

Working Paper No. 156

Deliberate disengagement: How education decreases political participation in electoral authoritarian regimes

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Abstract

A large literature examining advanced and consolidating democracies suggests that education increases political participation. However, in electoral authoritarian regimes, educated voters may instead deliberately disengage. If education increases critical capacities, political awareness, and support for democracy, educated citizens may believe that participation is futile or legitimates autocrats. We test this argument in Zimbabwe – a paradigmatic electoral authoritarian regime – by exploiting cross-cohort variation in access to education following a major educational reform. We find that education decreases political participation, substantially reducing the likelihood that better-educated citizens vote, contact politicians, or attend community meetings. Consistent with deliberate disengagement, education's negative effect on participation dissipated following 2008's more competitive election, which (temporarily) initiated unprecedented power sharing. Supporting the mechanisms underpinning our hypothesis, educated citizens experience better economic outcomes, are more interested in politics and more supportive of democracy, but are also more likely to criticize the government and support opposition parties.

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Introduction

The question of what motivates political participation is central to political science. Participation is essential for holding governments to account and for influencing incumbents to implement the policies that citizens demand. A vast literature asserts that education is a major driver of political participation, as well as many other forms of non-contentious civic action (Almond & Verba, 1963). La Due Lake and Huckfeldt (1998:567) argue that the positive relationship between education and political participation is “one of the most reliable results in empirical social science.” In a similar vein, Hillygus (2005:25) states that the idea that education is a primary driver of increased political participation is “largely uncontested,” while Putnam (1995:68) posits that education “is the best individual level predictor of participation.”

There are, however, several problems with these law-like assertions. First, isolating the effect of education – as distinct from innate ability (Spence, 1973), socioeconomic status (Jennings & Niemi, 1968), and family background (Nie, Junn, & Stehlik-Barry, 1996) – on political participation is a formidable challenge. Debates between “education as cause” vs. “education as proxy” remain far from settled (Berinsky & Lenz, 2011; Kam & Palmer, 2008). Second, with few exceptions, the existing empirical literature investigating the causal link between education and political participation has generally focused on a small set of rich advanced democracies (Sondheimer & Green, 2010). This is problematic, because existing accounts of the positive effect of education on participation implicitly assume that countries have an institutional environment and a civic culture that might be absent in many developing countries.

Third, analyses in developing countries have primarily focused on aggregate correlations between education and the likelihood that autocratic regimes transition to democracy.¹ Here, too, scholars have long assumed that education is a critical driver of political participation. For example, Deutsch (1961), Lerner (1958), and Lipset (1959) all posited that more-educated citizens are central to democracy. Indeed, Huntington (1991) explicitly claimed that education contributed to the “Third Wave of Democratization” in the 1970s and 1980s. Given that the nature of dictatorial regimes has dramatically changed over the past two decades (Schedler, 2013), and considering the dearth of individual-level analyses in this area, the nature of the relationship between education and nonviolent political participation in electoral authoritarian settings remains poorly understood.

We address this gap in the literature by examining the causal relationship between education and non-contentious political participation under electoral authoritarianism. Electoral authoritarian regimes are a hybrid: While they permit some popular participation and elite contestation by holding periodic elections, they fall a long way short of genuine democracy. In such regimes, elections – while not purely *pro forma* – are far from fair, the government is almost assured of remaining in power, and many other forms of political action are closely monitored (and often limited) by the regime (Levitsky & Way, 2010). What political role do more-educated citizens play in such contexts? We argue that the positive relationship between education and political participation does not necessarily apply in electoral authoritarian settings. We further argue that in electoral authoritarian regimes, education can be associated with *decreased* political participation, even when education – as modernization theorists have long assumed – increases interest in politics, support for democracy, and economic status.

¹ See e.g. Acemoglu et al. (2005); Glaeser, Ponzetto, & Shleifer (2007); and Murin & Wacziarg (2014). A small literature has instead examined education’s effect on violent forms of protest (Campante & Chor, 2012b; Samii & West, 2014).

Political participation in democratic settings can be understood as the embodiment of the liberal notion of free will (Rousseau, 1997). The act of voting, in particular, has been viewed as a manifestation of political equality and individual agency (Lijphart, 1997). Yet in many non-democratic settings, regimes compel political participation as a demonstration of allegiance, rather than to aggregate social preferences or enable citizen voice (Hermet, 1978).² Elections in electoral authoritarian regimes seek to legitimize incumbents, appease the international community, and demonstrate the omnipresence of the regime (Levitsky & Way, 2002). Under such conditions, political participation loses both its normative and instrumental appeals for many.

When participation does not provide genuine input into the political process, or when it merely serves to buttress the regime, refraining from political participation can serve as a powerful form of dissent (Hermet, 1978; Karklins, 1986) or reflect the recognition that costly political action is futile (Posner & Simon, 2002). This study's key insight is that more-educated citizens are more likely to exercise such deliberate political disengagement. First, education imbues citizens with cognitive abilities that facilitate more critical thinking, which may result in lower levels of support for the incumbent regime and thus less interest in legitimizing it with their participation.³ Second, education may similarly lead to value change, with more-educated individuals placing a higher premium on democratic values such as self-expression and individual voice than on social conformity (Inglehart & Welzel, 2005). Third, if education also increases knowledge and understanding of politics, educated voters may be more aware that political participation is unlikely to affect political outcomes. Fourth, educated individuals may feel a higher level of disillusionment with autocratic politics and economic mismanagement, given their greater economic potential (Campante & Chor, 2012a).

We test our disengagement argument using the case of Zimbabwe, a paradigmatic electoral authoritarian regime ruled by President Robert Mugabe and a civilian-military junta (Levitsky & Way, 2010). While elections have been held regularly since 1980, the incumbent regime has used a combination of intimidation, violence, manipulation of legal rules, and vote rigging to maintain power. Thus, as in many electoral authoritarian regimes, elections in Zimbabwe provide some restricted opportunities for public opinion to be registered without offering voters a genuine ability to determine the ultimate distribution of power. The 2008 election was more competitive, ultimately producing a power-sharing arrangement between Mugabe and opposition leader Morgan Tsvangirai, and thus presents a valuable opportunity to compare the effects of education in more and less politically competitive contexts.

Furthermore, Zimbabwe is an excellent case study because we are able to leverage a major policy reform to identify the causal relationship between education and participation. Immediately after majority rule was achieved in 1980, Zimbabwe implemented a far-reaching education reform that greatly increased access to education. The reform substantially increased access to secondary education for black students and induced some students to remedially attend primary school. We exploit this natural experiment to analyse the effects of education by comparing cohorts that were just young enough to enjoy greater access to secondary education to those that were just too old. To deal with non-compliance across cohorts, we also estimate instrumental variable (IV) regressions. Our primary result is that, in Zimbabwe, education *reduces* levels of political participation.

² This may be true in a broad range of African settings. Using Afrobarometer data, Bratton, Mattes, and Gyimah-Boadi (2005) find that “political participation – or at least voting, communing, and contacting – is predominantly mobilized” from above, rather than autonomous.

³ E.g. educated citizens may be able to critically evaluate regime propaganda, or may have the interest or financial means to access more critical foreign media.

Contrary to the conventional wisdom, a higher level of education reduces not only voting, but also other forms of non-contentious participation such as contacting one's local councillor and attending community meetings. Consistent with our argument that non-participation appears to be an informed choice by relatively cognizant and politically aware citizens, we find that education significantly increases economic well-being, interest in politics, and support for democracy. Furthermore, consistent with the idea that education facilitates critical thinking, we also find that increased education decreases support for the ruling party, reduces perceptions of government performance, and increases support for the main opposition party. Finally, compatible with the idea that educated citizens will re-engage with politics when the political sphere allows for more meaningful contestation, we show that the large negative relationship between education and participation weakens significantly after the 2008 election. We interpret these findings as evidence that educated citizens consciously choose to withdraw from the political sphere under electoral authoritarianism.

To increase confidence in our interpretation of these results, we examine two plausible alternative explanations for our findings. First, we rule out the possibility that more-educated constituents are less integrated into patronage or vote-buying mobilization networks, which could also lead to lower turnout. Second, we show that there is no evidence in our data that educated constituents are more likely to face violent repression intended to suppress participation.

This article's main contribution is to the vast literature on the relationship between education and political participation. Notwithstanding the centrality of education in political theories of democratic citizenship, much is still unknown about the nature of the relationship between educational attainment and political attitudes and behaviour. If education reduces political participation in electoral authoritarian regimes, this provides an additional qualification to the literature that commonly assumes a law-like *positive* relationship.⁴ We show that education increases the *ability* of citizens to participate in politics and leads to greater interest in politics. However, our findings suggest that whether citizens decide to use these facilities or "deliberately withdraw" from the political arena is likely to depend on regime type. The article also contributes to the literature on political participation in developing countries, especially that which focuses on attitudes rather than resources as the key determinant of participation (e.g. Norris, 2002; Cox, 2003; Kasara & Suryanarayan, 2014).

Finally, our results demonstrate that the impact of education might be conditional on a country's level of political contestation, which speaks to a growing cross-country literature that seeks to isolate the impact of education on democracy (Acemoglu et al., 2005; Glaeser, Ponzetto, & Shleifer, 2007; Woodberry, 2012). By discussing why those seen by modernization theorists as "agents of change" withdraw from politics, this paper also suggests one reason why – contrary to the expectation of the democratic-transition literature – many countries that took initial steps toward liberalization got "stuck" in electoral authoritarian equilibria (Carothers, 2002). As such, our findings also contribute to our understanding of regime stability and change theories.

Related literature

The relationship between education and political participation in advanced democracies is the subject of a vast literature. Whereas early work suffered from insufficient attention to causal identification, a number of recent studies have identified a positive effect of education on political

⁴ See Berinsky and Lenz (2011) for a comprehensive review of recent challenges to the traditional view that education has a uniform positive effect in the American context.

participation in the developed world.⁵ In developing countries in general, and in Africa in particular, there is a rich descriptive literature. For example, Evans and Rose (2006; 2007) find cross-sectional associations between education and support for democracy in Africa, while Mattes and Mughogho (2009) explore correlations between education, cognitive engagement, and participation in politics. While this literature documents associations between education, political attitudes, and participation, the study of the *causal* effect of education in developing countries is somewhat less developed. Furthermore, none of the recent studies that attempt to identify a causal relationship between education and participation in developing countries have examined this question in the context of a repressive electoral authoritarian regime.⁶

Friedman et al. (2011) use a field experiment in western Kenya to study the effect of an increase in education induced by a secondary school girls scholarship program. They find that secondary education made young women from disadvantaged ethnic groups in rural areas more politically informed, less deferential to political authority, and more likely to reject gender-based violence. They do not find, however, that secondary education increases support for democracy, community participation, political efficacy, or voting. Despite its innovative design, the study only measures outcomes four to five years after initial enrolment and examines an unrepresentative population. Given the comprehensiveness of Zimbabwe's education reform, we are able to identify mass public education's long-term effects for a wide range of compliers (from a representative sample) that small-scale field experiments cannot reach.

Our findings speak most directly to two recent working papers that examine the long-term political effects of education. Wantchekon, Novta, and Klačnja (2013) use the placement of the first missionary schools in Benin as a plausible source of exogenous variation in access to education. They find that both the first generation of formally educated Beninois and their descendants are more likely to join and campaign for political parties. They do not, however, report results regarding voting behaviour and political attitudes, perhaps because their main focus is education's effect on well-being. Larreguy and Marshall (2014) exploit Nigeria's 1976 education reform to show that increased educational attainment causes more political participation in the form of voting, contacting politicians, attending community meetings, and devoting attention to political events.⁷

While the above papers arguably identify the impact of education on political participation, they all examine this relationship in contexts of genuine political contestation. While Benin, Kenya, and Nigeria cannot be classified as consolidated democracies, they have experienced *competitive* elections and turnovers of executive power in recent years.⁸ Zimbabwe, by contrast, has had no alternation in executive power since majority rule was achieved in 1980, and election rigging has been widespread since 2002. These differences are reflected, for example, in Zimbabwe's Polity-2 score, which is significantly lower than those of Kenya, Benin, and Nigeria during the period covered by Afrobarometer (see Figure 1). Nevertheless, the level of contestation in Zimbabwe is not constant: The increase in the Polity score following the 2008 election reflects an important period of power sharing in which genuine change appeared possible.

⁵ Causal evidence for a positive relationship in developed countries includes Dee (2004), Persson (2011), and Sondheimer and Green (2010). Nevertheless, Berinsky and Lenz (2011), Kam and Palmer (2008), and Tenn (2007) provide evidence that not all types of schooling increase political participation.

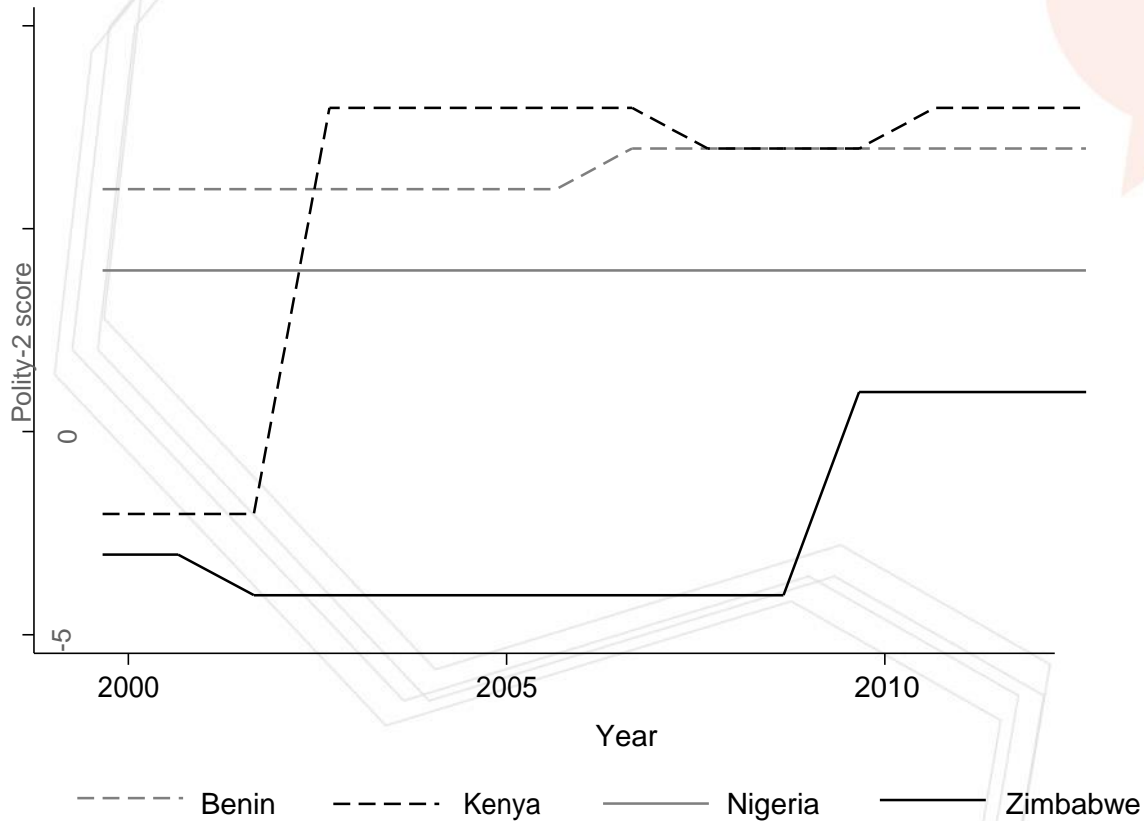
⁶ See Kuenzi (2006), Kuenzi and Lambright (2005), and MacLean (2011).

⁷ Our cohort-specific identification strategy is related also to work by Mattes (2011) and Mattes and Richmond (2015), who explore the ways in which achievement of majority rule has affected political attitudes among the post-apartheid generation in South Africa.

⁸ Benin and Kenya have experienced opposition electoral victories. In Nigeria, presidential power has alternated between northerners and southerners within the ruling party since the return to democracy in 1999.



Figure 1: Polity-2 scores by year



Politics and secondary education in Zimbabwe

Zimbabwe (then known as Rhodesia) was a British colony for much of the 20th century, with a small white settler elite, a large black African majority, and an apartheid-like set of institutions that ensured white dominance of political and economic life. In 1965, the white settler-led government declared independence from Britain in order to prolong its domination of the country. Armed resistance to white rule began in the mid-1960s and intensified after 1972, finally resulting in free elections and black majority rule in 1980. Robert Mugabe, Zimbabwe's first post-independence leader, still serves as president.

After an initial phase of violent conflict between the Zimbabwe African National Union– Patriotic Front (ZANU-PF) and its rival, the Patriotic Front–Zimbabwe African People's Union (PF-ZAPU), in 1987 the two parties merged into a Mugabe-dominated "ZANU-PF." Mugabe won 78% and 93% of the vote in the 1990 and 1996 presidential elections, respectively, while ZANU- PF won 117 out of 120 seats in the 1995 parliamentary election (Levitsky & Way, 2010).

Opposition to Mugabe's increasingly autocratic rule began to crystallize only in the late 1990s, when labour, religious, and civil-society groups, initially mobilized to enshrine term limits, formed the Movement for Democratic Change (MDC) opposition party. In 2000, the MDC defeated Mugabe's proposal for a new constitution, and later won an unprecedented 58 out of 120 seats in Parliament. Mugabe responded by increasing both political repression and the use of state resources to buy political support. In 2001, for example, he dispossessed white farmers via land invasions and

handed their farms over to ZANU-PF allies. In the 2002 presidential elections, Mugabe defeated Tsvangirai – the MDC presidential candidate – with 56% of the vote amid violence and widespread vote suppression (LeBas, 2006). Immediately after the 2005 parliamentary elections, in which ZANU-PF won 65% of the parliamentary constituencies against an internally divided MDC (Bratton, 2014), Mugabe launched Operation Murambatsvina (“Drive Out the Rubbish”), which displaced more than 700,000 people from informal urban settlements.

The 2008 elections took place in a context of agricultural collapse and macroeconomic instability, with hyperinflation at an annual rate of 231,000,000% (Bratton, 2014). Benefiting from (initially) lower levels of election-related violence than in 2002 (Levitsky & Way, 2010), the MDC won an outright parliamentary majority. Tsvangirai outpolled Mugabe in the first round of the presidential election, but the electoral commission stated that he did not achieve the 50% threshold required for victory. Prior to the second round, ZANU-PF and the military launched a brutal campaign of intimidation and beatings against MDC supporters and candidates, and Tsvangirai withdrew from the race. Mugabe won the resulting sham election by a landslide, but international pressure forced a government of national unity, with Mugabe as president and Tsvangirai as prime minister (LeBas, 2014).

The national unity government and the end of hyperinflation allowed the economy to rebound somewhat after 2009, and political violence declined. Yet despite the facade of power sharing, ZANU-PF retained de facto control, while internal divisions weakened the MDC. The 2013 elections marked the return of ZANU-PF dominance, as Mugabe comfortably beat Tsvangirai and won 70% of parliamentary seats (LeBas, 2014).

The education reform of 1980

Prior to independence, access to education for the black community was deliberately restricted. While schooling was compulsory and free for whites (until age 15), black Zimbabweans – who were not required to attend school – had to pay high school fees. In addition, black Zimbabweans were required to pass a series of increasingly difficult exams in order to continue past primary school, while continuation to the first cycle of secondary school was automatic for whites. The education budget for black Zimbabweans was tightly controlled at 2% of GDP, while out-of-pocket secondary school tuition and boarding fees cost almost two months of the average wage.⁹ The government spent about 12 times more per capita on primary schooling for whites than for blacks (Dorsey, 1989). King (2013) documents that such discriminative policies were implemented in many African colonies, which had come to associate education with greater unrest.

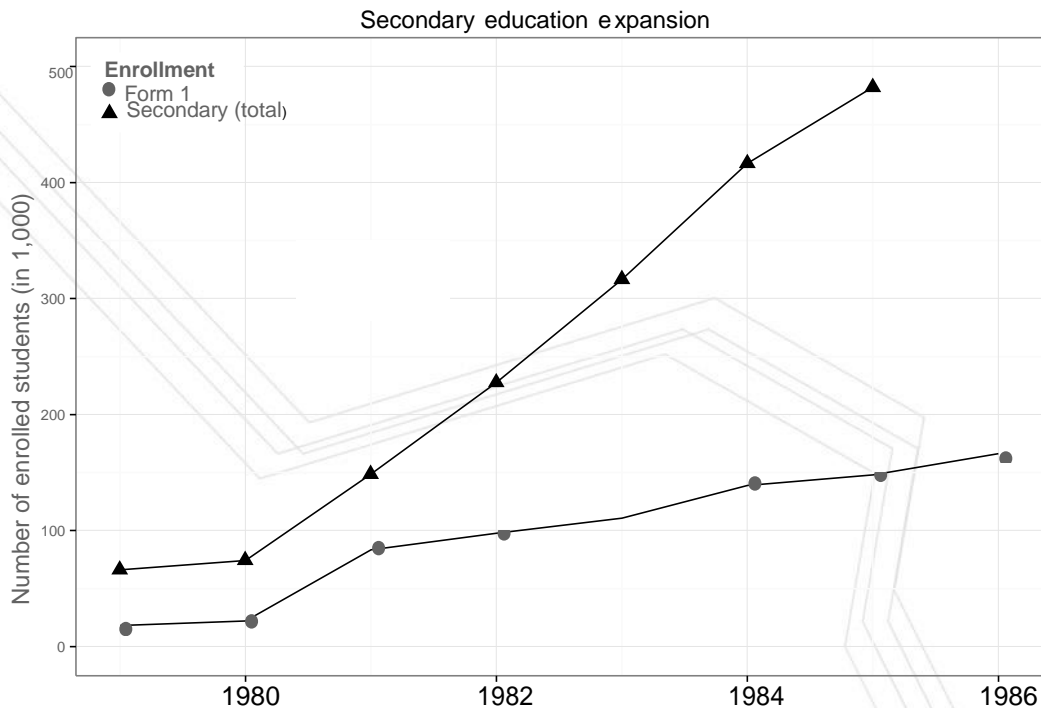
Starting in April 1980, the ZANU government implemented a wide-ranging set of educational reforms. Primary education was made free and compulsory for all Zimbabweans, regardless of colour. While some fees were applied for secondary school, automatic progression from primary to secondary school was decreed.¹⁰ Furthermore, age barriers were removed for older children, allowing those who did not start school on time to attend. The government also undertook a large-scale school-building campaign and reopened schools that had been closed during the independence war. The number of primary schools almost doubled (from 2,401 to 4,291) between 1980 and 1986, while the number of secondary schools increased seven-fold, from 177 to 1,276 (Bourne, 2011).

⁹ Authors’ calculation based on 1979 school fee data and 1977 wage data from Riddell (1980).

¹⁰ Riddell (1980) estimates that fees at more distant boarding schools were twice as high as tuition fees for secondary schooling around 1979.

The reform had an immediate effect: Overall student enrolment doubled in one year (Narman, 2003). As Figure 2 illustrates, this increase was most dramatic for secondary enrolment, which rose from 66,215 students in 1979 (7% of students) to 537,427 (19% of students) in 1986. The change is also apparent in the primary-secondary progression statistics: While in 1979 only 25% of primary-school leavers continued to secondary schools, by 1986 78% did.

Figure 2: Number of enrolled students in Form 1 (first year of secondary education) and in all secondary education grades by year | 1979-1986



Source: Government of Zimbabwe, *Annual Report of the Secretary of Education, 1986*

Based on the nature of Zimbabwe's education reform, we focus on the expansion of secondary education.¹¹ Although primary school fees were formally banned, the reform did not significantly affect primary educational attainment. This is likely because 80% of black Zimbabweans were already enrolled in primary school even under white rule and because some primary schools continued charging informal fees (Nhundu, 1992). Had the 1980 reforms significantly affected primary enrolment, we would observe a sharp discontinuity in education for the cohorts of primary school starting age in 1980, i.e. those born in 1972-74. However, we find no evidence of a discontinuity around those birth years (see Figure 5 below).¹² The reform's small effect on primary

¹¹ Agüero and Bharadwaj (2014) and Grépin and Bharadwaj (2014) similarly restrict their focus to secondary school access.

¹² Similarly, there is no discontinuous change in education levels for primary-school-aged cohorts for the 1972-74 cohorts in the Demographic and Health Surveys data. See, for example, Grépin and Bharadwaj (2014).



school completion principally reflects the remedial education of individuals whose education was interrupted by the war (Narman, 2003).¹³

Given the rapid expansion, qualified teachers could not be hired quickly enough, instructional quality often suffered, and school construction lagged behind enrolment, leading to overcrowded classrooms.¹⁴ While some slippage in quality was perhaps inevitable given the speed of the reform, it remained high enough to deliver substantial material benefits to the reform's beneficiaries, as we demonstrate below. Notwithstanding these challenges, Zimbabwe was widely recognized as a leader in expanding access to education in Africa during the 1980s (Dorsey, 1989) and provides a unique setting in which to examine the role of education in electoral authoritarian regimes that allow some restricted political contestation.

Research design

In this section we discuss the data sources, identification strategy, and estimation approaches that we use to identify the long-term causal effects of Zimbabwe's education reforms on individual political participation.

Data

To examine the effects of education on political participation in Zimbabwe, we combine all available rounds of the Afrobarometer surveys conducted in 1999, 2004, 2005, 2009, 2010, and 2012.¹⁵ Since the Afrobarometer questions change across survey rounds, different rounds may be used to test different outcome measures (see Online Appendix https://sites.sas.upenn.edu/ggros/files/onlineappendix_v1.pdf). We focus exclusively on black respondents, who comprise the overwhelming majority of Zimbabwe's population and were the education reform's target group.¹⁶

Education is our key (endogenous) explanatory variable, which is measured using the following seven-point ordinal scale: no schooling, incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete college, complete college. A one-category increase in the education measure is equivalent to about two to four years of education, given the discrete nature of the variable. Figure 3 shows the distribution of this measure in our data, indicating that the modal level of schooling is incomplete or complete secondary school.

¹³ This was facilitated by a program allowing teenagers to return to complete primary school on an abbreviated three-year schedule (Chung, 2006).

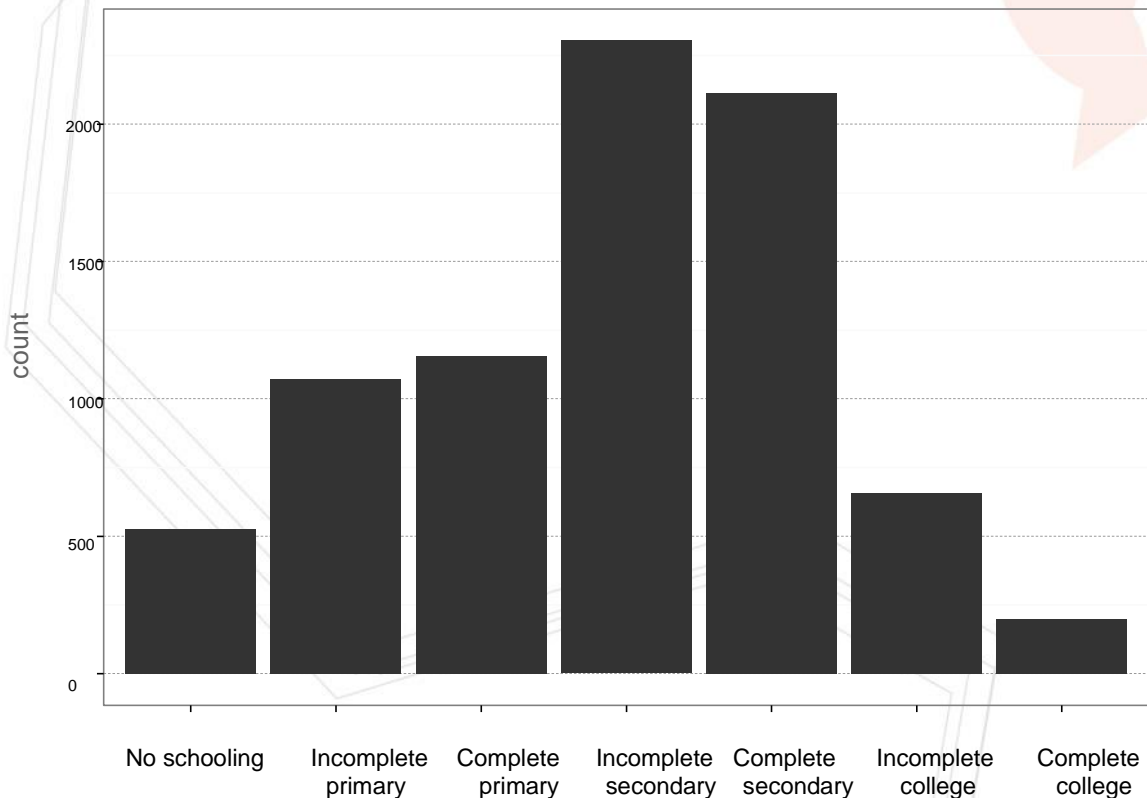
¹⁴ A construction lag could potentially violate our identification assumption if, for example, the lag was correlated with unobserved features of areas that are also correlated with support for the regime. Agüero and Ramachandran (2014), who use a similar identification strategy for health outcomes, show that while some districts indeed opened schools sooner than others, by 1983 all disparities were eliminated. Comparing the educational attainment of those born in districts in which secondary schools opened "earlier" to those born in districts that opened schools "later" shows no difference. Furthermore, Chung (2006) suggests that civil-service reformers resisted pressure to build schools based on political considerations.

¹⁵ Afrobarometer conducts nationally representative surveys on the political attitudes of citizens in selected African countries.

¹⁶ There were insufficient white voters to conduct a difference-in-differences analysis utilizing the fact that the education reform was specifically targeted at Zimbabwe's black population. However, Agüero and Ramachandran (2014) report that, using 2002 census data, there is no jump in education for white Zimbabweans at the 1980 threshold.



Figure 3: Sample distribution of educational attainment (8,020 observations)



Political participation, our principal dependent variable, is operationalized using four binary indicators. *Voted* indicates whether the respondent reported voting in the most recent legislative or presidential election. We also examine indicators for directly contacting one's local government councillor (*Contacted local councillor*), attending a community meeting (*Attended community meeting*), or joining other community members in raising an issue (*Raised issue at meeting*) within the past 12 months. Respectively, 73%, 41%, 68%, and 67% of respondents engaged in such activities.¹⁷ We then combine these four variables, which are positively correlated with a Cronbach's alpha of 0.58, into a summary index (*Participation scale*).¹⁸ Although we also present the results for each component separately, we place the greatest weight on our scale measure because it averages over the noise contained in the binary indicators.

¹⁷ We focus on non-contentious and undemanding forms of participation that relatively low-level education might reasonably affect. Consistent with their comparatively higher costs, only 19% contacted their MP or attended a demonstration, and unreported results find that education has no effect on either activity. We also examined local association membership, and found substantively similar effects to our main results below; since this variable is only available in a small number of surveys, these estimates are less precise.

¹⁸ All summary indices are constructed using the `alpha` command in Stata, which does not use case-wise deletion and therefore maximizes the available information from the constituent variables; a score is created for every observation for which there is a response to at least one item. The summative score is then divided by the number of items from which the sum is calculated.



Variation in access to secondary schooling

In order to identify the causal effect of educational attainment, we exploit the cross-cohort variation in access to secondary schooling arising from Zimbabwe's 1980 education reform. Specifically, we compare black citizens from cohorts that were just young enough to be fully or partially affected by the reform to black citizens from cohorts that were just too old to benefit from the educational expansion.

We define those born in 1967 or later, who were 13 or younger when the reform was implemented, as fully "treated" (*Secondary access* = 1). Those born in 1963 or earlier, and thus aged 17 or older in 1980, are defined as our control group that was not affected by the reform (*Secondary access* = 0). Finally, those aged 14-16 at the time of the reform's onset are considered "partially treated." Such individuals are coded according to the number of additional years of schooling available to them; for example, a black Zimbabwean aged 15 in 1980 is coded as receiving a "dosage" of one-half treatment, while individuals aged 14 and 16 are coded as receiving one-quarter and three-quarter dosages, respectively.¹⁹ This coding scheme, shown graphically in Figure 4, defines *Secondary access*, our source of exogenous variation.

Figure 4: Operationalization of *Secondary access*, the study's key treatment variable

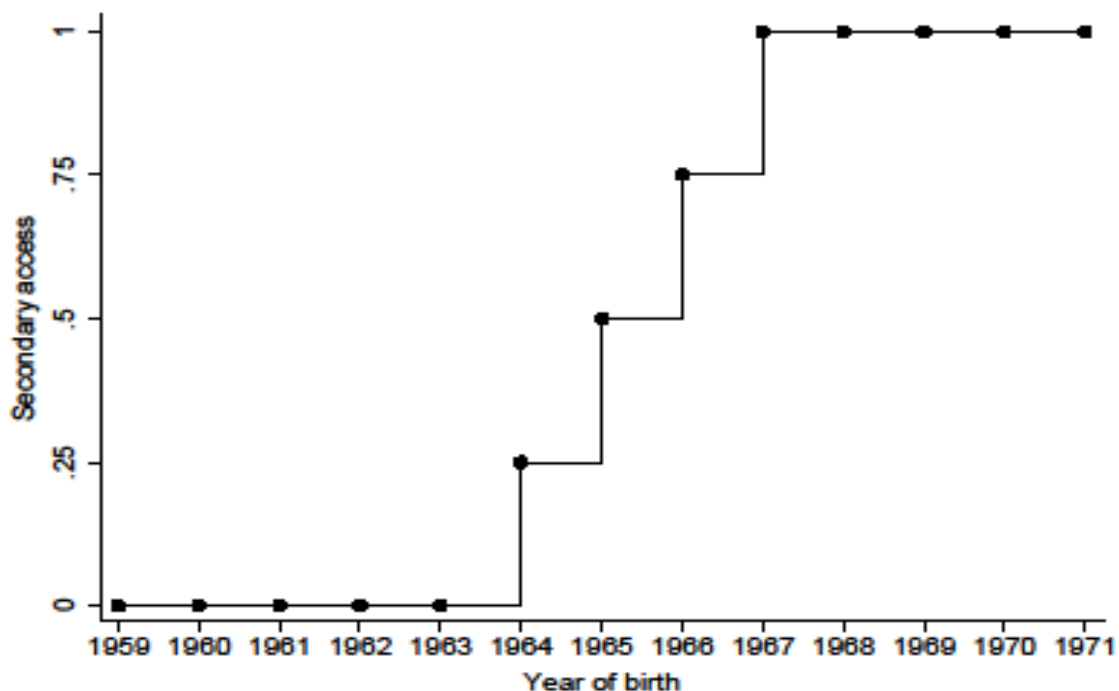
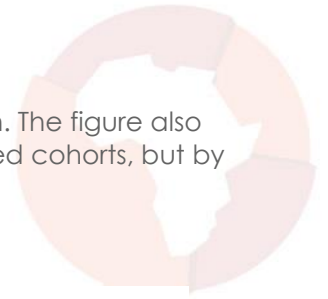


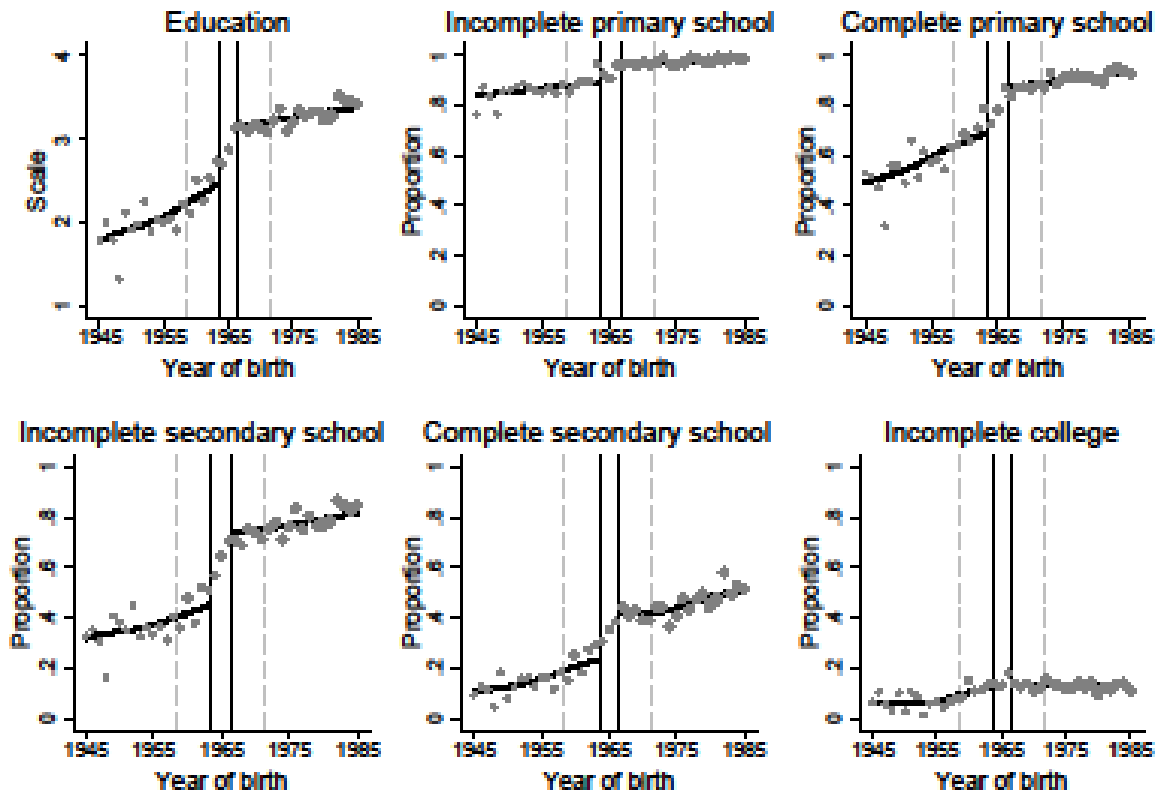
Figure 5 provides preliminary evidence that reform increased average educational attainment across cohorts. The education scale (in the top left) demonstrates that cohorts fully treated by the reform exhibit substantially higher levels of education relative to cohorts born in 1963 or earlier. The increase is large and almost equivalent to moving from complete primary to

¹⁹ This approach to partial treatment closely follows Bleakley (2010).



incomplete secondary, or from incomplete to complete secondary education. The figure also demonstrates that the reform increased the education levels of partially treated cohorts, but by less than fully treated cohorts.

Figure 5: Trends in educational attainment by cohort



Notes: Each gray dot represents average education for a given cohort (birth year). Large dots reflect larger sample sizes. Black lines are local polynomials fitted either side of the reform (indicated by the vertical dashed line). The vertical gray dashed lines indicate the bandwidth used for our main analysis.

We can identify the effects of the reform under the assumption that black Zimbabweans on both sides of the reform cut-off are effectively identical, with the exception that only some cohorts were eligible to enjoy access to secondary education. However, independence has undoubtedly brought about many important changes, and socialization processes could operate differently at different stages of life (Alwin & Krosnick, 1991; Sears & Valentino, 1997). To address such concerns, we only compare treated cohorts just young enough to be affected by the reform to control cohorts just too old to be affected. Our main analysis focuses on a “bandwidth” of five cohorts on either side of the reform cut-off years of birth (1963 and 1967). This is a powerful design, because neighbouring cohorts are subject to an almost identical economic, social, and political environment but differ due to a schooling reform that could not have been anticipated by parents prior to the reform.

In our sample of cohorts around the eligibility cut-offs, there are good reasons to believe that control cohorts differ from treated cohorts only with respect to their eligibility for secondary schooling. First, as Figure 5 indicates, trends in education are relatively flat once we focus on the

five cohorts either side of the reform's cut-off point (inside the gray dashed lines). Below, we also document flat trends in our political outcomes. Second, our robustness checks demonstrate that trends across cohorts are not driving our results by varying the bandwidth, using placebo reforms, and including flexible cohort trends either side of the reform. Third, as Figure 6 indicates, cohorts on either side of the reform are balanced across treatment groups with respect to gender, age at the date of the survey, and district-level political aggregates. Although there is a slight imbalance with respect to tribe, we show below that our findings are robust to the inclusion of pre-treatment covariates and that the effects of access to education do not vary across tribes. Finally, the frequency of surveyed individuals is not affected by the treatment, suggesting that there is no differential fertility or migration across cohorts around the reform.²⁰

Estimation strategies

Building on our key identifying assumption that access to secondary schooling is exogenous across cohorts just affected or just unaffected by the reform, we utilize two main approaches to identify the long-run effects of secondary education on political participation. Our first approach includes partially treated respondents, and thus enables us to exploit differences in treatment intensity (i.e. differential access to secondary schooling) across cohorts that were subject to essentially identical common shocks. We first estimate the reduced-form effects of increasing the availability of secondary education – which is equivalent to an “intent-to-treat” (ITT) analysis – by estimating the following regression equation using ordinary least squares (OLS):

$$Y_{icdt} = \gamma \text{Secondary access}_c + \eta_t + \varepsilon_{icdt} \quad (1)$$

where Y_{icdt} is an outcome measure and $\text{Secondary access}_c$ – our key treatment variable – allows the effect of the education reform to vary across partially treated adolescents. We include survey fixed effects, η_t , to account for time-varying shocks that impact respondents differently across survey rounds and cluster standard errors by district.²¹

Access to public education, however, does not necessarily entail enrolment. Rather, the link from educational access – an opportunity that equally affects all cohort members – to educational outcome is probabilistic. This is because not all primary students continue to secondary school, and because some older individuals returned to school after the war. To identify the effects of *actual* education among Zimbabweans who only received additional education because of the 1980 reform, we use access to secondary schooling to instrument for education. In our first stage, we estimate the effect of access to secondary education on a respondent's educational attainment:

$$\text{Education}_{icdt} = \delta \text{Secondary access}_c + \eta_t + \xi_{icdt} \quad (2)$$

before estimating the following structural equation using two-stage least squares (2SLS):

$$Y_{icdt} = \beta \text{Education}_{icdt} + \eta_t + \varsigma_{icdt} \quad (3)$$

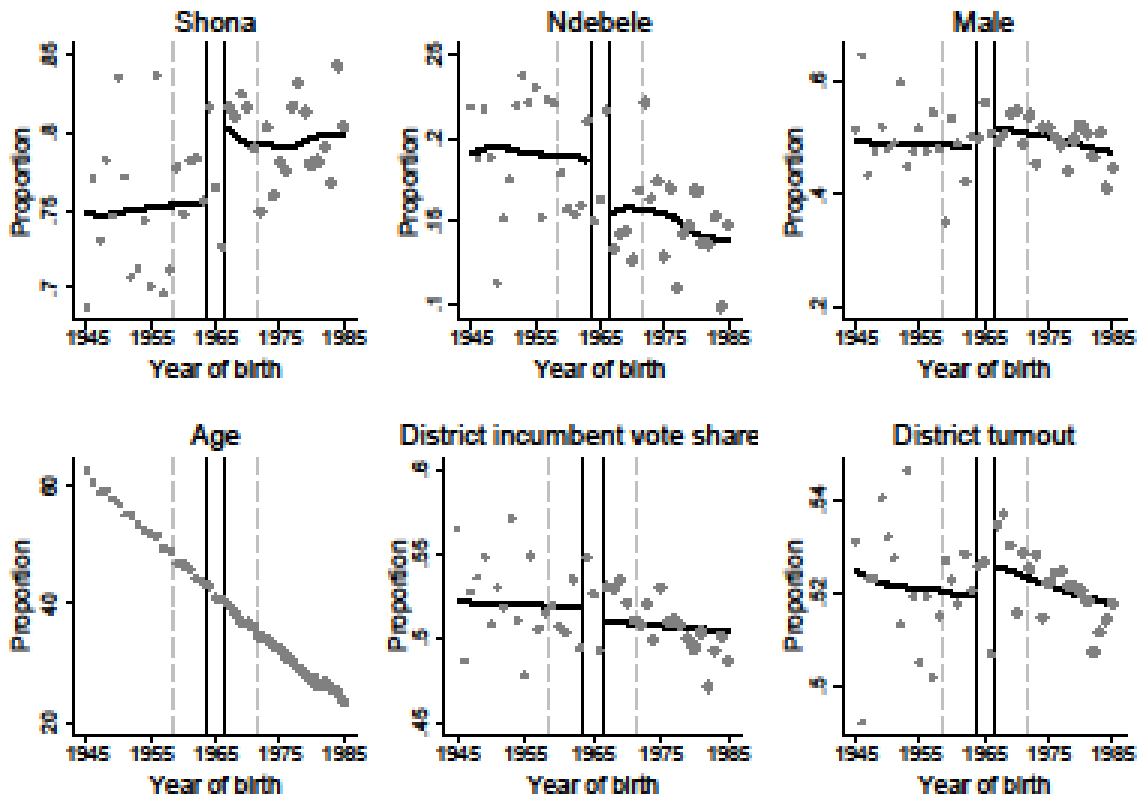
The IV estimates thus re-scale the reduced form to estimate the effect for black students who only remained in school because of the reform.

²⁰ Furthermore, the proportion of educated respondents in our surveys does not change after hyperinflation began in the mid-2000s.

²¹ Our results are robust to “double clustering” simultaneously by both district and cohort.



Figure 6: Trends in pre-treatment variables by cohort



Notes: See Figure 5.

Our linear coding of education follows Marshall (2014), who shows that coding an endogenous education variable as binary can significantly upwardly bias estimates if greater education at lower levels – which do not register in the first stage – also affects the outcome.²² Since any additional education may affect political behaviour, we use the seven-point education scale (described above) as our endogenous independent variable. This allows us to consistently estimate the average effect of an additional unit of education (Marshall, 2014).

IV estimation requires several additional assumptions. First, the relationship between the instrument (secondary access) and the endogenous variable (education) must be strong. The first-stage estimates in Table 1 show that the reform substantially increased education among affected cohorts, principally at the secondary level. Reinforcing the results in Figure 5, the estimate for our education scale in Column (1) indicates that being fully treated by the reform increases education by two-thirds of a level. This yields a large first-stage *F* statistic of 69, which far exceeds the standard critical value of 10 required to avoid weak instrument bias (Staiger & Stock, 1997).

²² Intuitively, this bias occurs because the reduced form captures any effect of increased schooling, while the first stage only normalizes the reduced-form estimates by the proportion of voters who were induced to complete high school.



Table 1: Estimates of education reform on educational attainment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Education	Incomplete primary	Complete primary	Incomplete secondary	Complete secondary	Incomplete college	Complete college
Secondary access	0.675*** (0.081)	0.065*** (0.012)	0.166*** (0.025)	0.263*** (0.026)	0.178*** (0.025)	0.007 (0.020)	-0.003 (0.008)
Observations	1,847	1,847	1,847	1,847	1,847	1,847	1,847

Notes: All specifications are estimated using OLS, include survey fixed effects, and cluster standard errors by district. All specifications include five cohorts either side of the cohorts fully affected or fully unaffected by the reform. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Second, the exclusion restriction requires that our instrument only affects political outcomes through increased education. We discuss this assumption in greater detail below.²³

Our second approach drops all partially treated respondents, and thus compares only untreated respondents who were born in 1963 or earlier (i.e. too early to be affected by the reform) to respondents who were fully treated. This allows us to focus on a sharp discontinuity in treatment assignment and implement a regression discontinuity (RD) design that relies on the weaker assumption that potential outcomes are smooth across the discontinuity.²⁴ For this second approach, we again estimate equations (1) and (3), excluding all partially treated respondents. Finding consistent results across both approaches should increase confidence in the study's findings.

Education and political participation in Zimbabwe

This section presents our main finding: that education reduces political participation in Zimbabwe, a paradigmatic electoral authoritarian regime. For each measure of participation, we provide both graphical evidence and regression estimates. Each regression table provides our reduced-form and IV estimates using both approaches to identification. We then demonstrate the robustness of our findings.

Main estimates

Contrary to the positive effects of education in advanced democracies (Sondheimer & Green, 2010) and democratic developing country contexts (Larreguy & Marshall, 2014), we find that in Zimbabwe education substantially and significantly reduces levels of political participation. Column (1) in Table 2 reports the estimates for our participation index across all estimation strategies. Relative to its sample mean of 0.64, Panel A shows that access to secondary education reduces participation by around 10%. Panel C shows that this estimate is barely affected by excluding partially treated respondents. The IV estimates in Panels B and D reveal a similar story: Using both the partially treated and RD approaches, a one-unit increase in education reduces participation by around 15% relative to its sample mean. Finally, we report the simple correlation between education and political participation in Panel E in the sample containing all cohorts, which also shows a negative correlation.²⁵

Importantly, as Table 2 and Figure 7 clearly indicate, the negative effects of education on our participation index are not simply due to an exceptionally large impact on one constituent variable. Rather, a one-unit increase in access to secondary schooling and a one-unit increase in the education scale both cause significant declines in a variety of ordinary forms of political participation. Specifically, our estimates suggest that being exposed to the full treatment reduces voting by 8 percentage points, contacting one's local councillor by 6 percentage points, attending a community meeting by 6 percentage points, and raising issues with others at a meeting by 2.5 percentage points. Similarly, a one-unit increase in education attainment reduces voting by 11 percentage points, contacting a local councillor by 9 percentage points, attending a community meeting by 9 percentage points, and raising an issue by 3 percentage points. Across all our specifications, only the decrease in raising an issue is not statistically significant.

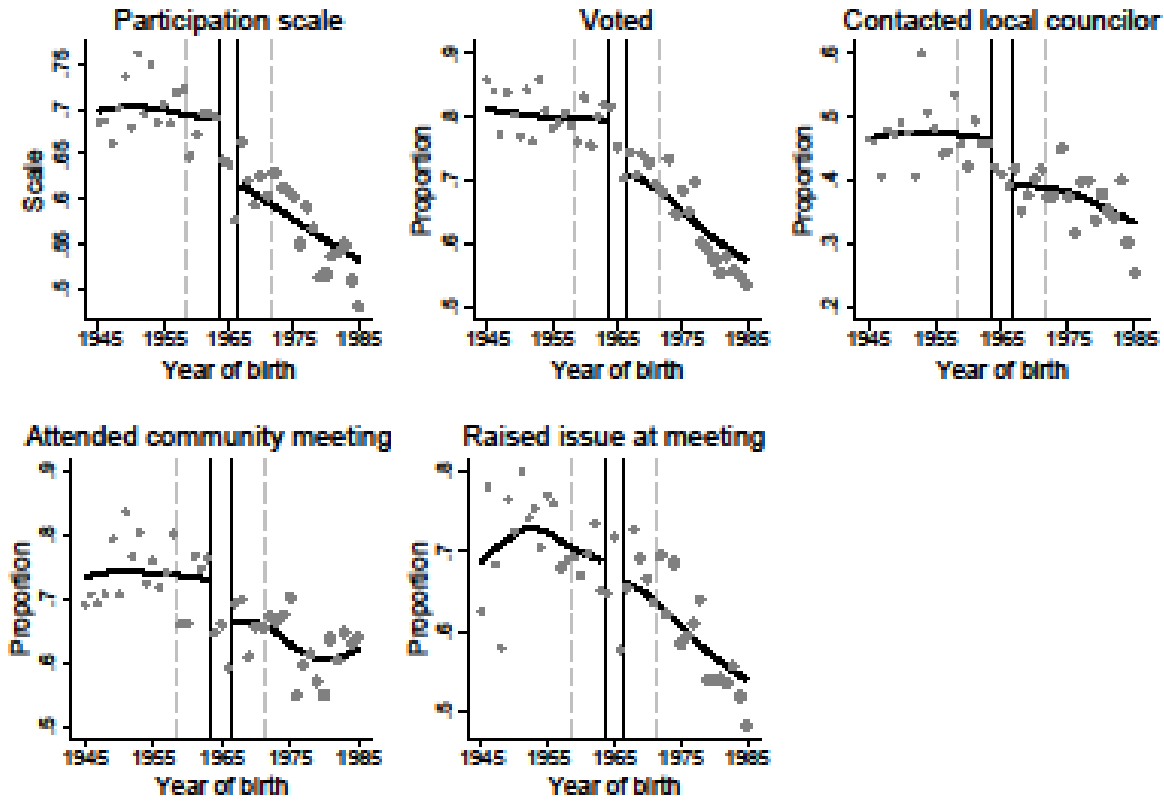
²³ There is no reason to suspect that monotonicity is violated.

²⁴ By removing partially treated cohorts, we slightly abuse the RD design, because the running variable is truncated. The RD requires that cohorts born in 1963 are comparable to cohorts born in 1967.

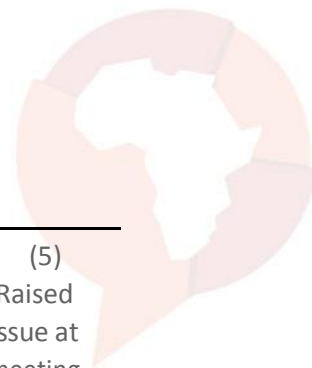
²⁵ Using indicators for each level of education reveals that each level of education further decreases participation until the effect plateaus at the college level.



Figure 7: Trends in political participation by cohort



Notes: See Figure 5.

**Table 2: The effects of education on political participation**

	(1) Participation scale	(2) Voted	(3) Contacted local councillor	(4) Attended community meeting	(5) Raised issue at meeting
Panel A: Reduced form					
Secondary access	-0.060*** (0.016)	-0.075*** (0.023)	-0.060** (0.026)	-0.058** (0.027)	-0.025 (0.030)
Observations	1,847	1,559	1,334	1,611	1,247
Panel B: Instrumental variables (IV)					
Education	-0.089*** (0.027)	-0.116*** (0.036)	-0.083** (0.037)	-0.084** (0.043)	-0.037 (0.043)
Observations	1,847	1,559	1,334	1,611	1,247
First-stage <i>F</i> statistic	69.0	60.1	64.6	56.4	49.9
Panel C: Regression discontinuity (reduced form)					
Secondary access	-0.062*** (0.016)	-0.072*** (0.023)	-0.064** (0.028)	-0.062** (0.028)	-0.024 (0.032)
Observations	1,470	1,250	1,062	1,281	985
Panel D: Regression discontinuity (IV)					
Education	-0.090*** (0.027)	-0.110*** (0.036)	-0.086** (0.039)	-0.088** (0.044)	-0.034 (0.046)
Observations	1,470	1,250	1,062	1,281	985
First-stage <i>F</i> statistic	71.0	58.9	72.5	61.1	54.7
Panel E: Correlation in the full sample					
Education	-0.036*** (0.007)	-0.042*** (0.007)	-0.036*** (0.008)	-0.032*** (0.010)	-0.033*** (0.010)
Observations	7,988	6,900	5,765	6,807	5,696

Notes: All specifications in Panels A, C, and E are estimated using OLS and include survey fixed effects. All specifications in Panels B and D are estimated using 2SLS, in which access to schooling is used to instrument for education, and include survey fixed effects. All specifications include five cohorts either side of the cohorts that were fully affected or fully unaffected by the reform; Panels C and D exclude partially treated cohorts born between 1964 and 1966. Standard errors are clustered by district in all specifications. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.



Robustness checks

Given that these findings challenge the conventional wisdom that education increases participation, it is essential to demonstrate their robustness. In Table 3 we present a series of checks testing our identifying assumptions.

We first show that our results are not an artifact of specification choices or cohort trends. Panels A and B show that the reduced-form estimates are similar when we include either three or 10 cohorts on either side of the reform eligibility threshold. To address the cohort trends concern, we employ placebo tests and control flexibly for cohort trends. In Panel C, we examine a placebo reform in which we estimate the reduced-form effects of a (hypothetical) reform in 1970 and compare cohorts five years on either side of this arbitrary cut-off. Contrary to the concern that trends are driving our results, we do not find a reduction in political participation around the placebo reform save in the case of raising an issue. We find no effects for placebo reforms in any year between 1960 and 1972.²⁶ Furthermore, Panel D shows that when we include 20 cohorts and cubic polynomial birth-year trends on either side of the reform cut-off, the reduced-form RD estimates are robust.²⁷

As noted above, plausible confounding explanations must relate to political differences between the cohorts immediately around the reform eligibility cut-off. First, one potential concern is a “first election” effect, such that respondents with different levels of treatment behave differently because they first voted in different elections (Meredith, 2009; Mullainathan & Washington, 2009). To show that this cannot explain our results, in Panel E we restrict attention to respondents born between 1963 and 1966 – who were first eligible to vote (at age 18) in the 1985 election – and find that the intensity of secondary access continues to significantly decrease political participation. Second, a subtler “coming of age” hypothesis is that older students may have been more cognizant of the independence movement, and their more intense support for Mugabe’s regime could be manifested in greater participation that has persisted until today. We thus test whether pro-nationalist sentiments are stronger among our older (untreated) cohorts. Contrary to this alternative explanation, as the Online Appendix shows, expression of national identity is instead positively (and insignificantly) associated with secondary access. Furthermore, persistent differences in support for Mugabe’s regime across cohorts cannot convincingly explain the differential change in the participation of younger (better-educated) cohorts following the more competitive 2008 election (see below).

Furthermore, although our design minimizes differences in citizen characteristics around the reform cut-off, we also show that our results are robust to the inclusion of other potentially confounding omitted variables. Panel E, which includes the pre-treatment variables described in Figure 6, yields similar results. In particular, these results suggest that participation is not being driven by compositional changes in the proportion of Shona and Ndebele respondents. Panel F demonstrates the robustness of our results to the inclusion of district fixed effects, although contacting a local councillor falls slightly outside statistical significance. Finally, although including age fixed effects decreases the precision of our estimates by removing considerable cross-cohort variation, we show in the Online Appendix that, if anything, the magnitudes of our negative estimates increase.

²⁶ The most recent placebo is 1972, which allows us to include five cohorts after the placebo reform, including partially treated cohorts.

²⁷ The inclusion of trends on either side of the discontinuity cannot be precisely estimated without extending the bandwidth. However, across all bandwidths, the estimates have similar magnitudes.

**Table 3: Robustness checks**

	(1) Participation scale	(2) Voted	(3) Contacted local councillor	(4) Attended community meeting	(5) Raised issue at meeting
Panel A: 3 Cohort bandwidth (reduced form)					
Secondary access	-0.063*** (0.021)	-0.065** (0.029)	-0.072* (0.043)	-0.081*** (0.030)	-0.016 (0.036)
Observations	1,220	1,022	887	1,068	828
Panel B: 10 Cohort bandwidth (reduced form)					
Secondary access	-0.076*** (0.013)	-0.098*** (0.015)	-0.080*** (0.020)	-0.074*** (0.023)	-0.055** (0.021)
Observations	3,427	2,909	2,471	2,981	2,343
Panel C: Placebo 1970 reform (RD reduced form)					
Secondary access	-0.005 (0.019)	-0.002 (0.026)	0.001 (0.041)	-0.014 (0.030)	-0.059* (0.032)
Observations	992	840	689	861	687
Panel D: Cubic cohort trends and 20 cohort bandwidth (RD reduced form)					
Secondary access	-0.064** (0.031)	-0.092* (0.051)	-0.053 (0.076)	-0.102** (0.042)	0.012 (0.062)
Observations	6,137	5,187	4,415	5,294	4,414
Panel E: Respondents first eligible to vote in the 1985 election (reduced form)					
Secondary access	-0.124*** (0.045)	-0.138** (0.055)	-0.072 (0.094)	-0.188*** (0.064)	-0.058 (0.068)
Observations	499	413	354	439	351
Panel F: Controlling for pre-treatment and district characteristics (reduced form)					
Secondary access	-0.062*** (0.016)	-0.076*** (0.023)	-0.058** (0.026)	-0.062** (0.026)	-0.035 (0.029)
Observations	1,847	1,559	1,334	1,611	1,247
Panel G: Controlling for district fixed effects (reduced form)					
Secondary access	-0.051*** (0.016)	-0.068*** (0.023)	-0.039 (0.026)	-0.052* (0.027)	-0.034 (0.031)
Observations	1,847	1,559	1,334	1,611	1,247

Notes: Panels A and B include three and 10 cohorts, respectively, either side of the reform. Panel C treats cohorts born between 1957 and 1961 as treated and compares them to cohorts born between 1952 and 1956. Panel D includes 20 cohorts either side of the first and last cohorts either side of the reform, excludes partially treated cohorts, and includes cubic (standardized) birth-year polynomials either side of the reform. Panel E includes only cohorts that turned 18 between 1981 and 1984. Panel F includes Shona, Ndebele, and male dummies as controls, as well as controls for the district incumbent vote share and district turnout at the nearest election. Panel G includes district fixed effects. Standard errors are clustered by district in all specifications. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

While our reduced-form (ITT) estimates do not require the exclusion restriction to hold, the IV estimates do. There are, however, good reasons to believe that the secondary education reform only affects participation through its effect on educational attainment. First, because education is highly proximate to the reform itself, most downstream behavioural responses – such as fertility, marriage, and vocation – are a function of a respondent's education. Second, the fact that the decrease in participation levels for partially treated respondents is lower than for fully treated respondents but higher than for untreated respondents increases our confidence that participation is responding to changes in actual schooling. If political responses to being affected by the reform itself were driving the results via some other channel, then it is hard to see why it would have differentially affected those receiving different instrument dosages. Third, a typical concern with such reforms is the possibility of cross-cohort spill-overs. However, if cohorts on either side of the reform cut-off interact with one another, spill-overs are likely to reduce the effects of schooling as behaviour becomes more homogeneous. Nevertheless, we examine the sensitivity of our results to arbitrary violations of the exclusion restriction by calculating the extent of the violation required to nullify our finding. Using Conley, Hansen, and Rossi (2012)'s most conservative (union of confidence intervals) sensitivity test, 37% of the reduced-form effect must operate through channels other than education for the 95% confidence interval of our 2SLS estimate of education's effect on the participation scale to include zero.

Deliberate disengagement: Testable implications

Why are more-educated Zimbabweans less likely to be politically active? This section tests the observable implications of our argument that in electoral authoritarian regimes, better-educated citizens – who recognize that their participation will have little effect on policy and distributive outcomes, yet will grant the regime a semblance of legitimacy – may deliberately disengage from political participation. Since this theory is difficult to test directly – we do not have access to the thought processes of our respondents when they are deciding whether (and how) to participate – we examine empirically the observable implications of the deliberate disengagement interpretation of our main finding that education reduces political participation in Zimbabwe's electoral authoritarian regime.

We propose four testable implications of our deliberate disengagement argument: first, whether the relationship between education and participation varies as the nature of elections in Zimbabwe has changed over time; second, whether access to secondary education indeed increases the economic welfare of the cohorts that benefited from it; third, whether education has a positive effect on attitudes that are closely associated with greater political participation in advanced democracies, such as political interest and support for democratic institutions; and fourth, whether education also creates a more critical citizenry, i.e. whether it has a *negative* effect on the level of support for the incumbent authoritarian regime. Finally, we rule out alternative mechanisms that could explain why education decreases participation in Zimbabwe, such as whether educated constituents demonstrate greater fear of political violence or whether they are less likely to be targets of electoral mobilization efforts.

For each test we present regression results in tabular form for both a summary index (when Cronbach's alpha exceeds 0.4) and for the index's constituent variables. Graphical results are provided in the Online Appendix.

Participation during competitive and non-competitive elections

We begin testing our deliberate disengagement argument by comparing survey rounds before and after 2008, which was the first election since 1980 to substantively affect the distribution of executive power. Zimbabwean elections during the study period (2000, 2002, 2005, and 2008)

varied significantly in character. Notably, the 2008 elections were the first in which the opposition obtained a share of power at the national level. The MDC (together with its splinter group, MDC-M) gained a majority in the House of Assembly, a majority of municipal councils, and some level of executive power through the internationally brokered national unity government. Thus, if educated voters are more likely to disengage when they feel that participation is futile or only serves to legitimate the government, we should also expect them to re-engage when elections are able to meaningfully influence political outcomes. To explicitly test this expectation, we compare the effects of education on political participation for respondents who were surveyed before and after 2009 (the first survey since the 2008 election).²⁸

The results, reported in Table 4, suggest that education had different effects before and after 2008. Consistent with our theoretical argument, the effect of education is negative and very large during the uncompetitive period before 2008. As demonstrated by the positive interaction term for post-2009 survey responses, the effect of education was essentially zero when elections affected the distribution of executive power. In no case is access to secondary education statistically significant for respondents surveyed since 2009, while the difference in coefficients is statistically significant for voting and contacting a local councillor. These results are also important because they cannot be easily reconciled with the alternative explanations discussed above.

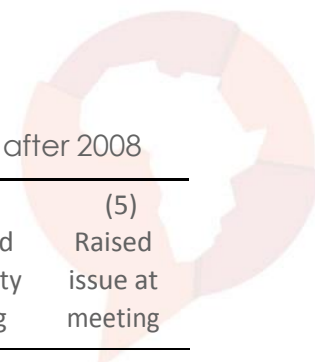
Education increases economic outcomes

We continue by showing that education has a positive effect on economic outcomes. Documenting this relationship serves as a marker that, despite concerns that the rapidly executed reform diluted the quality of schooling, the education received by post-1980 cohorts had significant welfare-enhancing implications. It also serves as a proxy for the social skills and cognitive abilities that are hypothesized to link education to political participation (Hillygus, 2005). According to Rosenstone and Hansen (1993), among others, well-educated voters are more likely to be politically active because schooling provides the "skills people need to understand the abstract subject of politics."

Closely related is the idea that increased cognitive ability leads to increased socioeconomic status (SES). Increased SES can lead to greater political participation either because some forms of participation are costly or because higher economic status leads to greater involvement in social networks, which are entry points into such participation (Verba, Schlozman, & Brady, 1995). We therefore examine the long-term economic returns of education for black Zimbabweans in terms of (a) employment (*Employed*), (b) self-reported living conditions (*Good living conditions*), and (c) a more objective poverty scale (*Poverty*).²⁹ We also combine these three measures to produce an economic outcomes scale (*Economic scale*). The Cronbach's alpha for this scale is 0.41.

²⁸ Comparing the characteristics of survey respondents before and after 2009, we find no significant differences in gender, tribe (Shona or Ndebele), district incumbent vote share, or education level. The lack of such differences also indicates that any out-migration during Zimbabwe's economic crisis did not systematically differ by type of survey respondent.

²⁹ The poverty scale combines indicators for whether an individual has gone without food, medicine, or cash in the past year. We note that some of the scales have a relatively low alpha – those results should be handled with greater care and with an eye on the effect on the constituent variables that make up the index.

**Table 4: The effects of education on political participation** | before and after 2008

	(1) Participation scale	(2) Voted	(3) Contacted local councillor	(4) Attended community meeting	(5) Raised issue at meeting
Panel A: Reduced form					
Secondary access	-0.097*** (0.020)	-0.157*** (0.038)	-0.102*** (0.027)	-0.078** (0.033)	-0.054 (0.043)
Secondary access ×Survey since 2009	0.074** (0.028)	0.140*** (0.044)	0.082* (0.047)	0.045 (0.039)	0.052 (0.062)
Observations	1,847	1,559	1,334	1,611	1,247
Panel B: Instrumental variables					
Education	-0.138*** (0.043)	-0.240*** (0.084)	-0.133*** (0.041)	-0.110* (0.059)	-0.078 (0.062)
Education ×Survey since 2009	0.102** (0.046)	0.213** (0.086)	0.104* (0.062)	0.063 (0.061)	0.076 (0.086)
Observations	1,847	1,559	1,334	1,611	1,247
First-stage <i>F</i> statistic	37.3	31.1	33.5	32.7	28.9
Panel C: Regression discontinuity (reduced form)					
Secondary access	-0.099*** (0.021)	-0.153*** (0.039)	-0.104*** (0.029)	-0.084** (0.034)	-0.054 (0.044)
Secondary access ×Survey since 2009	0.075** (0.029)	0.139*** (0.045)	0.078 (0.052)	0.052 (0.041)	0.056 (0.062)
Observations	1,470	1,250	1,062	1,281	985
Panel D: Regression discontinuity (instrumental variables)					
Education	-0.140*** (0.043)	-0.235*** (0.084)	-0.132*** (0.042)	-0.119** (0.060)	-0.080 (0.064)
Education ×Survey since 2009	0.104** (0.048)	0.214** (0.084)	0.095 (0.066)	0.073 (0.064)	0.082 (0.086)
Observations	1,470	1,250	1,062	1,281	985
First-stage <i>F</i> statistic	38.6	31.2	37.5	35.8	32.2

Notes: See Table 2.

As Table 5 shows, education improved Zimbabweans' economic well-being. Consistent with human capital models, treated adolescents are more likely to be employed two to three decades after the reform's onset. Furthermore, they appear to have higher income, as reflected in the increased objective measures of living standards (Column 4). Although not quite statistically significant, treated respondents are also likely to rate their living conditions more highly (Column 3). Together, this evidence suggests that education is valuable in terms of the skills taught and thus sharpens our theoretical puzzle: Despite greater economic resources, which should facilitate greater participation according to the current literature, we observe lower levels of participation.

Table 5: Estimates of secondary education reform on economic outcomes

	(1) Economic scale	(2) Employed	(3) Good living conditions	(4) Poverty
Panel A: Reduced form				
Secondary access	0.056*** (0.012)	0.099*** (0.021)	0.018 (0.026)	-0.035** (0.016)
Observations	1,847	1,847	1,483	1,847
Panel B: Instrumental variables				
Education	0.084*** (0.017)	0.147*** (0.029)	0.027 (0.039)	-0.052** (0.022)
Observations	1,847	1,847	1,483	1,847
First-stage <i>F</i> statistic	69.0	69.0	65.4	69.0
Panel C: Regression discontinuity (reduced form)				
Secondary access	0.066*** (0.012)	0.114*** (0.022)	0.024 (0.027)	-0.041** (0.017)
Observations	1,470	1,470	1,174	1,470
Panel D: Regression discontinuity (instrumental variables)				
Education	0.096*** (0.018)	0.167*** (0.031)	0.037 (0.040)	-0.059** (0.024)
Observations	1,470	1,470	1,174	1,470
First-stage <i>F</i> statistic	71.0	71.0	67.3	71.0

Notes: See Table 2



Political interest and support for democratic institutions

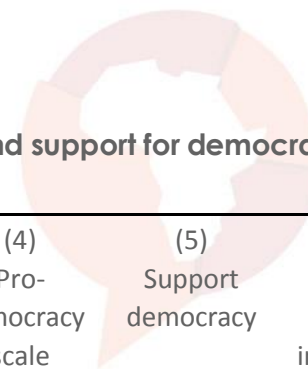
Education is also thought to increase interest in politics and support for democratic institutions. Dating back to Aristotle, through Thomas Jefferson and Tocqueville, it has been argued that education supports democratic institutions by breeding tolerance and acceptance of others' opinions. By contrast, Lerner (1958) highlights the impact of education on self-assessment and self-confidence. In Lerner's model, educated people in modernizing societies start developing opinions about public issues, which leads them to believe that they have the ability, and *thus should have the right*, to provide input on matters of importance that affect their welfare. This psychological change, argues Lerner (1958), translates into growing support for inclusive political institutions. Interest in politics is also important for citizen behaviour, as informed citizens can more accurately assess government performance and the likelihood that participation can affect political change. We therefore test whether education increases both interest in politics and support for democratic institutions. A null finding would be at odds with our "deliberate disengagement" argument.

Political interest is operationalized using three distinct measures. First, *News scale* combines indicators for whether respondents are exposed to news from the radio, television, or newspapers at least once a week. The scale has a Cronbach's alpha of 0.74. Second, *Politics not complicated* is an indicator variable for the 33% of respondents who agree or strongly agree that understanding politics is not complicated. Third, *Discuss politics* is an indicator for the 73% of respondents who report that they occasionally or frequently discuss politics with friends or family. In each case, higher values suggest greater political interest.

We measure support for democracy in two ways. First, we examine the relationship between education and *Support democracy*, an indicator for the 72% of respondents claiming to support or strongly support democracy. Importantly, this question is not asking respondents whether they approve of democracy as practiced in Zimbabwe. Second, to better capture specific support for the liberal institutions associated with democracy, we group the following nine indicators into a scale: do you agree that parties are needed, do you reject one-party government, do you reject one-man government, are you against government banning civil-society organisations, are you against government closing news stations, are you against presidential discretion, are you in favour of Parliament making the laws, do you agree that the president should obey the laws, and do you support term limits. All the variables that make up this *Support liberal institutions* index are positively correlated with a Cronbach's alpha of 0.83. Finally, we combine these two variables to produce a *Pro-democracy scale* (alpha of 0.41). As with political interest, larger values indicate greater support for democratic institutions.

Consistent with a large number of studies from Western democracies, Table 6 shows that education in Zimbabwe has a positive effect on political interest and support for democratic institutions. For political interest, a one-unit increase in education raises the likelihood that an individual regularly obtains political news by around 9 percentage points, or 25% relative to the sample mean (Column 1). Similarly, we find a positive, if weaker, effect of education on the belief that politics is not complicated (Column 2) and the frequency with which individuals discuss politics (Column 3). Our estimates thus demonstrate that reduced participation is not simply accounted for by a reduced interest in politics. In fact, educated Zimbabweans are *more* interested in politics, at the same time that they are *less* likely to participate.

Similarly belying an explanation rooted in a limited demand for democracy, we find that education increases support for democracy in the abstract (Column 4). Similarly, we find that an additional unit of education significantly increases the likelihood that an individual professes support for democracy by 7 percentage points (Column 5). The positive effect on support for liberal institutions suggests that voters possess a genuine understanding of the institutional building

**Table 6: Estimates of secondary education reform on political interest and support for democracy**

	(1) News scale	(2) Understanding politics not complicated	(3) Discuss politics	(4) Pro- democracy scale	(5) Support democracy	(6) Support liberal institutions
Panel A: Reduced form						
Secondary access	0.061*** (0.013)	0.037 (0.025)	0.027 (0.023)	0.036** (0.015)	0.049* (0.025)	0.023 (0.014)
Observations	1,847	1,095	1,611	1,847	1,847	1,847
Panel B: Instrumental variables						
Education	0.090*** (0.017)	0.060 (0.040)	0.039 (0.033)	0.054** (0.022)	0.073* (0.037)	0.034* (0.021)
Observations	1,847	1,095	1,611	1,847	1,847	1,847
First-stage <i>F</i> statistic	69.0	31.6	56.4	69.0	69.0	69.0
Panel C: Regression discontinuity (reduced form)						
Secondary access	0.064*** (0.013)	0.038 (0.025)	0.024 (0.022)	0.030** (0.015)	0.047* (0.024)	0.014 (0.014)
Observations	1,470	885	1,281	1,470	1,470	1,470
Panel D: Regression discontinuity (instrumental variables)						
Education	0.094*** (0.017)	0.061 (0.040)	0.034 (0.031)	0.045** (0.021)	0.068* (0.036)	0.021 (0.020)
Observations	1,470	885	1,281	1,470	1,470	1,470
First-stage <i>F</i> statistic	71.0	29.7	61.1	71.0	71.0	71.0

Notes: See Table 2



blocks required to support liberal democracy, although these estimates are typically not quite statistically significant (Column 6). In sum, our results suggest that, consistent with our deliberate disengagement argument, support for democracy increases with education.

Education increases criticism of the incumbent regime

If education reduces participation in non-contentious political action due to deliberate disengagement, it follows that education should be associated with *reduced* support for the incumbent autocratic regime. Qualitative assessments are consistent with this theoretical expectation. For example, Chung (2006:310) states that “the democratization of education also led to growing criticism of the government, as education enabled the newly educated young to voice their opinions eloquently and openly.”

To explicitly test this proposition, we assess support for the government using four different measures. Our first and second measures, *Close to ZANU-PF* and *Close to MDC*, indicate whether respondents claim to feel close or very close to the ruling party and the main opposition party; 24% and 23% of respondents reported being close or very close to ZANU-PF and the MDC, respectively. Third, we create a variable named *Incumbent trust and performance*, which is a summative rating scale combining three indicators for trusting the president, the ruling party, and its members of Parliament (MPs), and three indicators for whether the respondent believes that the president, MPs, and the local government are performing well in office (alpha of 0.85). Fourth, *Perceived government corruption* is a summative rating scale (alpha of 0.75) that combines four indicator variables asking whether the respondent believes the president, MPs, local councillors, and government officials are corrupt. Finally, we combine these four variables to produce the *View of government scale* (alpha of 0.58).

The results, shown in Table 7, support our theoretical argument: Across all specifications in Column (1), access to secondary education has a negative effect on the support for the government scale. Furthermore, Columns (2) and (3) show a significant decrease in support for ZANU-PF as well as a significant increase in support for the MDC. Trust in government also broadly decreases with education (Column 4). Finally, and consistent with the idea that political interest might decrease support for the regime, perceptions of corruption significantly increase with education. These findings suggest that more-educated citizens are more critical of Mugabe's regime and cognizant of its problems.

However, it remains possible that the differences between the pre- and post-2008 period could instead reflect changes in the characteristics required for deliberate disengagement, such as interest in politics, support for democracy, and disapproval of the regime. However, as the Online Appendix confirms, none of the potential mechanisms of deliberate disengagement – which are relatively long-term processes that should not substantially fluctuate across elections – changed across the pre- and post-2009 periods.

Alternative explanations

Finally, we eliminate alternative explanations of our deliberate disengagement argument. First we test whether uneducated voters are disproportionately the targets of turnout mobilization drives, because vote-buying efforts either target the poor or the regime's core supporters (Stokes et al., 2013). Less-educated voters seem *a priori* to be more likely to be included in such patronage networks. We therefore create the variable *Received gift* that indicates whether respondents report receiving a gift from a political party before the most

recent elections.³⁰ We also create an indicator, *Freedom to choose vote*, which proxies for pre-commitment to a party in exchange for some benefit.

Table 7: Estimates of secondary education reform on support for the government

	(1) View of government scale	(2) Close to ZANU-PF	(3) Close to MDC	(4) Government trust and performance	(5) Perceived government corruption
Panel A: Reduced form					
Secondary access	-0.053*** (0.016)	-0.057** (0.024)	0.088*** (0.023)	-0.029 (0.019)	0.037** (0.015)
Observations	1,847	1,847	1,847	1,847	1,847
Panel B: Instrumental variables					
Education	-0.078*** (0.022)	-0.084** (0.033)	0.130*** (0.034)	-0.044* (0.026)	0.055** (0.022)
Observations	1,847	1,847	1,847	1,847	1,847
First-stage <i>F</i> statistic	69.0	69.0	69.0	69.0	69.0
Panel C: Regression discontinuity (reduced form)					
Secondary access	-0.050*** (0.016)	-0.054** (0.025)	0.088*** (0.025)	-0.025 (0.019)	0.034** (0.017)
Observations	1,470	1,470	1,470	1,470	1,470
Panel D: Regression discontinuity (instrumental variables)					
Education	-0.074*** (0.022)	-0.079** (0.034)	0.129*** (0.035)	-0.036 (0.026)	0.050** (0.024)
Observations	1,470	1,470	1,470	1,470	1,470
First-stage <i>F</i> statistic	71.0	71.0	71.0	71.0	71.0

Notes: See Table 2

³⁰ This question has been used to proxy vote-buying (Kramon, 2014).

A second potential alternative explanation is that lower levels of political participation simply reflect the greater repression of educated citizens, for example, in order to suppress the opposition vote share. Similarly, educated citizens may pre-emptively disengage to avoid facing violence by signalling that they are not troublemakers. This channel seems plausible, given that in both the 2002 and 2008 elections the regime targeted significant violent repression at suspected MDC supporters.

We thus create an indicator variable, *Fear repression*, which captures whether respondents fear that they will be repressed. Similarly, we create *Vote monitored*, which captures respondents' belief that the authorities can know how they vote.

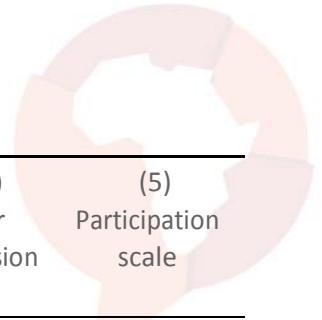
We do not find support for these alternative explanations. Columns (1) and (2) of Table 8 show that greater education does not decrease the likelihood that voters receive a gift during elections or perceive their vote to be unfree. If anything, more-educated voters are slightly more likely to receive a gift, although the difference is insignificant. Furthermore, if mobilization were driving our results, we might expect the negative effect of education to be largest in locations dominated by ZANU-PF or where turnout is high. However, Columns (3) and (4) report no significant negative coefficient on the interaction of access to secondary school for either the district-level ZANU-PF vote share or the turnout rate at the most recent election. We conclude that being more educated does not significantly reduce the likelihood that a voter is mobilized.

Turning to the repression hypothesis, Columns (1) and (3) in Table 9 show that education does not affect a respondent's fear of repression or belief that voting is monitored. Although the estimates are positive, neither is close to being statistically significant. Given that Mugabe has historically regarded the Ndebele as the opposition, if education induces greater fear then we should expect this to be greatest among the Ndebele. Again, the data does not support this possibility (Columns 2 and 4). Finally, we show that in districts with a large number of instances of violence against civilians by ZANU-PF – *Violent events*, as measured by the Armed Conflict Location and Event Data Project – educated voters are no less likely to participate in politics (Column 5).

Table 8: Mobilization explanations

	(1) Received Gift	(2) Freedom to choose vote	(3) Participation scale	(4) Participation scale
Secondary access	0.063** (0.030)	-0.026 (0.037)	-0.086** (0.034)	-0.029 (0.089)
Secondaryaccess×Incumbentshare			0.047 (0.073)	
Secondaryaccess×Turnout				-0.062 (0.176)
Observations	731	918	1,847	1847

Notes: All specifications are estimated using OLS, include survey fixed effects, and cluster standard errors by district. Specifications include five cohorts either side of the cohorts that were fully affected or fully unaffected by the reform. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

**Table 9: Repression explanations**

	(1) Vote monitored	(2) Vote monitored	(3) Fear repression	(4) Fear repression	(5) Participation scale
Secondary access	0.013 (0.029)	0.004 (0.033)	0.028 (0.027)	0.016 (0.029)	-0.054*** (0.019)
Secondary access × Ndebele		0.060 (0.075)		0.057 (0.090)	
Secondary access × Violent events					-0.00001 (0.00002)
Observations	918	918	918	918	1,847

Notes: See Table 8

Conclusion

Reflecting on the large positive correlation between education and political participation, Phillip Converse famously wrote that “education is everywhere the universal solvent, and the relationship is always in the same direction” (Converse, 1972:324). In this article we seek to qualify “Converse’s law” by testing whether the positive relationship between education and participation holds in electoral authoritarian settings. Specifically, we develop and test a theory of “deliberate disengagement,” according to which the more-educated citizenry may decide to disengage from politics when initial political liberalization efforts prove to be futile. Non-participation, we further argue, may serve as a non-violent form of protest designed to deprive the autocratic regime of enjoying a semblance of legitimacy. We then demonstrate this argument using the case of Zimbabwe, which in the past three decades has been controlled by a paradigmatic electoral authoritarian regime.

In short, our results strongly suggest that “Converse’s law” should indeed be qualified. Exploiting Zimbabwe’s major education reforms in 1980 as a natural experiment, we find that, in Zimbabwe, education reduces various forms of non-contentious political action. This finding is robust to various estimation approaches, to the inclusion and exclusion of “partially treated” respondents, to the inclusion of a battery of pre-treatment control variables, to various placebo tests, and to varying the length of the bandwidth around the cohort-eligibility cut-off point. This is, to the best of our knowledge, the first article to argue – and causally demonstrate – that the positive relationship between education and political participation is *conditional on regime type*. As such, it makes an important contribution to our understanding of the determinants of political participation in the developing world.

We also provide considerable evidence to support our claim that more-educated voters exhibit lower levels of political participation due to deliberate disengagement rather than another possible channel. Consistent with our theoretical argument, we find that education causally leads to greater support for democracy, weaker support for the incumbent autocrat, greater interest in politics, and higher living standards. We further find that these results are unlikely to be driven by

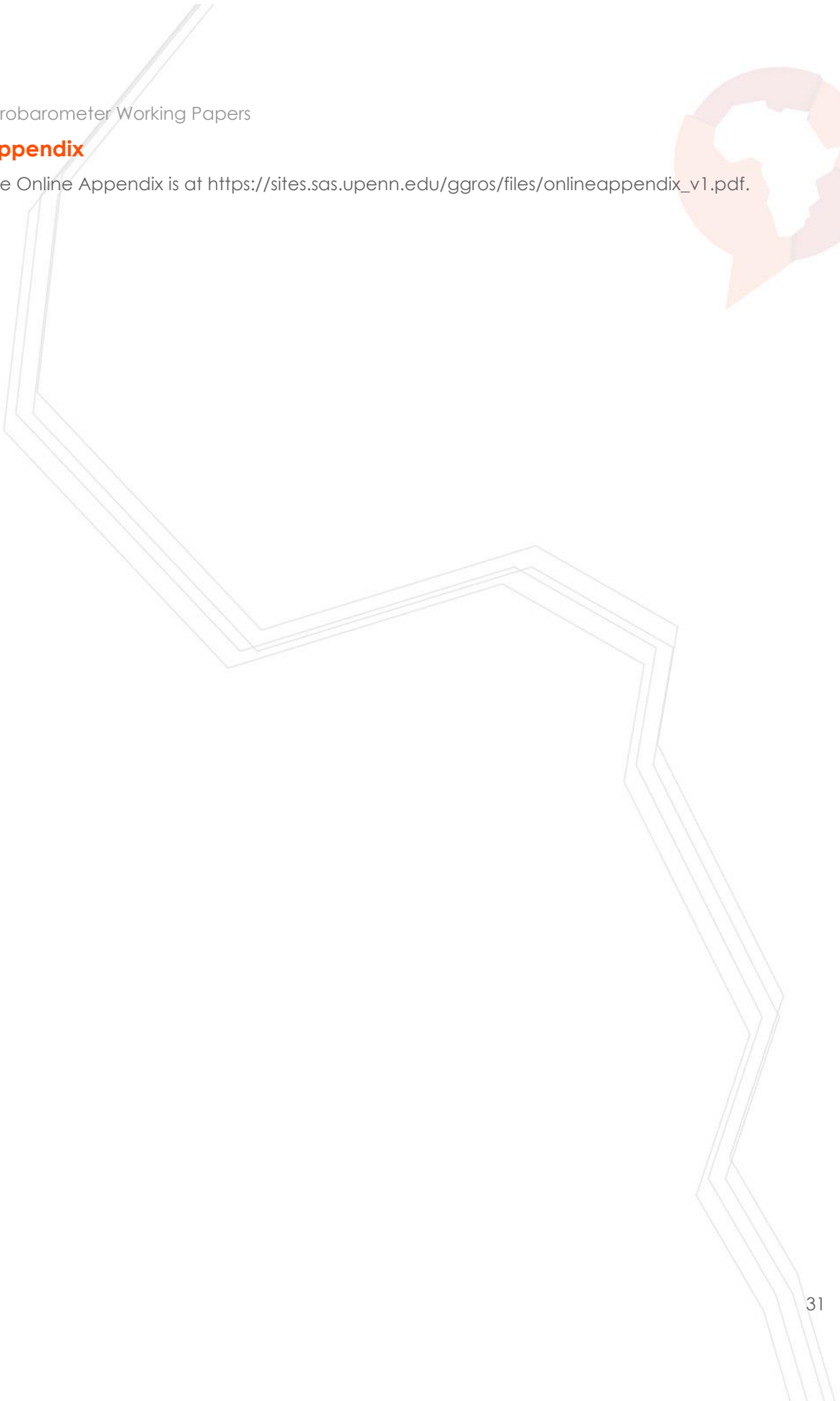


alternative explanations such as political repression, though we acknowledge that this may be an effective strategy for depressing support among opposition supporters.

Naturally, the findings reported in this study raise concerns regarding external validity. First, to provide a suggestive step in this direction, we pool the Afrobarometer surveys for all available countries and test whether the relationship between education and voting depends on regime type. Encouragingly, we find a significant negative correlation for closed anocracies (Burkina Faso, Tanzania, Uganda, and Zimbabwe, where Polity scores are between -4 and 0), and weak insignificant correlations for open anocracies (where Polity scores are between 1 and 5). While these correlations suggest that our findings might apply beyond Zimbabwe, more work is needed to further qualify the conditions under which educated citizens choose to withdraw from the political sphere. Second, this study investigates the negative effects of education on *non-violent* forms of participation. A fruitful avenue of future research would be to explore the conditions under which education leads individuals to instead support political violence, as Friedman et al. (2011) find in western Kenya, or to personally adopt violent means of opposing an autocrat, as seems to be the case in Burundi (Samii & West, 2014).

Appendix

The Online Appendix is at https://sites.sas.upenn.edu/ggros/files/onlineappendix_v1.pdf.



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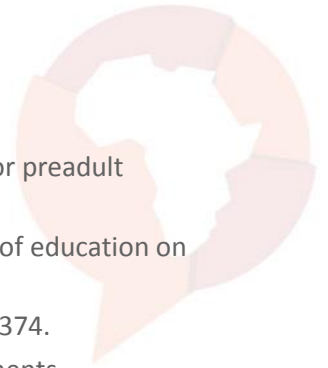
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