory variable, Education, is a six-category scale measuring the respondent’s highest level of education. The Afrobarometer registers six responses, which we code from 0 to 5: 17% reported receiving no schooling, 5% experienced incomplete primary schooling, 10% completed primary school, 15% experienced incomplete secondary schooling, 47% completed secondary school, and the remaining 6% received some college education. Given Nigeria’s comparatively progressive education policies in Africa, the proportion attending secondary school

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6 The 1976 program had mistakenly assumed oil revenues would persist (Csapo, 1983) alongside economic growth of 5% to 10%, of which 25% could be captured as tax revenues (Achor, 1977).
7 Previous research has found similar results: supply-side educational expansions can persistently reduce constraints on teacher availability (Andrabi, Das, & Khwaja, 2013).
8 Surveys are random samples stratified by state but are not representative by LGA. The response rate in 2008, the only year for which such information is publicly available, was 72%. See the Afrobarometer website for further details.
10 We obtain very similar results without excluding this 7% of the sample, although the relatively high noise in these estimates slightly reduces the precision of our first stage.
11 All scales are constructed using the alpha command in Stata, which does not use casewise deletion and therefore maximizes the available information from the constituent variables. The summative score is then divided by the number of items from which the sum is calculated.
12 We weight official turnout rates at the 1999, 2003, 2007, and 2011 presidential elections (from IDEA International) by the fraction of our sample asked whether they voted at each election. The difference is entirely attributable to the 2013 survey; all results are robust to excluding this wave.
13 We find similar results examining the 44% of respondents who are members of an association.
is relatively high. Furthermore, 73% of our sample was born after the UPE reform. Such a scale imposes linearity on the relationship between our outcomes and ordinal measure of education; we thus estimate the average effect of an additional unit of education. Using a dummy for completing a given level of education would violate the exclusion restriction and could thus substantially bias estimates if other levels of education induced by the treatment also affect the outcome (Marshall, 2016).

B. Empirical Strategy

To identify the effects of education—primarily primary schooling, but also downstream effects at higher education levels—on political behavior, we leverage the varying impact of Nigeria’s UPE program. Specifically, we use the pre-UPE variation in enrollment across LGAs by gender (see figure 1) to capture the differential intensity of the reform.

Reduced form. Our identification strategy, which is similar to Duflo (2001) and Bleakley (2010), exploits temporal and spatial variation. The temporal dimension distinguishes the periods before and after the 1976 UPE reform. Although UPE was abandoned in 1981, it had powerful persistent effects. The exact reason enrollment did not revert to pre-1976 levels is hard to discern—greater school availability, input quality, information about the value of education, or changed norms are all plausible explanations—but it is sufficient for our purpose to note that primary and secondary school enrollment remained relatively steady after 1981 before continuing to increase (see figure 4 below).

Since UPE affected all students of eligible age, a second dimension of variation is required to distinguish the introduction of UPE from cohort effects. Figure 1 shows considerable variation in the enrollment potential of UPE across LGAs and by gender. This second dimension defines the intensity of the UPE reform: where enrollment (for a given gender) was already high prior to the reform, the potential impact of UPE was smaller. Such spatial variation permits a difference-in-differences (DD) strategy, where low-intensity areas serve as control units able to differentiate trends in educational qualifications from the impact of UPE in high-intensity areas.

To map the intensity of UPE to survey respondents, we first count any individual born after 1969—and thus eligible to benefit from UPE’s educational expansion at age 6—as affected by the UPE program. This defines the reform dummy Post-UPE. Then, like Bleakley (2010), we measure the differential intensity of UPE’s impact across LGAs by using the gap between actual and potential enrollment. In particular, we define UPE Intensity as the male or female proportion of the LGA population born between 1960 and 1969 that had not completed primary school. Figure 3 confirms that while some respondents in our sample lived in LGAs with near-universal primary education for both genders, a large proportion, especially of female students, did not.

Individuals are mapped to LGAs based on their current LGA of residence. We therefore assume respondents were educated in the same LGA they currently reside in. An important concern is thus that migrants and nonmigrants differ in their political behavior, which could bias our estimates if migrants systematically moved to certain types of LGA. Although the Afrobarometer does not ask about migration, 23% of HNLSS respondents had not always lived in their current town or village. This represents an upper bound for the selection concern. Moreover, 40% of such migrants did not move across states and so may have migrated within an LGA or migrated to an LGA with similar UPE intensity. According to the Nigeria 2010 Internal Migration Survey, 75% of migration was between urban or between rural areas that tend to have relatively similar intensity scores and around half of respondents cited work reasons as affected by the UPE program. This defines the reform dummy Post-UPE. Then, like Bleakley (2010), we measure the differential intensity of UPE’s impact across LGAs

14 Among cohorts born too early to be eligible for UPE, 27% had no education at all.
15 Students born after 1964 were eligible for some free schooling. However, figure 4 shows no sharp deviation before 1970, suggesting that this nuance is unimportant. Nevertheless, table 3 and tables A3 and A5 in the online appendix, respectively, show that our results are robust to coding cohorts born after 1964 as partially eligible and removing the partially eligible 1964–1969 cohorts.
16 We prefer this measure to newly constructed schools per capita 1975–1981 because founding dates are nonrandomly missing for 11% of those schools—the rate of missing founding dates is more than double in northern states relative to southern states—and while UPE specifically mandated new classrooms, school sizes differed significantly across the country. Furthermore, the number of schools opened cannot capture variation by gender. Despite the limited variation in the number of schools opened, the correlation with our intensity measure is 0.43, and using it instead of our preferred measure provides similar estimates at the cost of our first-stage precision. Moreover, the effect loads on intensity when included alongside construction-based variables in the first stage. State schooling expenditure is available only at the state level and thus also fails to capture important variation in UPE’s intensity.
18 Rural to urban migration represented 16.2% of migration; urban to rural represented 8.6%.
for moving. Furthermore, existing evidence indicates that female migrants and nonmigrants do not differ significantly in their educational level (Osihi & Long, 2008). Although this evidence suggests that our results are unlikely to be driven by positive selection, we nevertheless conduct various robustness checks to address this concern below.

The following DD regression estimates the reduced-form effect of differential UPE exposure:

\[
Y_{icls} = \beta_1 \text{Intensity}_i + \beta_2 (\text{Post-UPE}_c \times \text{Intensity}_i) + X_i \gamma + \kappa_c + \eta_s + \zeta_l + \lambda_{pre,s} \text{Year born}_c + \lambda_{post,s} (\text{Post-UPE}_c \times \text{Year born}_c) + \epsilon_{icls},
\]

(1)

where \(Y_{icls}\) is a political outcome variable measured at time \(t\) for individual \(l\) from cohort, \(c\) in LGA \(l\) (within state \(s\)). We include survey, cohort, and state fixed effects—\(\zeta_l\), \(\kappa_c\), and \(\eta_s\), respectively—to absorb period effects across surveys and time-invariant differences across cohorts and states. Furthermore, the state-specific cohort trends for respondents, Year born, and Post-UPE \(c \times\) Year born, account for differential linear trends across cohorts that were and were not eligible for UPE in different states. Our robustness checks show similar results when instead using LGA fixed effects and LGA-specific cohort trends. Finally, we include gender, religion, and urban dummies in \(X_i\) to increase the efficiency of our estimates. To address spatial clustering of UPE intensity, standard errors are clustered by state throughout.\(^{19}\)

Our key identifying assumption requires that without the UPE reform, trends in \(Y_{icls}\) would not have differed across areas with different UPE intensities. Figure 4 broadly supports such parallel trends, showing similar trends in education levels and three key outcome variables across above- and below-median UPE intensities before the reform.\(^{20}\) Nevertheless, we include state-specific cohort trends to ensure that this does not mask more subtle heterogeneous trends, and conduct various placebo tests for cohorts unaffected by the reforms. Figure 4 also provides preliminary evidence that the UPE reform differentially influenced education levels and civic and political engagement across LGAs.

**Instrumental variables.** To estimate the effects of education, we would ideally estimate the following equation using OLS:

\[
Y_{icdl} = \tau \text{Education}_{icls} + W_l \theta + \kappa_c + \eta_s + \zeta_l + \lambda_{pre,s} \text{Year born}_c + \lambda_{post,s} (\text{Post-UPE}_c \times \text{Year born}_c) + \epsilon_{icdl}.
\]

(2)

Although we estimate such regressions for comparison purposes, these estimates are uninformative for two principal reasons. First, which individuals receive more education is unlikely to be (conditionally) random. Second, the effects of education likely differ across individuals: while individuals who would have attended primary or secondary school anyway might not experience large effects, education could substantially affect those with the lowest propensity to attend school.

To obtain unbiased estimates for the subpopulation of students who comply with UPE incentives, we use an IV strategy. This approach also reduces attenuation biases arising from classic measurement error in our education variable, which may be relatively common in developing contexts like Nigeria. Our IV strategy builds on the reduced form by using 2SLS to estimate a DD specification for our first stage:

\[
\text{Education}_{icls} = \alpha_1 \text{Intensity}_i + \alpha_2 (\text{Post-UPE}_c \times \text{Intensity}_i) + X_i \gamma + \kappa_c + \eta_s + \zeta_l + \lambda_{pre,s} \text{Year born}_c + \lambda_{post,s} (\text{Post-UPE}_c \times \text{Year born}_c) + \epsilon_{icls},
\]

(3)

where Post-UPE \(c \times\) Intensity, is the excluded instrument. UPE compliers are thus individuals who extended their education only because they were exposed to a relatively high intensity of the reform. Given that education is an important status symbol in Nigeria and because states with lower education were generally poorer, compliers likely come from relatively disadvantaged backgrounds; finding large effects for this group of young adults is common (Finkel & Smith, 2011; Friedman et al., 2016).

Using equation (3) we estimate equation (2) with 2SLS to identify the local average treatment effect of a unit increase in education among compliers (Angrist & Imbens, 1995). In addition to parallel trends, identification of the causal effect of education on political outcomes requires a strong first stage, monotonicity, and an exclusion restriction requiring that UPE intensity has no effect on political outcomes except by increasing education. We first demonstrate a strong first stage and provide evidence that the parallel trends assumption holds, before supporting the exclusion restriction as part of our robustness tests.

**IV. Results**

We present our main finding that education, induced by one of the developing world’s largest ever primary educational expansions, substantially increases civic and political
engagement in Nigeria. We start by demonstrating the existence of a strong first stage where UPE differentially affected schooling across Nigeria.

A. Positive Effect of UPE on Education

The results in table 1 confirm that UPE most dramatically increased education in LGAs with low prior rates of primary enrollment. Column 1 shows a significant positive effect for the interaction between the post-UPE period indicator and intensity: moving from the lowest to highest intensity LGA increased education by almost two-thirds of a level. In other words, UPE induced 0.2 more levels of education for an LGA with a 1 standard deviation greater intensity level. This first stage is strong, yielding an \( F \)-statistic of 21.8.

Beyond the inclusion of state-specific cohorts trends on either side of the reform, we check the parallel trends assumption using placebo tests. Restricting the sample to those born before 1970 and using 1965 as a placebo reform, column 2 finds no effect for the interaction with UPE intensity. Similarly, considering 1960 as a placebo reform and restricting the sample to those born before 1965—thereby guaranteeing that no respondents could have even partially benefited from the reform—column 3 also shows that there is no effect on schooling.

The remaining specifications examine dummies for attaining different levels of education. Unsurprisingly, columns 4 and 5 show large effects on the probability that a respondent has at least obtained incomplete and complete primary schooling. Columns 6 and 7 show that the reform also increased incomplete and complete secondary schooling, although the increase is smaller in magnitude. The increase in secondary education suggests that the reform either induced students to continue past primary education or encouraged students who would have only received primary education to receive more education (e.g., to signal high ability on the labor market; Spence, 1973).

B. Positive Effect of UPE and Education on Civic and Political Engagement

We present our main results identifying the effect of UPE and education on civic and political engagement in table 2. Beyond the simple correlations in panel A, the reduced-form and IV estimates in panels B and C show that education increases measures of interest in and knowledge of politics. The IV point estimate in column 1 reinforces the reduced-form effect by showing that a unit increase in education increases a respondent’s propensity to frequently discuss politics by 16 percentage points. This interest in politics is
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Table 1.—Effect of UPE Intensity on Education, First Stage

<table>
<thead>
<tr>
<th>Specifications</th>
<th>Education (1)</th>
<th>Education (2)</th>
<th>Education (3)</th>
<th>Incomplete Primary (4)</th>
<th>Complete Primary (5)</th>
<th>Incomplete Secondary (6)</th>
<th>Complete Secondary (7)</th>
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<tr>
<td>First-stage F-statistic</td>
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<td>0.0</td>
<td>0.0</td>
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Table 2.—Effect of UPE Intensity and Education on Civic and Political Engagement

<table>
<thead>
<tr>
<th>Specifications</th>
<th>Discuss Politics</th>
<th>News Scale</th>
<th>Political Knowledge Scale</th>
<th>Registered Voted</th>
<th>Contact Local Councillor</th>
<th>Contact Representative</th>
<th>Attend Community Meeting</th>
<th>Active Association Member</th>
</tr>
</thead>
<tbody>
<tr>
<td>A: Correlation (OLS)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Education</td>
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<td>0.266***</td>
<td>0.162***</td>
<td>0.034***</td>
<td>0.022***</td>
<td>0.022***</td>
<td>0.027***</td>
<td>0.012***</td>
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<tr>
<td></td>
<td>(0.003)</td>
<td>(0.009)</td>
<td>(0.013)</td>
<td>(0.005)</td>
<td>(0.005)</td>
<td>(0.003)</td>
<td>(0.003)</td>
<td>(0.002)</td>
</tr>
</tbody>
</table>

B: Reduced form (OLS)

| Post-UPE Intensity      | 0.095**          | 0.376***    | 0.433***                 | 0.143**          | 0.110*                  | 0.040                   | 0.097**                  | 0.020                    |
|                         | (0.041)          | (0.090)     | (0.130)                  | (0.052)          | (0.060)                 | (0.036)                 | (0.040)                  | (0.020)                  |

C: Instrumental variables (2SLS)

| Education               | 0.157***         | 0.628***    | 0.832***                 | 0.302**          | 0.227*                  | 0.069                   | 0.160***                 | 0.033                    |
|                         | (0.061)          | (0.127)     | (0.284)                  | (0.145)          | (0.131)                 | (0.061)                 | (0.061)                  | (0.029)                  |

Specifications in panels A and B are estimated using OLS and include state-specific cohort trends for cohorts affected and unaffected by UPE and religion, rural-urban, gender, state, cohort, and survey dummies. State-clustered standard errors in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

Reflected in following the news across a combination of television, radio, and newspaper media platforms in column 2, where a unit increase in education increases our news scale by two-thirds of a standard deviation. Confirming that these results do not simply reflect social desirability bias among educated respondents and that interest in politics translates into political knowledge, column 3 shows that an additional level of education increases the ability of respondents to name national and local government officials by almost an entire standard deviation. Since the UPE reform disproportionately affected disadvantaged members of the community with lower initial propensities to participate, observing large effects for such compliers relative to the correlations in panel A is not surprising.

We also find that education increases the likelihood that Nigerians engage in low-cost but fundamental forms of political participation. Column 4 of panel C shows that an additional level of education increases the likelihood that an individual is registered to vote by 30 percentage points. Consistent with this large increase in registration, column 5 estimates that a unit increase in education raises the probability that an individual reported voting in the previous election by 23 percentage points. The lower precision of these 2SLS estimates reflects the smaller samples available for these variables, although the large point estimates indicate that the uneducated were especially unlikely to turn out in a country where reported turnout is high.

Columns 6 to 8 identify smaller effects for more demanding forms of political participation. Column 6 of panels A and B shows no significant reduced-form or IV effect of education on attending a demonstration, although the point estimate indicates that education increases the likelihood
of demonstrating by 7 percentage points. Since peaceful demonstration is a costlier and more sophisticated form of participation undertaken by only 15% of the sample, it is perhaps unsurprising that lower levels of educated people induced by the UPE reform could not induce demonstration. The evidence from contacting political figures in columns 7 and 8 similarly show that primary and secondary education affect only basic forms of engagement. While column 7 finds that a single-category increase in education raises the probability of UPE compliers contacting local government officials by 16 percentage points, the UPE reform did not significantly affect the rarer and costlier activity of contacting a nationally elected representatives.\(^{22}\)

Consistent with civic culture and social capital theories arguing that education drives civic engagement (Almond & Verba, 1963; Putnam, Leonardi, & Nanetti, 1994), columns 9 and 10 demonstrate that education substantially increases community participation. Panel C shows that a unit increase in education increases the probability that an individual attends community meetings or is an active association member by 21 and 19 percentage points, respectively. Given that half the population attend meetings and one-third are active association members, these estimates suggest that education provides compliers with the skills required to confidently attend meetings.

C. Aggregate Effects of UPE

These preceding results demonstrate that education can substantially increase civic and political engagement for a wide range of citizens in a large and diverse developing country. However, although the effects on individual compliers are important for understanding voter behavior, the broader importance of Nigeria’s UPE policy requires that we analyze the reform’s effect on the national political landscape. To estimate UPE’s aggregate effects on the voting-age population by 2006, we multiply the reduced-form coefficient from column 5 of panel B in table 2 by the intensity of the reform for men and women in each LGA born since 1970:

$$\hat{\beta}_2 \sum_{g \in \{M,F\}} \sum_{l=1}^{774} \left( \text{Intensity}_{g,l} \times \text{Population share}_{g,l} \right).$$

(4)

where Population share\(_{g,l}\) is the share of affected voting-age men and women \(g\) in LGA \(l\) as a share of the voting-age population, as measured in the 2006 Census. These calculations indicate that UPE has increased voter turnout by 3.3 percentage points among eligible voters, the proportion of citizens frequently discussing politics by 2.9 percentage points, and community meeting attendance by 3.8 percentage points. Relative to the 60% turnout rate over our sample period, this increase in turnout represents a 5% increase in turnout. Relative to our sample averages, the increases in discussion and community meeting attendance respectively represent 14% and 8% increases. These effects suggest that the reform substantially reduced inequality in political participation by increasing engagement in Nigeria’s least educated and poorest regions.

These effects are large relative to prior experimental studies examining voter turnout. Sondheimer and Green (2010) show that the effects of Nigeria’s UPE program on turnout are similar in magnitude to education opportunity interventions in the United States also examining disadvantaged populations, while Friedman et al. (2016) find no effect of Kenya’s merit scholarship program on participation in politics and civic affairs. Canvassing or contacting voters by mail just before an election has generally produced intent to treat effects of 1 to 4 percentage points in the United States (Gerber & Green, 2000), except when extrinsic pressure was induced by the threat of publicizing turnout (Gerber, Green, & Larimer, 2008) or in an exceptionally effective case (Nickerson, 2008). A recent social media experiment shows that randomly assigning Facebook users political mobilization messages increased turnout by around 1 percentage point (Bond et al., 2012). Finally, although Banerjee et al. (2011) find that providing citizens with politician report cards before an election increased turnout by 2 percentage points, information treatments in developing countries have typically failed to affect political behavior (see Lieberman et al., 2014). Unlike most of these other studies, we show that UPE’s effects persist long after the experience of education. Therefore, in addition to the economic benefits of education, primary education expansion appears to be one of the most effective tools available to policymakers seeking to achieve the twins goals of economic and political development.

D. Robustness Checks

The key assumptions underlying our estimates are the parallel trends assumption and the exclusion restriction. We show that our results are robust to various plausible violations. We focus on three key variables—political knowledge, voting, and attending a community meeting within the last year—although robustness checks for our other outcomes are in table A3 of the online appendix.

A central concern is that our effects are driven by selective migration. In particular, given that we only know where a respondent currently resides, our results could be upwardly biased if voters affected by the UPE reform with low (high) propensities to participate moved from high- (low-) to low- (high-) intensity LGAs. Although educated migrants moving from rural areas with high intensity to urban areas with low intensity would likely bias our estimates downward, we nevertheless provide three tests to allay migration concerns. First, to reduce the risk of selective migration, we follow Duflo (2001) by restricting attention to students born before the program started in 1976. Although this reduction in the treated sample reduces the precision of our estimates (weakening the first stage to a point where our IV estimates

\(^{22}\) With LGA fixed effects and LGA-specific cohort trends, table A3 in the online appendix documents a significant but modest increase in contacting representatives.
do not turn out moved from the highest-intensity LGA to the lowest-intensity LGA. Average turnout among pre-UPE voters is 69% in the highest-intensity LGAs and 72% in the lowest-intensity LGAs, while the average population in the lowest-intensity LGAs is 1.8 times larger. Given that our HNLSS data indicate that UPE compliers are (at most) 10% more likely to migrate, nine out of every ten of these migrants would have to be a nonvoter to account for the reduced form effect reported in table 2.\textsuperscript{23} However, it is unlikely that virtually all migrants are nonvoters, especially given that the HNLSS data show that if anything, migrants are more likely to attend community meetings (which are positively correlated with voting). If, more plausibly, migration is uncorrelated with voting, migration could account for only 1.5% of the observed reduced-form effect. The concern is further reduced if migrants do not continue to abstain in higher education surroundings (DellaVigna et al., 2017; Marshall, 2017).

An important challenge to the parallel trends assumption is that our instrument is correlated with long-run trends in education or political outcomes. In particular, high-intensity LGAs may exhibit catch-up due to convergence rather than because of increased education due to the UPE program. However, contrary to this convergence concern, panel C shows that pre-UPE LGA education levels do not differentially predict political outcomes among those affected by UPE. Furthermore, panel D shows similar IV estimates when we included LGA fixed effects and pre- and post-UPE LGA-specific linear cohort trends to address the more general parallel trends concern that our results could reflect differential LGA-level cohort trends that cannot be captured by state-specific cohort trends and intercepts. Conversely, panel E shows that our estimates are not sensitive to the exclusion of state-specific cohort trends.

We also consider potential exclusion restriction violations. In particular, the greater construction entailed by the UPE program in high-intensity areas could affect political outcomes through indirect labor market externalities, showing citizens that government can effectively deliver services, or by generating local political engagement with local contracting, planning, and community meetings and discussions where UPE entailed structural changes. Similarly, better-educated individuals could relay or perpetuate their civic norms across pre- and post-UPE cohorts within their networks (Finkel & Smith, 2011), although this general equilibrium effect is likely to bias our estimates downward by reducing the difference between respondents eligible and ineligible for UPE. To address these concerns, we conduct placebo tests restricting the sample to respondents born in

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**Table 3. Robustness Checks**

<table>
<thead>
<tr>
<th></th>
<th>Political Knowledge Scale</th>
<th>Voted (1)</th>
<th>Attend Community Meeting (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>A: Born before 1976 only (reduced form)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post-UPE × Intensity</td>
<td>0.351***</td>
<td>0.169*</td>
<td>0.084</td>
</tr>
<tr>
<td>Observations</td>
<td>3,655</td>
<td>5,288</td>
<td>7,226</td>
</tr>
<tr>
<td><strong>B: Without LGAs with a 2006 population exceeding 500,000 (2SLS)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Intensity</td>
<td>0.827***</td>
<td>0.205</td>
<td>0.193*</td>
</tr>
<tr>
<td>Observations</td>
<td>6,848</td>
<td>10,826</td>
<td>14,682</td>
</tr>
<tr>
<td><strong>C: Correlation with intensity, born after 1969 only</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Intensity</td>
<td>0.092</td>
<td>−0.023</td>
<td>0.011</td>
</tr>
<tr>
<td>Observations</td>
<td>5,341</td>
<td>8,784</td>
<td>11,811</td>
</tr>
<tr>
<td><strong>D: LGA fixed effects and LGA-specific cohort trends (2SLS)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Education</td>
<td>0.354***</td>
<td>0.171**</td>
<td>0.106*</td>
</tr>
<tr>
<td>Observations</td>
<td>7,547</td>
<td>11,974</td>
<td>16,191</td>
</tr>
<tr>
<td><strong>E: State fixed effects and no cohort trends (2SLS)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Education</td>
<td>0.442**</td>
<td>0.236**</td>
<td>0.234**</td>
</tr>
<tr>
<td>Observations</td>
<td>7,547</td>
<td>11,974</td>
<td>16,191</td>
</tr>
<tr>
<td><strong>F: 1965 placebo reform, born before 1970 only (reduced form)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1965 placebo × Intensity</td>
<td>−0.118</td>
<td>0.009</td>
<td>−0.073</td>
</tr>
<tr>
<td>Observations</td>
<td>2,206</td>
<td>3,190</td>
<td>4,380</td>
</tr>
<tr>
<td><strong>G: 1960 placebo reform, born before 1965 only (reduced form)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1960 placebo × Intensity</td>
<td>0.061</td>
<td>0.082</td>
<td>−0.120</td>
</tr>
<tr>
<td>Observations</td>
<td>1,386</td>
<td>2,011</td>
<td>2,728</td>
</tr>
<tr>
<td><strong>H: State-postreform fixed effects (2SLS)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Education</td>
<td>0.844***</td>
<td>0.207</td>
<td>0.212**</td>
</tr>
<tr>
<td>Observations</td>
<td>7,547</td>
<td>11,974</td>
<td>16,191</td>
</tr>
<tr>
<td><strong>I: Controlling for pretreatment covariates by UPE eligibility (2SLS)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Education</td>
<td>0.803***</td>
<td>0.216*</td>
<td>0.175**</td>
</tr>
<tr>
<td>Observations</td>
<td>7,547</td>
<td>11,974</td>
<td>16,191</td>
</tr>
<tr>
<td><strong>J: Allowing students to be partially affected by UPE (2SLS)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Education</td>
<td>0.466**</td>
<td>0.243*</td>
<td>0.248*</td>
</tr>
<tr>
<td>Observations</td>
<td>7,547</td>
<td>11,974</td>
<td>16,191</td>
</tr>
</tbody>
</table>

See table 2 for reduced-form and IV estimation. Robustness checks are described in detail in the main text. See table A3 in the online appendix for other outcome variable results. \textsuperscript{*p < 0.1, **p < 0.05, ***p < 0.01.}

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cannot become unreliable, the magnitudes of our reduced-form point estimates in panel A of table 3 are essentially unaffected. Second, to mitigate the concern that voters migrating to urban areas drive the results, panel B shows that the results are robust to removing the 14 LGAs with a population exceeding 500,000 in 2006.

Third, we conduct a simple bounding exercise to demonstrate that migration could not plausibly have caused the magnitude of our turnout result (to take an important example). Consider the worst possible case, where voters who

\textsuperscript{23} More specifically, if 10% of the population in the highest-intensity LGA (denote this by H) moved to the lowest-intensity LGA (L), then turnout in H would rise from 69% to 76%, while turnout in L would fall from 72% to 69% if nine out of ten of these voters continue not to vote. Taking the difference between the new turnout rates would almost exactly account for our reduced-form effect. Missing migration outcomes in the HNLSS are uncorrelated with our instrument.
Christian and Muslim share of the LGA population. Since LGAs did not exist in 1963, the 1963 LGA population was imputed by distributing the 1963 state population across LGAs according to 2006 LGA-level population weights. Given relatively low levels of migration and stable religious differences, religious composition today remains a good proxy for religious composition at the time of the reform. Moreover, our instrument is not correlated with 2010 religious composition.

24 We obtain similar results if intensity is defined using only cohorts born before 1965 instead of those born between 1960 and 1969, for our placebo test.

25 The correlation between the five- (1960–1964) and ten-year measures is 0.94. Defining UPE intensity by the proportion lacking incomplete primary schooling is almost perfectly correlated (0.99) with the complete primary schooling measure.

26 Specifically, we replace the post-UPE indicator by a variable that counts the number of years of free primary school available to individuals already in school. Given the nature of such partial treatment, we exclude state-specific cohort trends.

27 The correlation between the five- (1960–1964) and ten-year measures is 0.94. Defining UPE intensity by the proportion lacking incomplete primary schooling is almost perfectly correlated (0.99) with the complete primary schooling measure.

28 Religious competition is defined for each LGA as 1 − |n1 − n2| where n1 and n2 denote the LGA share of the population of the two largest religious groups.

29 Due to data availability, this was possible only for the 2005–2013 surveys.

E. Heterogeneity by Religion and Ethnicity

In a country where 28% of our respondents regard political violence as legitimate and corruption is pervasive, political engagement induced by schooling may not necessarily support democratic representation. We focus on the especially important concerns that participation increases are concentrated among locally dominant religious and ethnic groups and may thus reflect political capture, or that education increases in-group identification and thus exacerbates social divisions.

To examine whether increased political engagement masks a sinister reality, we first compare the effects of education across LGAs with differing religious and tribal compositions. We use the HNLSM data to split the sample by above- and below-median religious (Herfindahl) fragmentation scores, above- and below-median religious group competition, and whether respondents are from majority or nonmajority religious groups within their LGA. We also defined members of the main ethnic group in each LGA by identifying cases where the ethnic group speaks the LGA’s main language, as defined by the Local Government Handbook 1998, and cases where the Afrobarometer data tell us that respondents share an ethnic group that coincides with the majority ethnic group within the Afrobarometer sample. Because splitting the samples reduces the power of the first stage, we focus on our reduced-form estimates.

Table 4 finds little evidence of stronger effects among dominant groups. Rather, high UPE intensity almost invariably increases political interest, community participation, and, especially, political participation more in fragmented and competitive areas and among nonmajority members. In fact, in eleven of twelve cases, the reduced form coefficient is larger in magnitude among minority groups and respondents in fragmented and competitive LGAs. This supports Putnam’s (2000) claim that civic participation develops most effectively through bridging associations where individuals from different backgrounds interact together. Tables A6 and A7 in the online appendix report broadly similar results for
of democracy.

Moreover, additional schooling also does not increase support for ethnic and religious group identification in the Democratic Republic of Congo and Kenya. Encouragingly, additional schooling also does not increase support for violence or active participation in religious associations.30

30 We also found no evidence of a differential effect among women, although this finding from Friedman et al. (2016) in Kenya may be relevant.

Table 5.—Effect of UPE Intensity and Education on Support for Violence and Ethnic, National, and Religious Identification

<table>
<thead>
<tr>
<th></th>
<th>Political Violence</th>
<th>Active Religious Association</th>
<th>Ethnic over National Group</th>
<th>Support United Nigeria</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Unjustified</td>
<td>Member</td>
<td>Group</td>
<td></td>
</tr>
<tr>
<td>A: Correlation (OLS)</td>
<td>0.005</td>
<td>0.029**</td>
<td>−0.015**</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>B: Reduced form (OLS)</td>
<td>−0.028</td>
<td>−0.008</td>
<td>−0.006</td>
<td>−0.041</td>
</tr>
<tr>
<td></td>
<td>(0.057)</td>
<td>(0.047)</td>
<td>(0.043)</td>
<td>(0.048)</td>
</tr>
<tr>
<td>C: Instrumental variables (2SLS)</td>
<td>0.004</td>
<td>−0.013</td>
<td>−0.010</td>
<td>−0.052</td>
</tr>
<tr>
<td></td>
<td>(0.087)</td>
<td>(0.078)</td>
<td>(0.070)</td>
<td>(0.060)</td>
</tr>
</tbody>
</table>

Specifications in panels A and B are estimated using OLS and include state-specific cohort trends for cohorts affected and unaffected by UPE, and religion, urban-rural, gender, state, cohort, and survey dummies. Specifications in panel C include the same covariates but are estimated using 2SLS. The specifications in panels B and C also include a linear intensity term. The number of observations across outcomes varies due to data availability (see the online appendix). State-clustered standard errors in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

Table 4.—Differences in UPE Intensity’s Effect by Religion and Ethnic Group

<table>
<thead>
<tr>
<th></th>
<th>Political Knowledge Scale</th>
<th>Voted</th>
<th>Attend Community Meeting</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Panel A</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Above-median LGA religious fragmentation only</td>
<td>0.593***</td>
<td>0.330***</td>
<td>0.245***</td>
</tr>
<tr>
<td>Post-UPE × Intensity</td>
<td>(0.193)</td>
<td>(0.064)</td>
<td>(0.064)</td>
</tr>
<tr>
<td>Observations</td>
<td>3,822</td>
<td>5,961</td>
<td>8,087</td>
</tr>
<tr>
<td>Below-median LGA religious fragmentation only</td>
<td>0.359*</td>
<td>−0.099</td>
<td>0.026</td>
</tr>
<tr>
<td>Post-UPE × Intensity</td>
<td>(0.180)</td>
<td>(0.087)</td>
<td>(0.053)</td>
</tr>
<tr>
<td>Observations</td>
<td>3,725</td>
<td>6,013</td>
<td>8,104</td>
</tr>
<tr>
<td>Panel B</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Above-median religious competition LGAs only</td>
<td>0.579***</td>
<td>0.297***</td>
<td>0.234***</td>
</tr>
<tr>
<td>Post-UPE × Intensity</td>
<td>(0.195)</td>
<td>(0.074)</td>
<td>(0.083)</td>
</tr>
<tr>
<td>Observations</td>
<td>3,685</td>
<td>5,727</td>
<td>7,791</td>
</tr>
<tr>
<td>Below-median religious competition LGAs only</td>
<td>0.289*</td>
<td>−0.066</td>
<td>0.019</td>
</tr>
<tr>
<td>Post-UPE × Intensity</td>
<td>(0.168)</td>
<td>(0.072)</td>
<td>(0.057)</td>
</tr>
<tr>
<td>Observations</td>
<td>3,862</td>
<td>6,247</td>
<td>8,400</td>
</tr>
<tr>
<td>Panel C</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>LGA religious minority members only</td>
<td>0.613*</td>
<td>0.237*</td>
<td>0.188</td>
</tr>
<tr>
<td>Post-UPE × Intensity</td>
<td>(0.307)</td>
<td>(0.121)</td>
<td>(0.116)</td>
</tr>
<tr>
<td>Observations</td>
<td>1,470</td>
<td>2,224</td>
<td>2,953</td>
</tr>
<tr>
<td>LGA religious majority members only</td>
<td>0.425***</td>
<td>0.067</td>
<td>0.099*</td>
</tr>
<tr>
<td>Post-UPE × Intensity</td>
<td>(0.121)</td>
<td>(0.057)</td>
<td>(0.055)</td>
</tr>
<tr>
<td>Observations</td>
<td>6,077</td>
<td>9,750</td>
<td>13,238</td>
</tr>
<tr>
<td>Panel D</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>LGA non-main ethnic group members only</td>
<td>0.498*</td>
<td>0.132</td>
<td>0.245*</td>
</tr>
<tr>
<td>Post-UPE × Intensity</td>
<td>(0.269)</td>
<td>(0.152)</td>
<td>(0.134)</td>
</tr>
<tr>
<td>Observations</td>
<td>1,491</td>
<td>2,199</td>
<td>3,030</td>
</tr>
<tr>
<td>LGA main ethnic group members only</td>
<td>0.557**</td>
<td>0.092</td>
<td>0.177**</td>
</tr>
<tr>
<td>Post-UPE × Intensity</td>
<td>(0.222)</td>
<td>(0.102)</td>
<td>(0.080)</td>
</tr>
<tr>
<td>Observations</td>
<td>2,943</td>
<td>4,483</td>
<td>5,853</td>
</tr>
</tbody>
</table>

All specifications are estimated using OLS and include a linear intensity term, state-specific cohort trends for cohorts affected and unaffected by UPE, and religion, urban-rural, gender, state, cohort, and survey dummies. The number of observations across outcomes varies due to data availability (see the online appendix). State-clustered standard errors in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

Utilizing variation in Nigeria’s nationwide UPE program not affect contact over private issues. Together, these results suggest that education does not accentuate existing divisions; if anything, education encourages civic and political engagement most among locally weak social groups and in LGAs more susceptible to capture or conflict.

Finally, table 5 shows that additional schooling does not affect a UPE-complier’s likelihood of increasing their ethnic identity relative to their national identity, while education did not make compliers less supportive of a united Nigeria. Friedman et al. (2016) similarly find that education does not affect ethnic and religious group identification in the Democratic Republic of Congo and Kenya. Encouragingly, additional schooling also does not increase support for violence or active participation in religious associations.

Moreover, table A8 in the online appendix also shows that the propensity to contact religious or traditional leaders is unaffected. This evidence further suggests that education is not exacerbating local social divisions or increasing participation that may represent institutional capture. Furthermore, table A9 also shows that our results are not being driven by students attending primary school during periods of democracy.

V. Conclusion

In this paper, we present evidence on the long-term effects of government-provided education on adult civic and political engagement from a large nonconsolidated democracy. Utilizing variation in Nigeria’s nationwide UPE program
implemented in 1976, we find that primary and secondary schooling increase fundamental forms of participation such as voting, interest in politics, and community participation. These results are identified for the large and policy-relevant group of citizens who obtained additional schooling only because of the UPE reform. Despite many citizens remaining unaffected by the reform, the importance of UPE for compliers has increased overall turnout by 3 percentage points and community meeting attendance by 4 percentage points many years after the reform.

Addressing the concern that education could induce antidemocratic political participation, we find, contrary to Huntington’s (1968) claim that educational expansion may be incongruent with a nonconsolidated political system, no evidence that education engenders institutional capture or conflict between Nigeria’s array of ethnic and religious groups. If anything, UPE supported the engagement of minority groups in fractionalized locations without increasing support for political violence.

Although reducing educational inequality is an important step in supporting democracy, it cannot guarantee democratic consolidation. Basic education plays a key role in shaping basic forms of civic and political participation but does not induce higher-level engagement with politics through demonstration or contacting nonlocal political figures. Furthermore, evidence from Zimbabwe suggests that a minimum level of democracy is necessary before education can increase political participation (Croke et al., 2016). Moreover, our focus on citizen participation can only partially illuminate the political equilibrium. Although we found little evidence that education itself changed voter policy preferences, the reform increased the participation of voters who are relatively poorer, less likely to believe that the government is effectively handling the economy, and less likely to support the then ruling PDP party. Future research should also consider the strategic behavior of political actors responding to a more educated citizenry.

Nevertheless, given the importance of education in developing democracies, establishing the direction and substantial magnitude of education’s political effects is a crucial first step in disentangling the relationship between education and democracy. Our findings are thus highly relevant for the many developing countries currently expanding primary and secondary education. Unlike many studies examining the causal effects of programs supporting education, our findings identify long-run effects for a wide range of compliers in a nationwide government-run program. They are thus optimistic in that they show, unlike recent correlative research (Finkel & Ernst, 2005), that publicly provided education can produce substantial prodemocratic effects.

With the prodemocratic effects of education in a particularly diverse developing democracy now established, future research should consider the mechanisms underpinning these effects to help governments design their education systems to support prodemocratic behavior. While beyond the scope of this study, it is important to differentiate the skills and values learned at school (Bowles & Gintis, 1976; Verba, Schlozman, & Brady, 1995) from downstream effects such as increased income, community interactions, and empowered social status.

REFERENCES


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