THE EFFECT OF EDUCATION ON CIVIC AND POLITICAL ENGAGEMENT IN NON-CONSOLIDATED DEMOCRACIES: EVIDENCE FROM NIGERIA [*]

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Developing democracies are experiencing unprecedented increases in primary and secondary schooling. To identify education's long-run political effects, we utilize 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.

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1 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 and 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 and Miguel 2012; Gugerty and Kremer 2008; Humphreys, Sanchez de la Sierra and van der Windt 2012), while civic education programs have produced mixed results (e.g. Finkel and Smith 2011; Finkel, Horowitz and Rojo-Mendoza 2012). Similarly, political information interventions across the developing world have generally failed to increase political engagement (see Lieberman, Posner and Tsai 2014).

Although the Millennium Development Goals (MDGs) are primarily concerned with eradicating poverty, the push for universal primary education 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 and 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 and 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 and Chor 2012*a*; Glaeser et al. 2004). Recent studies have sought to address the difficulty of identifying education's political effects by exploiting individual-level variation. In particular, studies in consolidated democracies have found

that primary and secondary education can substantially increase political participation (e.g. Dee 2004; Milligan, Moretti and Oreopoulos 2004; Sondheimer and Green 2010).

However, education's participatory effects in non-consolidated democracies—polities that hold relatively fair and competitive multi-party elections, but which 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 and Miguel 2012; Gugerty and Kremer 2008), then increased education could accentuate institutional capture (Bardhan 2002; Gugerty and Kremer 2008) and support for political violence (Friedman et al. 2016). This could ultimately lead to violent uprisings and democratic breakdown (Campante and Chor 2012b; Huntington 1968). These concerns are especially pertinent where local ethnic and religious divisions are already 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 and Palmer 2008).[1] Recent field experiments have found mixed effects of foreign donor-supported educational programs (Friedman et al. 2016; Gottlieb 2016; Kuenzi 2006). However, such experiments typically focus on short, group-specific and small-scale NGO-implemented programs, and may thus differ substantially from the nationwide government-implemented programs currently being enacted as part of the MDGs. Given that surveys are rarely administered more than a year after

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

the intervention, it is uncertain whether education's impacts are lasting or have reached fruition. In contrast, natural experiments can evaluate the effects of major educational reforms, but have yet to be utilized to identify primary and secondary education's political effects in developing contexts.[2] secondary enrollment. Using Afrobarometer survey data, from 1999-2013, we exploit variation in the intensity of the UPE reform—captured by variation in pre-reform 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; Duflo 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

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[2]Existing work examines the effects of higher levels of education on participation in electoral authoritarian regimes (Croke et al. forthcoming) or on involvement in electoral campaigns (Wantchekon, Klašnja and Novta 2015).

This paper identifies primary and secondary education's long-run effects on civic and political engagement in Nigeria's non-consolidated democracy, following one of Africa's largest government-provided educational expansions. In 1976, Nigeria implemented its Universal Primary Education (UPE) program, providing six tuition-free years of primary education to all six year-olds. This increased primary enrollment by more than seven million by 1981 (Osili and Long 2008; Oyelere 2010), which in turn substantially increased

to including LGA-specific cohort trends, while placebo tests indicate no differential pre-trends 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, education's effects 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 pro-democratic civic and political engagement, even in non-consolidated 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 pro-democratic participation. However, given recent evidence that education stimulated disengagement in Zimbabwe until elections became more competitive in 2008 (Croke et al. forthcoming)—albeit still far below the levels of competition in our Nigerian sample—it seems that education's pro-participation effects may only materialize when politics is sufficiently competitive for voters to be able to mean-

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ingfully express their preferences.

The paper proceeds as follows. Section 2 provides an overview of education policies in Nigeria, focusing particularly on the 1976 UPE reform. Section 3 details the data and empirical strategy. Section 4 shows our main results and robustness checks. Section 5 concludes.

2   Politics and Primary Education in Nigeria

Despite its political instability, Nigeria has experimented with some of Africa's most ambitious nationwide education policies. This section provides a brief historical overview of Nigerian politics and education provision, before detailing the 1976 educational reform underpinning our identification strategy.

2.1   Social and political context

Africa's most populous nation containing 162.5m people in 2011, Nigeria is a major oil and gas producer, but ranks poorly 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 will be 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 and 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

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featured vote buying, polling irregularities and violence (e.g. Bratton 2008; Collier and 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. 301 LGAs were first established in 1976, broadly along ethnic lines. In 1999, the number of LGAs increased to 774, although the constitution established that local governments would be subject to state-level legislation (Egbe 2014). Although their state-level oversight varies significantly, local governments are responsible for developing economic plans, road maintenance, sewage and refuse disposal, and some levies and tax collection.

Despite frequent shifts between military and democratic government, civil society and local politics in Nigeria are relatively vibrant. A large number of non-governmental 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). 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—that install local politicians that depend upon their patronage for re-election (Albin-Lackey and Rawlence 2007).

2.2   Pre-1976 education

Prior to independence, Great Britain divided Nigeria into three semi-autonomous 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 and Long 2008).[3]

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 pre-existing educational disparities, enrollment in the North remained low due to traditional attitudes towards 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 and Long 2008; Oyelere 2010).

[Figure 1 about here]

Figures 1a and 1b illustrate the considerable variation in the proportion of male and female students failing to complete primary school in the ten cohorts preceding UPE's introduction in 1976. Using representative LGA survey data from the 2009-2010 Harmonized Nigeria Living Standards Survey (HNLSS),[4] they demonstrate that enrollment was lowest in Nigeria's more rural and Northern areas, which were and remain least economically developed.

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[3]See Figure A1 in the Online Appendix.

[4]The World Bank and Nigerian National Bureau of Statistics implemented this survey. Ten households from ten enumeration areas were surveyed across all 774 LGAs.

2.3   1976 Universal Primary Education program

Buoyed by their oil revenue boom, Nigeria's post-independence military federal government led by Olusegun Obasanjo announced in 1974 "one of the most ambitious education projects in African history" (Bray 1981:1). Starting 1st September 1976, the government implemented its nationwide UPE program, providing six 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 700m 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.

[Figure 2 about here]

Despite efforts to increase formal teacher training, under-staffing 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).[5] However, continuing investments substantially increased teacher supply

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[5]This would downwardly bias our estimates if education quality was lower where the reform

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 4.4m in 1974 to 13.8m in 1981 (Osili and Long 2008; Oyelere 2010). This exceeded government expectations based on the 1963 Census of 11.5m 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.[6] 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 and Long 2008). As Figure 4 shows below, enrollment barely changed, and with time continued to increase. This suggests that school availability and better inputs, rather than fees, principally drove later enrollment decisions.[7] Supporting this claim, Ozigi and Ocho (1981) find that the experience of UPE raised Northern parents' willingness to pay for schooling.

3   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.

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was most intense. Moreover, the under-supply afflicting all states makes it unlikely that teachers migrated across states for work.

[6]The 1976 program had mistakenly assumed oil revenues would persist (Csapo 1983) alongside economic growth of 5-10%, of which 25% could be captured as tax revenues (Achor 1977).

[7]Previous research has found similar results, finding that supply-side educational expansions can persistently reduce constraints on teacher availability (Andrabi, Das and Khwaja 2013).

3.1 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 1999-2013. 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.

3.1.1 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 five-point ordinal scales asking how frequently respondents follow the news on television, by radio or in newspapers. The scale has a Cronbach's alpha inter-item reliability coefficient 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 govern-

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[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.

[9]The surveys were conducted in: 1999, 2001, 2003, 2005, 2007, 2008 and 2013.

[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.

ment chairman/woman (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 that are registered to vote, while *Voted* is a dummy for the 64% of the sample that reported voting at the last federal election. While social desirability bias is an important concern for self-reported turnout (e.g. Karp and 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 last 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 last year and active participation in local associations—*Active association member*—over the last year.[13] In our sample, 48% of respondents have attended a community meeting, while 29% are active association members.

3.1.2   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

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[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 that are members of an association.

progressive education policies in Africa, the proportion attending secondary school is relatively high. Furthermore, 73% of our sample was born after the UPE reform.[14] 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*a*).

3.2 Empirical Strategy

To identify the effects of education—principally 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 Figures 1a and 1b) to capture the differential intensity of the reform.

3.2.1 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. Figures 1a and 1b show 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

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[14]Among cohorts born too early to be eligible for UPE, 27% had no education at all.

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—who is thus eligible to benefit from UPE's educational expansion at age 6—as impacted by the UPE program. This defines the reform dummy *Post-UPE*.[15] 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.[16] 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.

[Figure 3 about here]

Individuals are mapped to LGAs based on their current LGA of residence. We therefore assume

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[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 1975-81 because founding dates are non-randomly 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 blue first stage precision. Moreover, the effect loads on intensity when included alongside construction-based variables in the first stage. State schooling expenditure is only available at the state level, and thus also fails to capture important variation in UPE's intensity.

respondents were educated in the same LGA they currently reside in. An important concern is thus that migrants and non-migrants 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.[17] 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 which tend to have relatively similar intensity scores,[18] and around half of respondents cited work reasons for moving. Furthermore, existing evidence indicates that **female** migrants and non-migrants do not differ significantly in their educational level (Osili and 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:

$$Y_{i,c,l,s,t} = \beta_1 \text{Intensity}_l + \beta_2 \left(\text{Post-UPE}_c \times \text{Intensity}_l \right) + X_i \gamma + \kappa_c + \eta_s + \zeta_t$$
$$+ \lambda_{pre,s} \text{Year born}_c + \lambda_{post,s} \left(\text{Post-UPE}_c \times \text{Year born}_c \right) + \varepsilon_{i,c,l,s,t}, \quad (1)$$

where $Y_{i,c,l,s,t}$ is a political outcome variable measured at time t for individual i from cohort c in LGA l (within state s). We include survey, cohort and state fixed effects—ζ_t, κ_c and η_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$_c$ and Post-UPE$_c \times$ Year born$_c$, 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

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[17]Similarly, Osili and Long (2008) note that two-thirds of the 1999 Demographic Health Survey sample never moved.

[18]Rural to urban migration represented 16.2% of migration; urban to rural represented 8.6%.

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 $Y_{i,c,l,s,t}$ 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.

[Figure 4 about here]

3.2.2 Instrumental variables

To estimate the effects of education, we would ideally estimate the following equation using OLS:

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\begin{aligned}
Y_{i,c,l,s,t} = {} & \tau \text{Education}_{i,c,l,s,t} + W_i\theta + \kappa_c + \eta_s + \zeta_t \\
& + \lambda_{pre,s}\text{Year born}_c + \lambda_{post,s}\left(\text{Post-UPE}_c \times \text{Year born}_c\right) + \varepsilon_{i,c,l,s,t}.
\end{aligned} \tag{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 that would have attended primary or secondary school anyway may not experience large effects, education could substantially affect those with the lowest propensity to attend school.

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[19]We cluster by the 36 states (and Abuja) at the time when Afrobarometer was conducted. Standard errors are barely affected when clustering instead by the 19 states that existed in 1976.

[20]Figure A2 in the Online Appendix shows similar trends for each level of education separately.

To obtain unbiased estimates for the subpopulation of students that 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 upon the reduced form by using OLS to estimate a DD specification for our first stage:

$$\text{Education}_{i,c,l,s,t} = \alpha_1 \text{Intensity}_l + \alpha_2 \Big(\text{Post-UPE}_c \times \text{Intensity}_l \Big) + X_i \gamma + \kappa_c + \eta_s + \zeta_t$$
$$+ \lambda_{pre,s} \text{Year born}_c + \lambda_{post,s} \Big(\text{Post-UPE}_c \times \text{Year born}_c \Big) + \varepsilon_{i,c,l,s,t}, \quad (3)$$

where Post-UPE$_c \times$ Intensity$_l$ is the excluded instrument. UPE compliers are thus individuals that only extended their education 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 (e.g. Finkel and 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 and 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.

4 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.

4.1 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.[21] Alternatively put, UPE induced 0.2 more levels of education for an LGA with a one standard deviation greater intensity level. This first stage is strong, yielding an *F* statistic of 21.8.

[Table 1 about here]

Beyond the inclusion of state-specific cohorts trends either side of the reform, we further 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 that would have only received primary education to receive more education, e.g. to signal high ability on the labor market (Spence 1973).

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[21]Interacting our instrument with gender indicates that the first stage effect is 40% lower for women. Given that our reduced form estimates are similarly lower for women as well, our IV estimates suggest similar effects for men and women.

4.2 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. Beyond the simple correlations in Panel A, the reduced form and IV estimates in panels B and C of Table 2 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 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 impacted 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.

[Table 2 about here]

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 last 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)-(8) identify smaller effects for more demanding forms of political participation. Column (6) of panels A and B show no significant reduced form or IV effect of education on attend-

ing 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 only undertaken by 15% of the sample, it is perhaps unsurprising that lower levels of educated 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 only affect 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 impact 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 and Verba 1963; Putnam, Leonardi and 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.

4.3   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

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[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.

intensity of the reform for men and women in each LGA born since 1970:

$$\hat{\beta}_2 \sum_{g \in \{M,F\}} \sum_{l=1}^{774} \left(\text{Intensity}_{g,l} \times \text{Population share}_{g,l} \right), \tag{4}$$

where Population share$_{g,l}$ is the share of affected voting age men/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 U.S. 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 1-4 percentage points in the U.S. (Gerber and Green 2000), except when extrinsic pressure was induced by the threat of publicizing turnout (Gerber, Green and 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 one 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 two percentage points, information treatments in developing countries have typically failed to affect political behavior (see Lieberman, Posner and Tsai 2014). Unlike most of these studies, we show UPE's effects persist long after the experience of education. Therefore, in

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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.

4.4 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 downwardly bias our estimates, 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 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.

[Table 3 about here]

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 that 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

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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 indicates that UPE-compliers are (at most) 10% more likely to migrate, 9 out of every 10 of these migrants would have to be a non-voter to account for the reduced form effect reported in Table 2.[23] However, it is unlikely that virtually all migrants are non-voters, especially given that the HNLSS data shows 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 only account for 1.5% of the observed reduced form effect. The concern is further reduced if migrants do not continue to abstain in higher-education surroundings (e.g. DellaVigna et al. 2014; Marshall 2016b).

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 ser-

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[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 9 out of 10 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.

vices, 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 and Smith 2011), although this general equilibrium effect is likely to downwardly bias our estimates 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 before 1970, and hence too old to be affected by UPE's educational opportunities. Panel F treats those born between 1965 and 1969 as if they were treated, and compares them to those born before 1965. Panel G removes those born between 1965 and 1969, who might have been partially treated by the program, and treats those born between 1960 and 1964 as treated instead. With only one significantly positive coefficient across 18 tests (including those reported in the Online Appendix), these support our exclusion restriction.[24]

Another exclusion restriction concern is that UPE simultaneously increased the quality of education. If anything, however, quality decreased as schools struggled to meet the influx of new students, and potentially underestimates education's engagement effects if education civic attitudes and skills. Asagwara (1997) and Csapo (1983) point to under-estimated enrollment and major initial reductions in qualified teachers, teacher-pupil ratios, adequate learning environments, learning materials and student supervision that were partially recovered but never surpassed—and not just in Northern states. Moreover, Oyelere (2010) considers quality in detail, finding no evidence that UPE affected school quality or created differential returns to schooling.

A more general concern is that our reform intensity variable captures differences other than schooling that may have started to differentially affect students around the date of the UPE reform. Given the spatial differences in pre-reform schooling shown in Figures 1a and 1b, our results could reflect other region-specific changes affecting younger cohorts. To address this concern, we interact our state fixed effects with our UPE eligibility indicator. Exploiting only such within-state

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[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 tests.

variation in the reform's intensity, panel H shows similar point estimates. To capture differences that may be less geographically concentrated, we also simultaneously control for the interaction of our UPE eligibility indicator with estimates of 1963 LGA population size and the Christian and Muslim share of the LGA population.[25] The results in panel I demonstrate the robustness of our findings to the possibility that factors associated with these salient sociodemographic characteristics also differentially affected cohorts around the time of the UPE reform.

Finally, our results are also robust to alternative operationalizations of UPE reform intensity. First, although Figure 4 and Oyelere (2010) suggest otherwise, the UPE reform could also have induced the five (birth-year) cohorts already in primary school at the time of the reform to remain in school longer. However, following Bleakley (2010) and Johnson (2014), panel J shows that our results are unaffected if we use a step function to define being partially affected by the UPE reform.[26] Second, defining UPE intensity by the primary completion rates of those born 1960-1964 (who were not even partially affected by UPE) or the proportion lacking incomplete (rather than complete) primary schooling, Table A4 in the Online Appendix shows similar results.[27]

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[25]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.

[26]Specifically, we replace the post-UPE indicator by a variable that counts that 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.

4.5 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 HNLSS data to split the sample by above and below-median religious (Herfindahl) fragmentation scores, above and below-median religious group competition,[28] and whether respondents are from majority or non-majority 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 tells us that respondents share an ethnic group that coincides with the majority ethnic group within the Afrobarometer sample.[29] 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 non-majority members. In fact, in 11 of 12 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

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[28]Religious competition is defined for each LGA as $1 - |n_1 - n_2|$ where n_1 and n_2 denote the LGA share of the population of the two largest religious groups.

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

broadly similar results for our other outcomes. Further suggesting that the UPE reform reduced economic and political inequalities, the reform also induced larger increases in the education of voters from non-dominant groups.

[Table 4 about here]

Potentially less benign is the result, shown in Table A6 of the Online Appendix, that respondents in fractionalized and competitive LGAs tend to contact their representatives more often. While this is unsurprising, given that such respondents are most likely to have voted for or feel connected to such politicians, this suggests that education cannot resolve all differentials. However, Table A8 in the Online Appendix indicates that education increases the propensity to contact a local government official over a community issue, but does 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.

[Table 5 about here]

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.[30] 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.

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[30]We also found no evidence of a differential effect among women, although this finding from Friedman et al. (2016) in Kenya may only be relevant for the young women in their sample.

5 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 non-consolidated 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 only obtained additional schooling because of the UPE reform. Despite many citizens remaining unaffected by the reform, the importance of UPE for compliers has increased overall turnout by three percentage points and community meeting attendance by four percentage points many years after the reform.

Addressing the concern that education could induce anti-democratic political participation, we find—contrary to Huntington's (1968) claim that educational expansion may be incongruent with a non-consolidated 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 non-local political figures. Furthermore, evidence from Zimbabwe suggests that a minimum level of democracy is necessary before education can increase political participation (Croke et al. forthcoming). 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 that 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.[31] 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 and Ernst 2005), that publicly-provided education can produce substantial pro-democratic effects.

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

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[31]High intensity LGAs and low levels of education are correlated with such characteristics. However, Table A10 in the Online Appendix shows that these political preferences and incumbent performance evaluations are, on average, unaffected by the UPE education reform.

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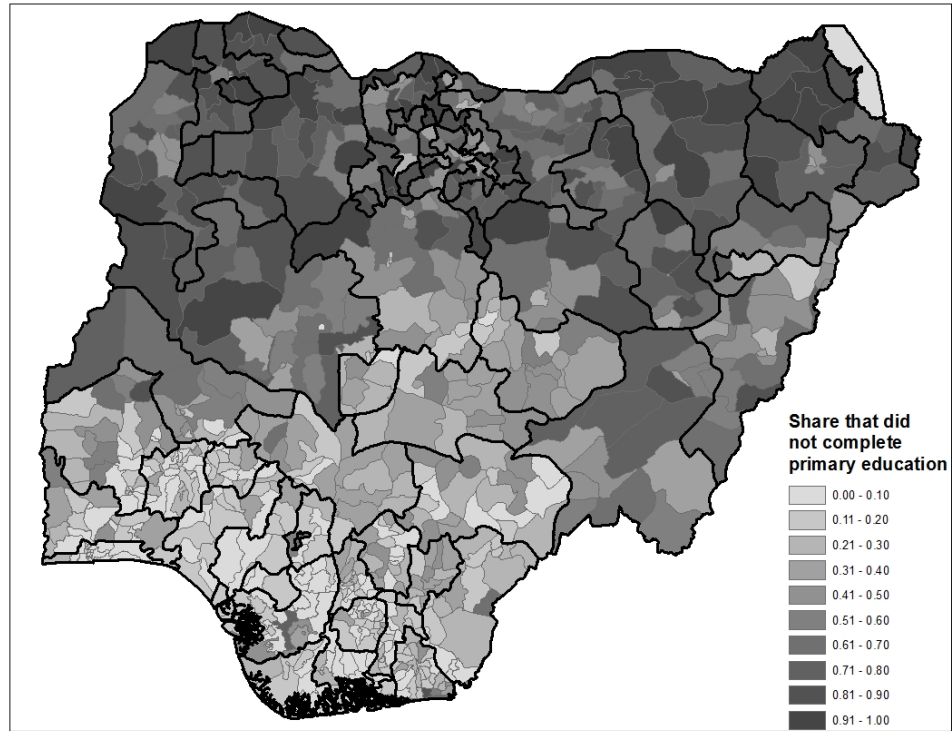
(a) Male students
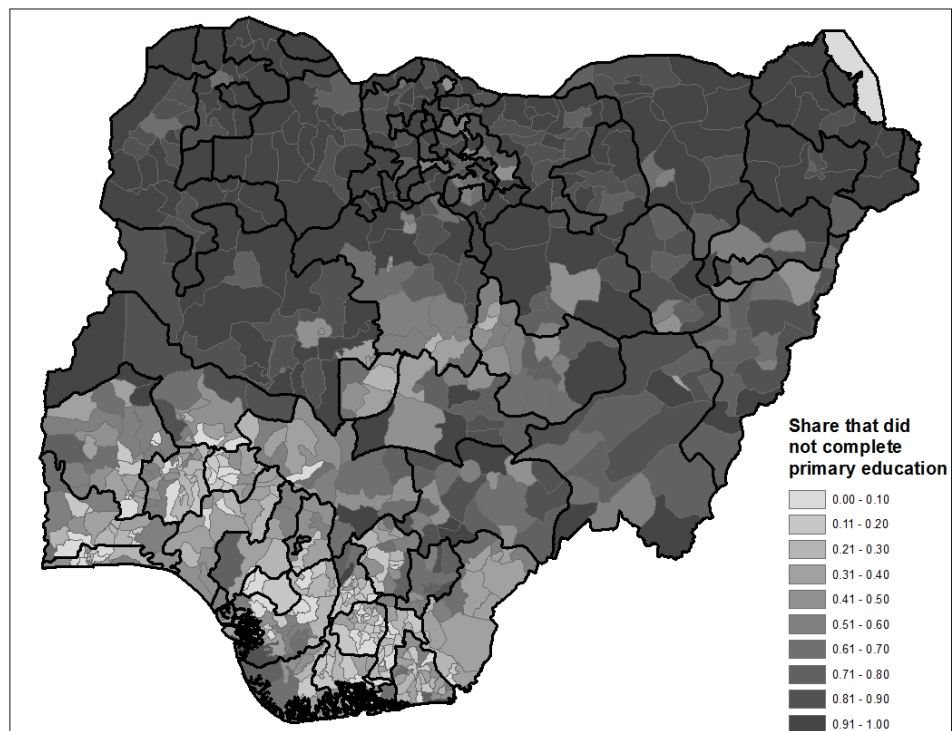
(b) Female students

Figure 1: Proportion of students not completing primary school born 1960-1969 by LGA (source: HNLSS)
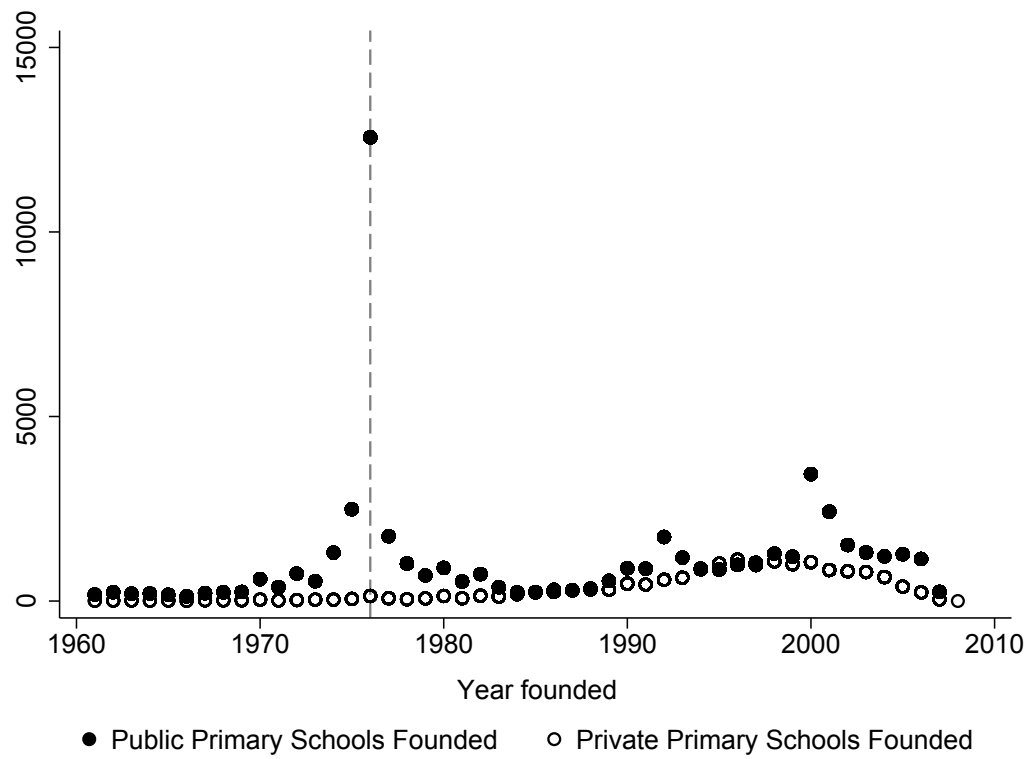
Figure 2: Number of public and private primary schools founded since independence (source: Nigerian Primary School Census 2008)

38
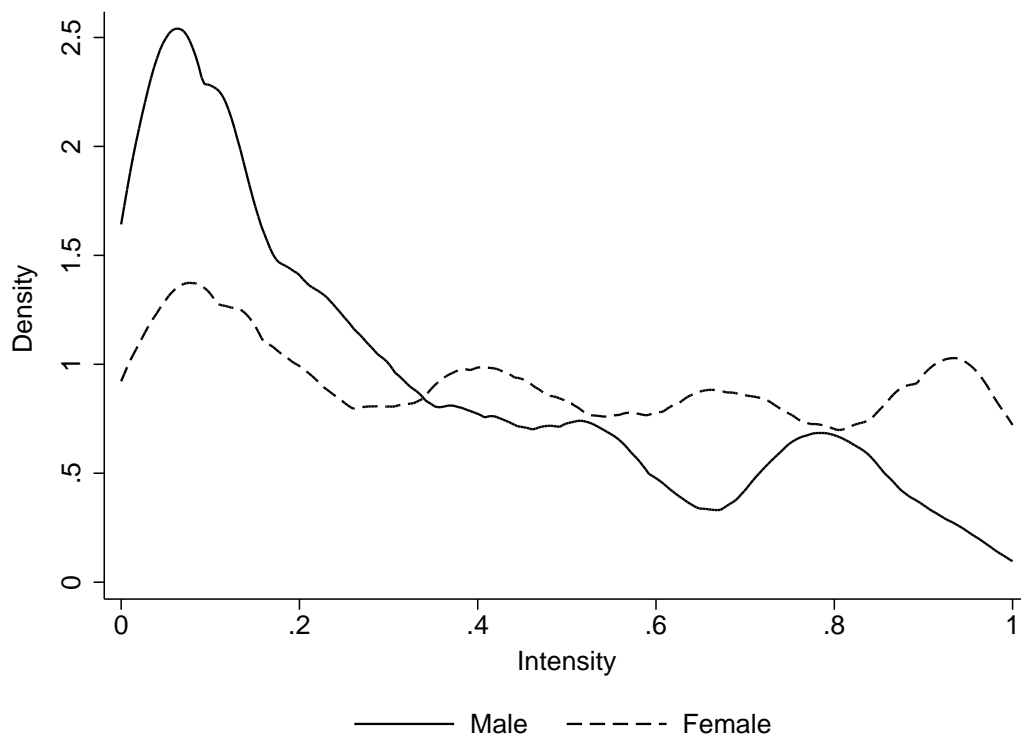
Figure 3: UPE intensity distribution across LGAs and by gender (source: HNLSS)

Note: The figure shows the kernel density distribution in our sample separately for men and women.
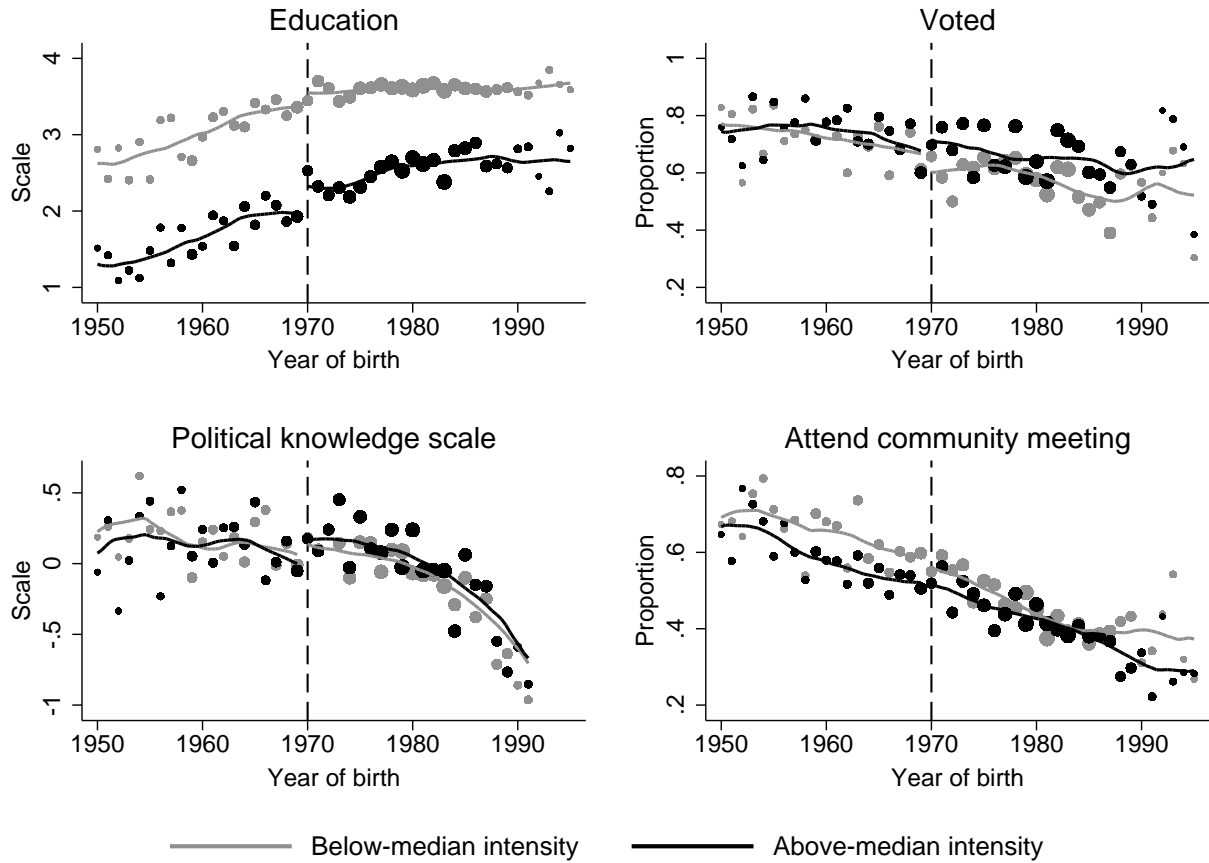
Figure 4: Trends in education level, voting, political knowledge and community meeting attendance, by UPE intensity (sources: Afrobarometer and HNLSS)

Notes: Each dot represents the average education score by cohort in above- and below-median UPE intensity cases. The size of the dot reflects the quantity of data in our sample. We overlay local polynomial curves (bandwidth of 2) to show trends in education across treatment intensity. Above-median and below-median intensity denote respondents above and below the sample median LGA-gender UPE intensity score. The graphs show similar pre-trends across above- and below-median intensity groups.

Table 1: Effect of UPE intensity on education (first stage)

	Education	Education	Education	Incomplete primary	Complete primary	Incomplete secondary	Complete secondary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Intensity	-0.965***	-0.596**	-0.669**	-0.226***	-0.255***	-0.253***	-0.174***
	(0.192)	(0.251)	(0.290)	(0.051)	(0.056)	(0.049)	(0.046)
Post-UPE × Intensity	0.598***			0.139***	0.144***	0.135***	0.090***
	(0.128)			(0.031)	(0.040)	(0.045)	(0.032)
1965 Placebo × Intensity		-0.224					
		(0.223)					
1960 Placebo × Intensity			0.020				
			(0.326)				
Observations	16,289	4,402	2,792	16,289	16,289	16,289	16,289
Outcome mean	2.89	2.38	2.22	0.83	0.78	0.68	0.53
Outcome std. dev.	1.58	1.78	1.81	0.37	0.41	0.47	0.50
Instrument mean	0.28	0.26	0.31	0.28	0.28	0.28	0.28
Instrument std. dev.	0.32	0.31	0.32	0.32	0.32	0.32	0.32
First stage F statistic	21.8	1.0	0.0				

Notes: All specifications are estimated using OLS, and include state-specific cohort trends for cohorts affected and not affected by UPE and religion, rural-urban, gender, state, cohort, and survey dummies. State-clustered standard errors in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table 2: Effect of UPE intensity and education on civic and political engagement

	Political interest			Basic political participation		Less frequent forms of political participation			Community participation	
	Discuss politics often	News scale	Political knowledge scale	Registered voter	Voted	Attended demonstration	Contact local councilor	Contact representative	Attend community meeting	Active association member
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Correlation (OLS)										
Education	0.035***	0.260***	0.162***	0.034***	0.022***	0.022***	0.027***	0.012***	0.033***	0.029***
	(0.003)	(0.009)	(0.013)	(0.003)	(0.005)	(0.003)	(0.003)	(0.002)	(0.005)	(0.005)
Panel B: Reduced form (OLS)										
Post-UPE × Intensity	0.095**	0.376***	0.433***	0.143**	0.110*	0.040	0.097**	0.020	0.125**	0.115**
	(0.041)	(0.090)	(0.130)	(0.052)	(0.060)	(0.036)	(0.040)	(0.020)	(0.058)	(0.051)
Panel C: Instrumental variables (2SLS)										
Education	0.157***	0.628***	0.832***	0.302**	0.227*	0.069	0.160***	0.033	0.211**	0.191**
	(0.061)	(0.127)	(0.284)	(0.145)	(0.131)	(0.061)	(0.061)	(0.029)	(0.105)	(0.092)
Observations	16,190	16,273	7,547	7,510	11,974	16,072	16,240	16,212	16,191	16,206
Outcome mean	0.20	0.00	0.00	0.78	0.64	0.14	0.16	0.06	0.48	0.29
Outcome standard deviation	0.40	1.00	1.00	0.42	0.48	0.35	0.37	0.24	0.50	0.46
First stage F statistic	22.4	22.3	9.5	8.1	12.4	21.8	21.0	21.1	21.2	21.4

Notes: Specifications in panels A and B are estimated using OLS, and include state-specific cohort trends for cohorts affected and not affected by UPE and religion, rural-urban, 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 Online Appendix). State-clustered standard errors in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table 3: Robustness checks

	Political knowledge scale (1)	Voted (2)	Attend community meeting (3)
Panel A: Born before 1976 only (reduced form)			
Post-UPE × Intensity	0.351**	0.169*	0.084
	(0.161)	(0.084)	(0.072)
Observations	3,655	5,288	7,226
Panel B: Without LGAs with a 2006 population exceeding 500,000 (2SLS)			
Intensity	0.827***	0.205	0.193*
	(0.280)	(0.137)	(0.104)
Observations	6,848	10,826	14,682
Panel C: Correlation with intensity, born after 1969 only			
Intensity	0.092	-0.023	0.011
	(0.122)	(0.055)	(0.053)
Observations	5,341	8,784	11,811
Panel D: LGA fixed effects and LGA-specific cohort trends (2SLS)			
Education	0.354***	0.171**	0.106*
	(0.121)	(0.087)	(0.062)
Observations	7,547	11,974	16,191
Panel E: State fixed effects and no cohort trends (2SLS)			
Education	0.442***	0.236**	0.234**
	(0.154)	(0.105)	(0.108)
Observations	7,547	11,974	16,191
Panel F: 1965 placebo reform, born before 1970 only (reduced form)			
1965 placebo × Intensity	-0.118	0.009	-0.073
	(0.189)	(0.047)	(0.073)
Observations	2,206	3,190	4,380
Panel G: 1960 placebo reform, born before 1965 only (reduced form)			
1960 placebo × Intensity	0.061	0.082	-0.120
	(0.262)	(0.105)	(0.104)
Observations	1,386	2,011	2,778
Panel H: State-post reform fixed effects (2SLS)			
Education	0.844***	0.207	0.212**
	(0.286)	(0.129)	(0.107)
Observations	7,547	11,974	16,191
Panel I: Controlling for pre-treatment covariates by UPE eligibility (2SLS)			
Education	0.803***	0.216*	0.175**
	(0.282)	(0.131)	(0.089)
Observations	7,547	11,974	16,191
Panel J: Allowing students to be partially affected by UPE (2SLS)			
Education	0.466**	0.243*	0.248*
	(0.204)	(0.128)	(0.137)
Observations	7,547	11,974	16,191

Notes: 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.

Table 4: Differences in UPE intensity's effect by religion and ethnic group

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

Notes: All specifications are estimated using OLS, and include a linear intensity term, state-specific cohort trends for cohorts affected and not affected by UPE and religion, rural-urban, gender, state, cohort, and survey dummies. The number of observations across outcomes varies due to data availability (see Online Appendix). State-clustered standard errors in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table 5: Effect of UPE intensity and education on support for violence and ethnic, national and religious identification

	Political violence unjustified (1)	Active religious assoc. member (2)	Ethnic over national group (3)	Support united Nigeria (4)
Panel A: Correlation (OLS)				
Education	0.005	0.029***	-0.015***	0.001
	(0.005)	(0.004)	(0.004)	(0.005)
Panel B: Reduced form (OLS)				
Post-UPE × Intensity	-0.028	-0.008	-0.006	-0.041
	(0.057)	(0.047)	(0.043)	(0.048)
Panel C: Instrumental variables (2SLS)				
Education	-0.044	-0.013	-0.010	-0.052
	(0.087)	(0.078)	(0.070)	(0.060)
Observations	11,539	16,271	15,959	6,410
Outcome mean	0.72	0.55	0.26	0.80
Outcome standard deviation	0.45	0.50	0.44	0.40
First stage F statistic	18.7	21.8	20.8	25.1

Notes: Specifications in panels A and B are estimated using OLS, and include state-specific cohort trends for cohorts affected and not affected by UPE and religion, rural-urban, 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 Online Appendix). State-clustered standard errors in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

The Effect of Education on Civic and Political Engagement in Non-Consolidated Democracies: Evidence from Nigeria

Horacio Larreguy [*]   John Marshall [†]

May 2016

1 Missionary schools
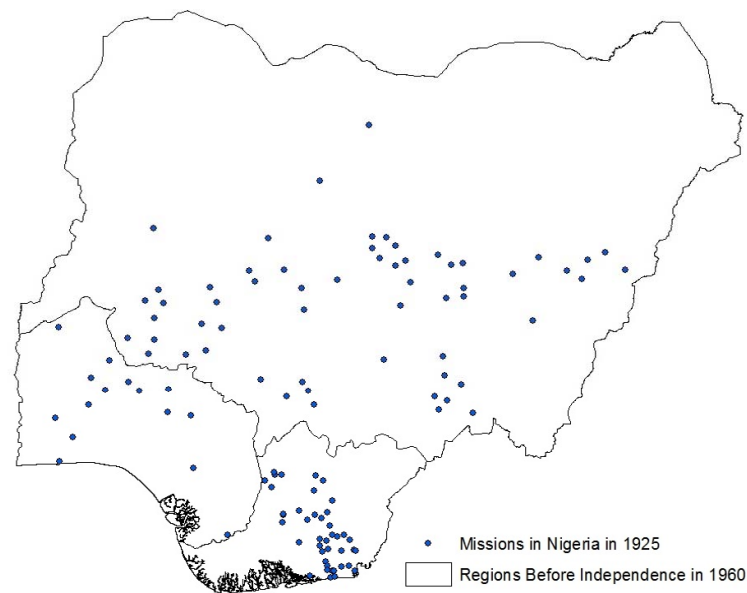

Figure A1: Christian missionary stations in Nigeria 1925 (source: Streit 1929)

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2   Detailed variable definitions

The following variables come from the 1999-2013 Afrobarometer rounds, unless noted otherwise (for the last four variables). Table A1 provides summary statistics. Table A2 shows the pairwise correlations between our main outcome variables; note that most variables are binary.

Discuss politics often. Indicator for respondents answering "frequently" when asked "When you get together with your friends or family, would you say you discuss political matters: Never, Occasionally, or Frequently."

Political knowledge scale. Summative rating scale combining five correct-incorrect questions asking whether a respondent can identify the name of politicians in the following positions: vice-president, finance minister, own representative, state governor, local government chairman/woman. A summative rating scale averages observed responses across all scale items. All five were asked in 1999, and none were asked in 2001, 2003, 2007 or 2013; respondents were asked to name their representative in 2005 and 2009, their local government head and vice-president in 2005, and finance minister in 2009. Cronbach's alpha inter-item scale reliability score of 0.52.

News scale. Summative rating scale combining five-point ordinal responses to the question "How often do you get news from the following sources: television/Radio/Newspapers?" A summative rating scale averages observed responses across all scale items. We pre-standardized all items to address biases creates by missing values. Cronbach's alpha inter-item scale reliability score of 0.69.

Registered voter. Indicator coded 1 if respondent is a registered voter.

Voted. Indicator coded 1 if respondent voted in most recent federal election. This question was not asked in the 2001 or 2007 survey waves.

Attend demonstration. Indicator coded 1 if the respondent attended a demonstration or protest march during the last year.

Contact local councilor/representative. Indicator coded 1 if the respondent contacted a local government councilor/representative to the National Assembly at least once in the last year.

Attend community meeting. Indicator coded 1 if the respondent attended a community meeting once or twice, several times or often in the last year.

Active association member. Indicator coded 1 if the respondent is an active member or official leader in a voluntary association or community group.

Female. Indicator coded 1 if respondent is female.

Religion. Set of indicators coded 1 for respondents identifying as Christian, Muslim, no religion, other religion or traditional religion.

Urban. Indicator coded 1 if respondent lives in an urban area.

Year of birth. We calculate year of birth by subtracting a respondent's stated age from the year of the survey. We use this to define our post-UPE reform variable and our cohort fixed effects. Month of birth was not available.

Contact over community issue. Indicator coded 1 if the respondent contacted a formal

leader regarding a community problem during the last year. Not asked in 1999, 2001, 2005, 2008 and 2013 surveys.

Contact over private issue. Indicator coded 1 if the respondent contacted a formal leader regarding a personal problem during the last year. Not asked in 1999, 2001, 2005, 2008 and 2013 surveys.

Ethnic over national group. Indicator coded 1 for respondents strongly or weakly identifying themselves as part of their ethnic group as opposed to Nigeria.

Support united Nigeria. Indicator coded 1 for respondents agreeing or strongly agreeing that Nigeria should not split because there are problems. Not asked in 1999, 2005, 2008 and 2013 surveys.

Political violence unjustified. Indicator coded 1 if the respondent agrees or strongly agrees that "The use of violence is never justified in Nigerian politics." Not asked in 2008 and 2013 surveys.

Active religious association member. Indicator coded 1 if the respondent is an active member or official leader in a religious association.

Contact religious/traditional leader. Indicator coded 1 if the respondent contacted a religious/traditional leader at least one during the last year. Not asked in 2013 survey.

Government handling issues scale. Summative scale combining seven indicators that the government handles economy/employment/inflation/health/education/inequality/water-related issues well or very well as opposed to badly or very badly.

Government performance scale. Summative scale combining three indicators that the respondent approves or strongly approves the performance of the president/National Assembly/local government.

Trust in government scale. Summative scale combining five indicators that the respondent trusts somewhat or a lot in the president/National Assembly/electoral commission/local government/ruling party.

Government corruption scale. Summative scale combining three indicators that the respondent considers the elected officials at the presidency/National Assembly/local government corrupt or very corrupt.

Inequality has improved. Indicator coded 1 if the respondent agrees that Nigeria is better or much better in terms of the gap between rich and poor.

Feel close to PDP. Indicator coded 1 if the respondent feels close or very close to PDP.

Post-UPE. Indicator coded 1 for respondents born in 1970 or later.

Religious fragmentation index. LGA religion Herfindahl index, using data from the HNLSS.

Religious competition. Variable defined by $1 - |n_1 - n_2|$ where n_1 and n_2 are the LGA share of the population for the two largest religious groups, using data from the HNLSS.

Religious majority. Indicator coded 1 if the respondent adheres to the majority religion in their LGA of residence (defined by HNLSS data).

Main ethnic group. Indicator coded 1 if the respondent belongs to the main LGA ethnic group defined either using the main language spoken in the LGA (from the 1998 Local Government Yearbook) or the ethnic group of the majority surveyed in his or her LGA.

3

Intensity. Calculated as follows:

$$\text{Intensity}_{g,l} = 1 - \overline{\text{Primary schooling}}_{g,l} \tag{1}$$

where $\overline{\text{Primary schooling}}_{g,l}$ is the proportion of the LGA (l) population by gender (g) that completed primary school among the cohorts born between 1960 and 1969, as measured by the HNLSS; see robustness checks for alternative definitions. This is then interacted with a dummy *Post-UPE* for cohorts affected by UPE—those born after 1969—to produce our excluded instrument.

Table A1: Summary statistics

	Obs.	Mean	Std. dev.	Min.	Max.	Waves not asked
Dependent variables						
Discuss politics often	16,190	0.20	0.40	0	1	
News scale	16,273	0.00	1.00	-2.42	1.66	
Political knowledge scale	7,547	0.00	1.00	-1.24	2.07	2001, 2003, 2007, 2013
Registered voter	7,510	0.78	0.42	0	1	2001, 2007, 2008, 2013
Voted	11,974	0.64	0.48	0	1	2001, 2007
Attend demonstration	16,072	0.14	0.35	0	1	
Contact local councilor	16,240	0.16	0.37	0	1	
Contact representative	16,212	0.06	0.24	0	1	
Attend community meeting	16,191	0.48	0.50	0	1	
Active association member	16,206	0.29	0.46	0	1	
Political violence unjustified	11,539	0.72	0.45	0	1	2008, 2013
Active religious association member	16,271	0.55	0.50	0	1	
Ethnic over national group	15,959	0.26	0.44	0	1	
Support united Nigeria	6,410	0.80	0.40	0	1	1999, 2005, 2008, 2013
Contact about community issue	4,436	0.22	0.42	0	1	1999, 2001, 2003, 2007, 2013
Contact about private issue	4,436	0.37	0.48	0	1	1999, 2001, 2003, 2007, 2013
Contact religious leader	13,963	0.46	0.50	0	1	2013
Contact traditional leader	13,956	0.23	0.42	0	1	2013
Government handling issues scale	16,213	0.36	0.33	0	1	
Government performance scale	12,961	0.43	0.40	0	1	1999
Trust in government scale	16,159	0.33	0.37	0	1	
Government corruption scale	12,766	0.52	0.44	0	1	1999
Inequality has improved	8,532	0.13	0.34	0	1	1999, 2008, 2013
Feel close to PDP	7,211	0.26	0.44	0	1	2005, 2007, 2008, 2013
Education variables						
Education	16,289	2.89	1.58	0	5	
Incomplete primary schooling	16,289	0.83	0.37	0	1	
Complete primary schooling	16,289	0.78	0.41	0	1	
Incomplete secondary schooling	16,289	0.68	0.47	0	1	
Complete secondary schooling	16,289	0.53	0.50	0	1	
UPE reform variables						
Post-UPE	16,289	0.73	0.44	0	1	
Intensity	16,289	0.39	0.31	0	1	
Post-UPE × Intensity	16,289	0.28	0.32	0	1	
Control variables						
Female	16,289	0.49	0.50	0	1	
Christian	16,289	0.56	0.50	0	1	
Muslim	16,289	0.41	0.49	0	1	
No religion	16,289	0.00	0.07	0	1	
Other religion	16,289	0.01	0.09	0	1	
Traditional religion	16,289	0.02	0.14	0	1	
Urban	16,289	0.53	0.50	0	1	
Year of birth	16,289	1975.21	10.10	1950	1995	
Survey year	16,289	2004.90	4.49	1999	2013	
Religious fragmentation index	16,289	0.20	0.18	0	0.64	
Religious competition	16,289	0.26	0.30	0	1.00	
Religious majority	16,289	0.82	0.39	0	1.00	
Main ethnic group	8,952	0.66	0.47	0	1	1999, 2001, 2003

Table A2: Outcome pairwise correlation matrix

	Discuss politics often	News scale	Political knowledge scale	Registered voter	Voted	Attend demonstration	Contact local councilor	Contact representative	Attend community meeting	Active association member
Discuss politics often	1									
News scale	0.1715	1								
Political knowledge scale	0.1584	0.1225	1							
Registered voter	0.1232	0.1154	0.1515	1						
Voted	0.1213	0.0549	0.1263	0.6856	1					
Attend demonstration	0.1089	0.1291	0.0775	0.0506	0.0359	1				
Contact local councilor	0.1555	0.089	0.1325	0.0861	0.0859	0.2079	1			
Contact representative	0.119	0.0661	0.0492	0.0135	0.0313	0.1775	0.4674	1		
Attend community meeting	0.1445	0.0544	0.1357	0.1552	0.147	0.2625	0.248	0.1369	1	
Active association member	0.1504	0.0952	0.1312	0.1405	0.1154	0.2	0.2241	0.147	0.3666	1

Note: All correlations are statistically significant.

3 Graphical representation of first stage by education level
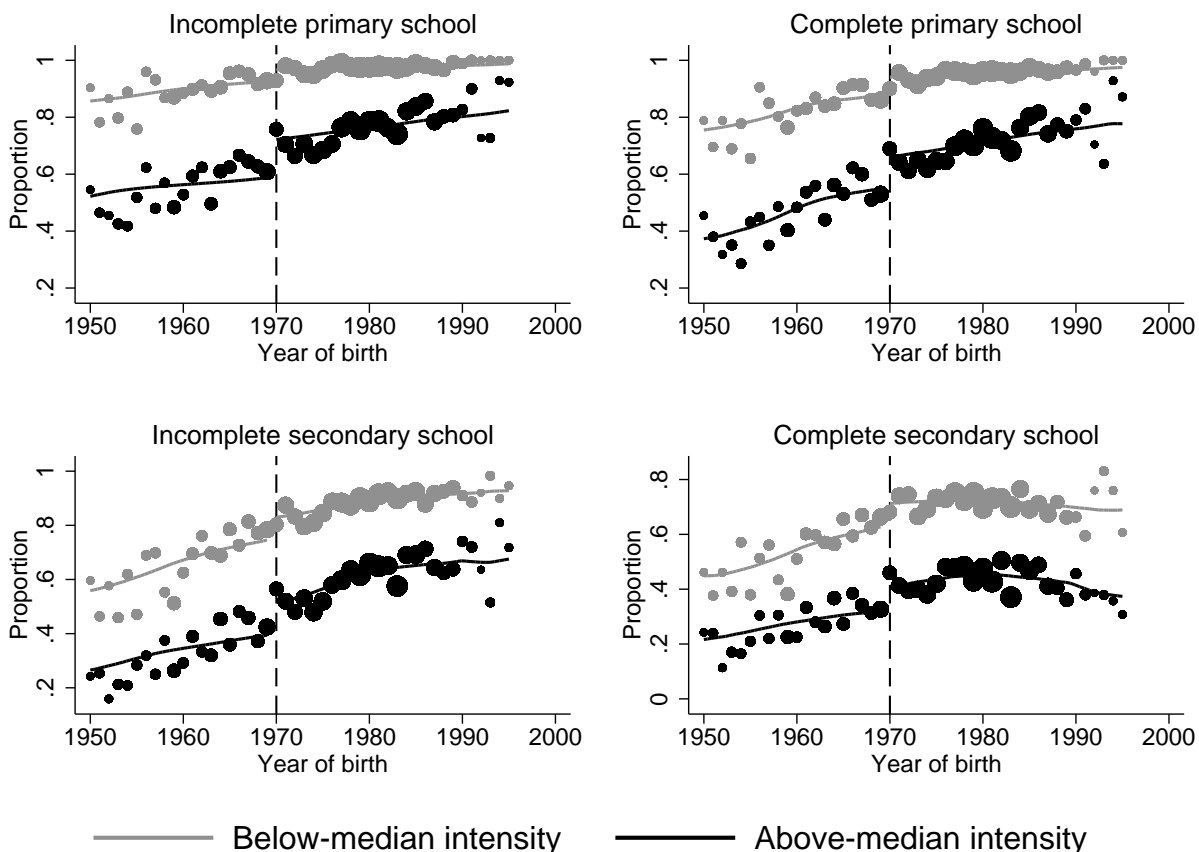
Figure A2: Trends in education level by UPE intensity (sources: Afrobarometer and HNLSS)

Notes: Each dot represents the average education score by cohort in above- and below-median UPE intensity cases. The size of the dot reflects the quantity of data in our sample. We overlay local polynomial curves (bandwidth of 2) to show trends in education across treatment intensity. Above-median and below-median intensity denote respondents above and below the sample median LGA-gender UPE intensity score. The graphs show similar pre-trends across above- and below-median intensity groups.

4 Additional results cited in the main paper

Table A3 provides the robustness tests for the outcomes not included in the main paper. With the exception of the migration check in panel A, which removes a large proportion of

treated units, the results strongly support our main findings through a mix of placebo tests, correlations in specific subsamples and controls. In the case of panel A, the reduced form effects for discussing politics and active association membership are similar in magnitude but experience increased standard errors. Only in the case of our news scale is our estimate somewhat smaller; although this result is robust to most of our checks, it is the more sensitive of our findings.

Table A4 shows that the results are highly robust to two alternative definitions of UPE intensity. These results suggest that our measure of intensity is accurately capturing variation in the impact of the reform. Table A5 shows similar reduced form estimates when partially treated cohorts are removed from the sample, although we unsurprisingly lose precision from dropping a significant fraction of the sample (as the weak first stage shows).

Finally, Tables A6 and A7 show heterogeneous effects by religion and ethnic group for the variables not included in the main paper. Table A8 shows the results for the additional indicators of ethno-religious division and possible conflict cited in the paper. The variables used in Table A8 and in the heterogeneous effects section of the main paper are defined above, while summary statistics are provided in Table A1. Note that ethnic group variables are only available for 2005-2013, so the sample is substantially reduced. Furthermore, given that most surveys did not ask about contacting local leaders about community and private problems, the first stage for those specifications is very weak ($F = 0.9$) and cause the 2SLS estimates to be unreliable. For this regressions, the reader should rely on the reduced form estimates.

Table A9 interacts the effect of education with the share of their six primary school years that occurred under democracy. The results show that, with the exception of attending community meetings, attending primary school under democracy has no differential effect on civic and political engagement. Finally, A10 provides little evidence that the UPE reform has affected evaluations of and attitudes toward the government.

All other checks cited in the main text can be replicated using our replication materials.

Table A3: Robustness checks—all other outcomes

	Discuss politics often (1)	News scale (2)	Registered voter (3)	Attend demo. (4)	Contact local councilor (5)	Contact rep. (6)	Active association member (7)
Panel A: Born before 1976 only (reduced form)							
Post-UPE × Intensity	0.094*	0.221	0.101	0.044	0.096*	-0.030	0.080
	(0.055)	(0.143)	(0.068)	(0.038)	(0.051)	(0.028)	(0.082)
Observations	7,222	7,253	4,075	7,185	7,242	7,224	7,237
Panel B: Without LGAs with a 2006 population exceeding 500,000 (2SLS)							
Intensity	0.143**	0.624***	0.301**	0.068	0.156**	0.028	0.187**
	(0.060)	(0.123)	(0.145)	(0.059)	(0.062)	(0.031)	(0.093)
Observations	14,682	14,759	6,750	14,572	14,730	14,705	14,696
Panel C: Correlation with intensity, born after 1969 only							
Intensity	-0.023	-0.266**	-0.016	0.018	0.006	-0.018	-0.020
	(0.031)	(0.128)	(0.053)	(0.023)	(0.021)	(0.017)	(0.037)
Observations	11,813	11,877	5,085	11,719	11,853	11,840	11,816
Panel D: LGA fixed effects and LGA-specific cohort trends (2SLS)							
Education	0.142**	0.478***	0.205**	-0.008	0.183***	0.061*	0.152**
	(0.067)	(0.152)	(0.102)	(0.046)	(0.057)	(0.034)	(0.076)
Observations	16,190	16,273	7,510	16,072	16,240	16,212	16,206
Panel E: State fixed effects and no cohort trends (2SLS)							
Education	0.128***	0.278***	0.285*	-0.021	0.153**	0.029	0.263***
	(0.048)	(0.101)	(0.166)	(0.045)	(0.068)	(0.033)	(0.083)
Observations	16,190	16,273	7,510	16,072	16,240	16,212	16,206
Panel F: 1965 placebo reform, born before 1970 only (reduced form)							
1965 Placebo × Intensity	0.056	0.248**	-0.089	-0.100***	-0.075	0.008	-0.088
	(0.053)	(0.118)	(0.062)	(0.032)	(0.045)	(0.037)	(0.070)
Observations	4,377	4,396	2,425	4,353	4,387	4,372	4,390
Panel G: 1960 placebo reform, born before 1965 only (reduced form)							
1960 placebo × Intensity	0.023	0.082	-0.017	-0.117*	-0.105	-0.047	-0.063
	(0.081)	(0.224)	(0.099)	(0.068)	(0.090)	(0.041)	(0.096)
Observations	2,771	2,787	1,544	2,759	2,786	2,777	2,787
Panel H: State-post reform fixed effects (2SLS)							
Education	0.156**	0.639***	0.288**	0.060	0.154**	0.025	0.189**
	(0.062)	(0.133)	(0.146)	(0.063)	(0.060)	(0.030)	(0.094)
Observations	16,190	16,273	7,510	16,072	16,240	16,212	16,206
Panel I: Controlling for pre-treatment covariates by UPE eligibility (2SLS)							
Education	0.160***	0.597***	0.288**	0.073	0.160***	0.038	0.166*
	(0.057)	(0.137)	(0.142)	(0.062)	(0.053)	(0.030)	(0.088)
Observations	16,190	16,273	7,510	16,072	16,240	16,212	16,206
Panel J: Allowing students to be partially affected by UPE (2SLS)							
Education	0.152***	0.258**	0.319	-0.045	0.149*	0.038	0.341***
	(0.059)	(0.128)	(0.213)	(0.052)	(0.084)	(0.038)	(0.120)
Observations	16,190	16,273	7,510	16,072	16,240	16,212	16,206

Notes: All specifications are estimated using OLS, and include a linear intensity term, state-specific cohort trends for cohorts affected and not affected by UPE, and religion, rural-urban, gender, state, cohort, and survey dummies. Reduced form and 2SLS specifications include the linear intensity term. The number of observations across outcomes varies due to data availability (see Table 1 in the paper). State-clustered standard errors in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A4: Alternative measures of intensity (2SLS estimates)

	Discuss politics often (1)	News scale (2)	Political knowledge scale (3)	Registered voter (4)	Voted (5)	Attend demonstration (6)	Contact local councilor (7)	Contact rep. (8)	Attend community meeting (9)	Active association member (10)
Panel A: Intensity defined by cohort born between 1960 and 1964										
Education	0.175***	0.181	0.572**	0.335	0.284*	-0.053	0.164*	0.029	0.256*	0.277***
	(0.061)	(0.119)	(0.232)	(0.213)	(0.150)	(0.059)	(0.087)	(0.039)	(0.143)	(0.107)
Observations	16,190	16,273	7,547	7,510	11,974	16,072	16,240	16,212	16,191	16,206
First stage F statistic	10.5	10.6	5.8	6.0	7.1	11.1	10.5	10.7	11.7	11.5
Panel B: Intensity defined by incomplete primary schooling										
Education	0.126**	0.251**	0.436**	0.300	0.237**	-0.033	0.172**	0.036	0.268**	0.289***
	(0.057)	(0.113)	(0.170)	(0.187)	(0.116)	(0.052)	(0.077)	(0.036)	(0.128)	(0.100)
Observations	16,190	16,273	7,547	7,510	11,974	16,072	16,240	16,212	16,191	16,206
First stage F statistic	11.1	10.9	7.6	6.6	8.5	11.5	11.0	11.3	11.9	12.1

Notes: Panel A defines UPE intensity using cohorts born between 1960 and 1965, while panel B defines UPE intensity using the proportion with incomplete (rather than complete) primary schooling for the cohorts born between 1960 and 1969. Otherwise, specifications are identical to those in the main paper. * denotes $p < 0.1$,

** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A5: Removing partially treated cohorts

	Discuss politics often (1)	News scale (2)	Political knowledge scale (3)	Registered voter (4)	Voted (5)	Attend demonstration (6)	Contact local councilor (7)	Contact rep. (8)	Attend community meeting (9)	Active association member (10)
Panel A: Reduced form (OLS)										
Post-UPE × Intensity	0.118**	0.365***	0.360**	0.110*	0.074	0.006	0.059	0.016	0.099	0.107*
	(0.048)	(0.118)	(0.173)	(0.062)	(0.062)	(0.041)	(0.047)	(0.026)	(0.070)	(0.061)
Observations	14,584	14,664	6,727	6,629	10,795	14,478	14,639	14,617	14,589	14,603
Panel B: Instrumental variables (2SLS)										
Education	0.251**	0.768***	1.084	0.427	0.206	0.014	0.125	0.032	0.217	0.225
	(0.109)	(0.211)	(0.690)	(0.356)	(0.173)	(0.086)	(0.093)	(0.051)	(0.153)	(0.144)
Observations	14,584	14,664	6,727	6,629	10,795	14,478	14,639	14,617	14,589	14,603
First stage F statistic	6.7	7.1	2.0	1.9	3.4	6.5	6.8	6.9	6.4	6.8

Notes: Both panel drop cohorts born between 1965 and 1969. Otherwise, specifications are identical to those in the main paper. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A6: Differences in UPE intensity's effect by religion and ethnic group 1—all other outcomes

	Discuss politics often	News scale	Registered voter	Attend demonstration	Contact local councilor	Contact rep.	Active association member
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A							
Above-median LGA religious fragmentation only							
Post-UPE × Intensity	0.192***	0.430***	0.230***	0.045	0.128**	-0.005	0.186**
	(0.051)	(0.113)	(0.055)	(0.044)	(0.053)	(0.028)	(0.082)
Observations	8,074	8,117	3,791	8,050	8,104	8,094	8,098
Below-median LGA religious fragmentation only							
Post-UPE × Intensity	0.001	0.305*	0.077	0.019	0.067	0.066**	0.049
	(0.063)	(0.160)	(0.098)	(0.058)	(0.041)	(0.031)	(0.067)
Observations	8,116	8,156	3,719	8,022	8,136	8,118	8,108
Panel B							
Above-median religious competition LGAs only							
Post-UPE × Intensity	0.197***	0.373***	0.150*	0.021	0.125**	0.002	0.123
	(0.057)	(0.109)	(0.078)	(0.045)	(0.060)	(0.030)	(0.087)
Observations	7,777	7,819	3,644	7,755	7,807	7,798	7,802
Below-median religious competition LGAs only							
Post-UPE × Intensity	0.034	0.319**	0.072	0.030	0.073*	0.051*	0.122*
	(0.052)	(0.138)	(0.098)	(0.053)	(0.043)	(0.030)	(0.072)
Observations	8,413	8,454	3,866	8,317	8,433	8,414	8,404

Note: See Table A3.

Table A7: Differences in UPE intensity's effect by religion and ethnic group 2—all other outcomes

	Discuss politics often	News scale	Registered voter	Attend demonstration	Contact local councilor	Contact rep.	Active association member
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel C							
LGA religious minority members only							
Post-UPE × Intensity	0.165*	0.693***	0.206*	0.013	0.134	0.020	0.196
	(0.085)	(0.172)	(0.116)	(0.087)	(0.118)	(0.057)	(0.137)
Observations	2,953	2,964	1,456	2,937	2,956	2,956	2,952
LGA religious majority members only							
Post-UPE × Intensity	0.084**	0.301***	0.135**	0.046	0.090***	0.020	0.093*
	(0.041)	(0.087)	(0.060)	(0.045)	(0.031)	(0.022)	(0.053)
Observations	13,237	13,309	6,054	13,135	13,284	13,256	13,254
Panel D							
LGA non-main ethnic group members only							
Post-UPE × Intensity	0.129	-0.058	0.484***	0.148	0.399***	0.141*	0.222*
	(0.112)	(0.213)	(0.153)	(0.132)	(0.099)	(0.081)	(0.129)
Observations	3,035	3,056	695	2,983	3,040	3,031	3,032
LGA main ethnic group members only							
Post-UPE × Intensity	0.185**	0.545***	0.113	0.056	0.219***	0.055	0.252***
	(0.070)	(0.188)	(0.091)	(0.046)	(0.064)	(0.049)	(0.079)
Observations	5,867	5,892	1,496	5,788	5,874	5,863	5,874

Notes: See Table A6.

Table A8: Effect of education on contacting politicians and local leaders

	Contact about community issue			Contact about private issue			Contact religious leader			Contact traditional leader		
	(1) OLS	(2) OLS	(3) 2SLS	(4) OLS	(5) OLS	(6) 2SLS	(7) OLS	(8) OLS	(9) 2SLS	(10) OLS	(11) OLS	(12) 2SLS
Education	0.029*** (0.008)		0.801 (0.867)	0.015** (0.007)		-0.538 (0.598)	0.033*** (0.005)		-0.083 (0.081)	0.015*** (0.005)		0.041 (0.074)
Intensity× Post-UPE		0.194** (0.082)			-0.130 (0.095)			-0.055 (0.051)			0.026 (0.049)	
Observations	4,436	4,436	4,436	4,436	4,436	4,436	13,963	13,963	13,963	13,956	13,956	13,956
Outcome mean	0.22	0.22	0.22	0.37	0.37	0.37	0.46	0.46	0.46	0.23	0.23	0.23
Outcome std. dev.	0.42	0.42	0.42	0.48	0.48	0.48	0.50	0.50	0.50	0.42	0.42	0.4
First stage F statistic			0.9			0.9			23.1			21.9

Notes: See Table A3.

Table A9: Interaction of education with share of schooling under democracy (2SLS estimates)

	Discuss politics often (1)	News scale (2)	Political knowledge scale (3)	Voted (4)	Attend demonstration (5)	Contact local councilor (6)	Contact rep. (7)	Attend community meeting (8)	Active association member (9)
Education	0.164***	0.673***	0.876***	0.245*	0.074	0.177***	0.033	0.180	0.177*
	(0.063)	(0.142)	(0.296)	(0.144)	(0.062)	(0.065)	(0.030)	(0.112)	(0.099)
Education × democracy at school	-0.014	-0.085	-0.064	-0.027	-0.009	-0.033	-0.001	0.058*	0.026
	(0.021)	(0.060)	(0.097)	(0.034)	(0.014)	(0.020)	(0.011)	(0.030)	(0.025)
Observations	16,190	16,273	7,547	11,974	16,072	16,240	16,212	16,191	16,206
First stage F statistic	11.5	11.6	4.8	6.2	11.3	11.1	11.2	11.1	11.3

Notes: The share of education under democracy is the proportion of years in primary school under a democratically elected government. State-clustered standard errors in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A10: Effect of UPE intensity and education on incumbent performance evaluations and closeness to ruling party

	Govt. handling issues scale	Govt. performance scale	Trust in govt. scale	Govt. corruption scale	Inequality has improved	Feel close to PDP
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Correlation (OLS)						
Education	0.010***	0.010**	0.002	-0.002	0.010***	0.019***
	(0.004)	(0.005)	(0.003)	(0.003)	(0.003)	(0.005)
Panel B: Reduced form (OLS)						
Post-UPE × Intensity	-0.034	0.011	0.030	-0.047	-0.030	-0.023
	(0.033)	(0.046)	(0.035)	(0.031)	(0.047)	(0.056)
Panel C: Instrumental variables (2SLS)						
Education	-0.056	0.022	0.051	-0.098	-0.051	-0.031
	(0.061)	(0.094)	(0.059)	(0.067)	(0.083)	(0.074)
Observations	16,213	12,961	16,159	12,766	8,532	7,211
Outcome mean	0.36	0.43	0.33	0.52	0.13	0.26
Outcome standard deviation	0.33	0.40	0.37	0.44	0.34	0.44
First stage F statistic	23.1	15.0	20.6	12.4	17.0	17.3

Notes: Specifications in panels A and B are estimated using OLS, and include state-specific cohort trends for cohorts affected and not affected by UPE and religion, rural-urban, 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 Online Appendix). State-clustered standard errors in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

References

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