

DELIBERATE DISENGAGEMENT: HOW EDUCATION DECREASES POLITICAL PARTICIPATION IN ELECTORAL AUTHORITARIAN REGIMES *

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JANUARY 2015

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 local councilors, 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 the disengagement hypothesis, educated citizens experience better economic outcomes, are more interested in politics, and are more supportive of democracy, but are also more likely to criticize the government and support opposition parties.

*This article benefited from helpful conversations with, and suggestions from, Karen Grépin, Patrick O’Halloran, Marc Meredith, and participants at the Boston Working Group in African Political Economy.

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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 and 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 and Niemi, 1968), or family background (Nie, Junn and Stehlik-Barry, 1996)—on political participation is a formidable challenge. Debates between “education as cause” versus “education as proxy” remain far from settled (Berinsky and Lenz, 2011; Kam and 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 and 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,

¹See Acemoglu et al. (2005), Barro (1999), and Murin and Wacziarg (2014). See Glaeser, Ponzetto and Shleifer (2007) for the role of education in consolidating democracy. A small literature has instead examined education’s effect on violent forms of protest (Campante and Chor, 2012a; Samii and West, 2014).

scholars, especially those associated with modernization theory, have long assumed that education is a critical driver of political participation. For example, Deutsch (1961) and Lerner (1958) stipulated that more-educated citizens are key to generating pressure for greater citizen inclusion, and Lipset (1959) has argued that more-educated citizens engage in the political actions necessary to sustain representative governments. Indeed, Huntington (1991) has 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 the study of the democratization-education nexus, the nature of the relationship between education and non-violent political participation in present-day 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 and 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 democratic institutions, and the resources needed to meaningfully engage in political action.

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 the democratic ideals of political equality and individual agency (Lijphart, 1997). Yet in

many non-democratic settings, quintessential political actions such as voting cannot necessarily be viewed as the exercise of free will. Indeed, various forms of participation in electoral authoritarian settings are designed to mobilize citizens to demonstrate allegiance to the regime rather than to aggregate societal preferences or enable citizen voice (Hermet, 1978). Elections in many electoral authoritarian regimes are not designed to allow citizens to freely elect their leaders, but rather to provide incumbents with a facade of legitimacy, appease the international community, and demonstrate the omnipresence of the regime (Levitsky and 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 and 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.² 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 and 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 and Chor, 2012b).

We test our disengagement argument using the case of Zimbabwe, a paradigmatic electoral authoritarian regime ruled by President Mugabe and a civilian-military junta (Levitsky and Way, 2010). While elections have been held regularly since 1980, the incumbent regime has used a

²E.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.

combination of intimidation, violence, manipulation of legal rules, and vote rigging to ensure that the opposition could not take power.³ 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 became more competitive, ultimately producing a power-sharing executive between Mugabe and opposition leader Morgan Tsvangirai, and thus presents a valuable opportunity to compare the effects of education during 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, which removed or significantly reduced progression requirements, as well as geographic and financial constraints, substantially increased access to secondary education for black students and induced some students to remedially attend primary school. We exploit this natural experiment to analyze 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 allow for non-compliance across cohorts, we also estimate instrumental variable (IV) regressions.

Our primary result is that, in Zimbabwe, education *reduces* levels of political participation. 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 councilor 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 still 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

³Members of the ruling party, the Zimbabwe African National Union–Patriotic Front (ZANU-PF), “believe that ZANU-PF has a right to rule in perpetuity and thus will never recognize an opposition victory at the polls” (Bratton and Masunungure, 2008, 43).

increased education decreases support for the ruling party, reduces perceptions of government performance, and increases support for the main opposition party. Finally, consistent 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. Together, 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 behavior. 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 also suggest that whether citizens decide to use these facilities or “deliberately withdraw” from the political arena is likely to depend on systemic attributes—specifically, whether the regime is sufficiently democratic and receptive to bottom-up input.

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 participa-

⁴See [Berinsky and Lenz \(2011, 328-360\)](#) for a comprehensive review of recent challenges to the traditional view that education has an uniform positive effect in the American context.

tion. For example, [Cox \(2003\)](#) and [Norris \(2002\)](#) find a positive association between institutional trust and voter turnout, and [Kasara and Suryanarayan \(2014\)](#) show that voter turnout for higher-resource constituents depends on both the state’s taxation capacity and the congruence of voters’ political preferences with those of the poor. Our results also challenge a body of work that argues that education serves as a tool of indoctrination and social control that instills obedience to authority ([Lott, 1999](#)). Our findings suggest instead that education supports critical thinking, which can lead citizens in political settings such as Zimbabwe’s to oppose authority that is perceived as illegitimate.

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 and Shleifer, 2007](#); [Woodberry, 2012](#)). By discussing why those deemed 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 towards 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 credibly identified a positive effect of education on political participation in the developed world.⁵ The study of the effect of education in developing

⁵Causal evidence for a positive relationship in developed countries includes [Dee \(2004\)](#), [Henderson and Chatfield \(2011\)](#), [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 intervention increase political participation.

countries is less developed, and still suffers, for the most part, from insufficient attention to causality.⁶ Furthermore, none of the recent studies that attempt to identify a causal relationship between education and participation in a developing country have examined this question in the context of a repressive electoral authoritarian regime.

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-biased 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 4-5 years after initial enrollment 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. Wantchekon, Novta and Klačnja (2013) do not, however, report results regarding voting behavior and political attitudes, perhaps since 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 local officials, attending community meetings, and devoting attention to and learning about political events.

While the above papers arguably identify the impact of education on political participation, they

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

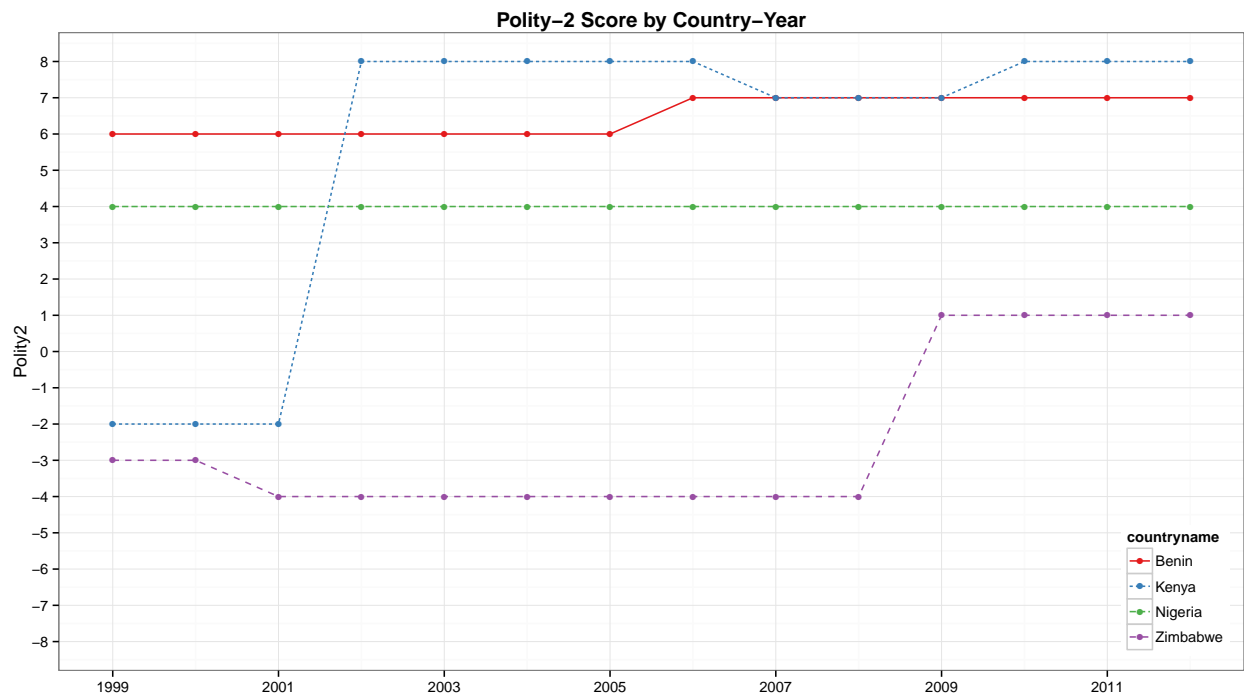


Figure 1: Polity-2 Scores by Year

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 since adopting far-reaching liberalization reforms.⁷ 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 the 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.

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, as the wave of independence swept through the African continent, 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 head of state, still serves as president.

In contrast to dire predictions from the outgoing white-dominated regime, economic growth in the first years of majority rule was relatively strong, enabling Mugabe's new Zimbabwe African National Union (ZANU) government to dramatically expand access to health, education, and public services to the previously disenfranchised black population. However, political conflict between

⁷Benin and Kenya have had electoral transitions between the former ruling party and opposition parties. In Nigeria, presidential power has alternated between northerners and southerners within the ruling party since the return to democracy in 1999.

the two main independence movements quickly emerged; ZANU began a campaign of bloody repression against its rival, the Zimbabwe African People's Union (ZAPU), in 1983 (Bourne, 2011; Bratton, 2014). A truce brokered in 1987 merged the two parties into "ZANU-PF," and the constitution was changed from a parliamentary to a presidential system with a strong executive office, held by Mugabe. These changes facilitated ZANU-PF political domination throughout the 1990s: 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 and Way, 2010).

Opposition to Mugabe's increasingly autocratic rule began to crystallize only in the late 1990s, when labor, religious, and civil society groups mobilized around the goal of introducing a new constitution with term limits. These groups eventually formed Zimbabwe's main opposition party, the Movement for Democratic Change (MDC). Mugabe responded by proposing a new constitution, which was defeated in a referendum in 2000. Later that year, the MDC won an unprecedented 58 out of 120 seats in parliament.⁸

Rather than heralding a democratizing turn, these surprising results drove Mugabe to secure his position 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 Morgan Tsvangirai—the MDC presidential candidate—with 56% of the vote amid violence and widespread vote suppression, especially in MDC areas (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 over 700,000 people from informal urban settlements (which were often MDC strongholds). Amid agricultural collapse and macroeconomic instability, hyperinflation

⁸At the time, Zimbabwe's parliament had 150 seats, 30 of which were directly appointed by Mugabe. Thus ZANU-PF still had a large working majority despite the close election result.

emerged and reached 1,281% in 2006 and 231,000,000% in 2008 (Bratton, 2014, 84).

The opposition was more unified in the lead-up to the 2008 elections. Benefiting from (initially) lower levels of election-related violence than in 2002 (Levitsky and Way, 2010), the MDC won an outright parliamentary majority in the 2008 election, as well as the majority of municipal councils. 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 (the official total was 48% for Tsvangirai vs. 43% for Mugabe). 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. Nevertheless, although the MDC chose the National Assembly speaker and controlled several ministries, ZANU-PF retained control of key coercive institutions such as the police and military. Moreover, the MDC was politically weakened by growing internal divisions and the loss of moral authority implied by sharing power with ZANU-PF. This facilitated the return of ZANU-PF dominance (LeBas, 2014). The 2013 elections, reported to have been less violent than previous elections, ended the power-sharing agreement as Mugabe comfortably beat Tsvangirai and won 70% of parliamentary seats.

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 available data suggests that the government spent about 12 times more per capita on primary schooling for whites (Dorsey, 1989). Planning documents from the period further reveal that the desired projection was for only 3% of black Zimbabweans to complete secondary education (Nhundu, 1992).¹⁰ 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 color. While some fees were applied for secondary school, automatic progression from primary to secondary school was decreed.¹¹ 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 war of independence. The number of primary schools almost doubled (from 2,401 to 4,291) between 1980 and 1986, while the number of secondary schools increased dramatically, from 177 to 1,276 (Bourne, 2011). This infrastructure investment was financed by an increase in the budget share devoted to education and through communities' voluntary in-kind contributions.

Zimbabwe's education reform had an immediate effect: overall student enrollment doubled in one year (Narman, 2003). As Figure 2 illustrates, this increase was most dramatic for secondary enrollment, 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.

Based on the nature of Zimbabwe's education reform, we focus on the expansion of secondary

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

¹⁰See Atkinson (1972) for an illuminating account of Rhodesia's education system and the policies dictating the level of schooling Africans received.

¹¹Riddell (1980, 46) estimates that fees at more distant boarding schools were twice as large as tuition fees for secondary schooling around 1979.

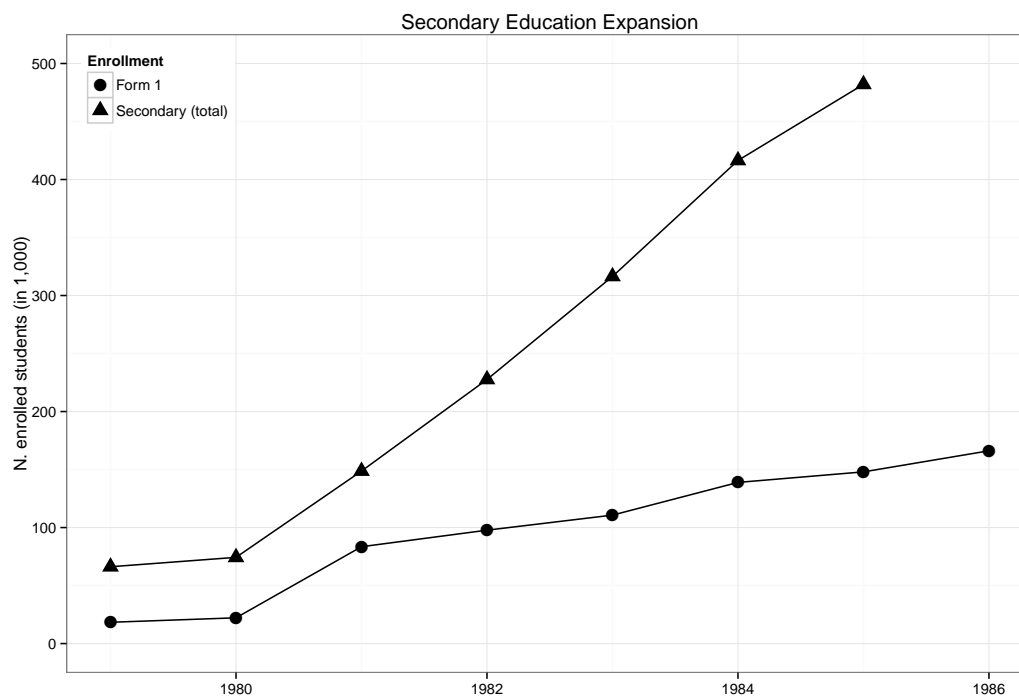


Figure 2: Number of Enrolled Students in Form 1 (First Year of Secondary Education) and in all Secondary Education Grades by Year, 1979-1986

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

education.¹² 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 enrollment, we would observe a sharp discontinuity in education for the cohorts of primary school starting age in 1980, i.e., those born from 1972-74. However, we find no evidence of a discontinuity around those birth years (see Figure 5 below).¹³ The reform's small effect on primary school completion principally reflects the remedial education of individuals whose education was interrupted by the war (Narman, 2003).¹⁴

Analysts of Zimbabwe's education reforms note that, given the rapid expansion, qualified teachers could not be hired quickly enough, instructional quality often suffered, and school construction lagged behind enrollment, leading to overcrowded classrooms.¹⁵ 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).

¹²Agüero and Bharadwaj (2014) and Grépin and Bharadwaj (2014) similarly restrict their focus to secondary school access.

¹³Similarly, 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).

¹⁴This was facilitated by a program allowing teenagers to return to complete primary school on an abbreviated three-year schedule (Chung, 2006, 228).

¹⁵A 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's (2006, 272) first-hand account suggests that Ministry of Education technocrats resisted pressure from politicians to build schools based on political considerations.

In sum, Zimbabwe has undergone two dramatic social and political processes in the past 30 years. First, its expansive education reform reached its peak in the early 1990s. Second, its political transformation has been characterized by an increasingly repressive political order. These transformations provide a unique setting in which to examine the role of education in electoral authoritarian regimes that allow some restricted political contestation, but that are also characterized by social and economic vulnerability, opposition intimidation, and political violence.

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 2012.¹⁶ Since the Afrobarometer questions change across survey rounds, different rounds may be used to test different outcome measures (see Online Appendix). We focus exclusively on black respondents, who comprise the overwhelming majority of Zimbabwe’s population and were the education reform’s target group.¹⁷

¹⁶The Afrobarometer initiative—a collaborative enterprise of Michigan State University, South Africa’s Institute for Democracy, and the Ghanaian Centre for Democratic Development—conducts nationally representative surveys on the political attitudes of citizens in selected African countries.

¹⁷There 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.

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 2-4 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 high school.

Political participation, our principle 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 councilor (*Contacted local councilor*), 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.¹⁸ We then combine these four variables, which are positively correlated with a Cronbach's alpha of 0.58, into a summary index (*Participation scale*).¹⁹ Although we also present the results for each component separately, we place greatest weight on our scale measure because it averages over the noise contained in the binary indicators.

¹⁸We focus on non-contentious and undemanding forms of participation that relatively low-level education might reasonably affect. Contacting one's member of parliament (MP) and attending a demonstration represent additional forms of participation. Consistent with their comparatively higher costs, both are rare in our sample: only 19% contacted their MP or attended a demonstration. Consistent with the unlikelihood that median levels of education would induce changes in such behavior, 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.

¹⁹All summary indices are constructed using the `alpha` command in Stata, which does not use casewise 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.

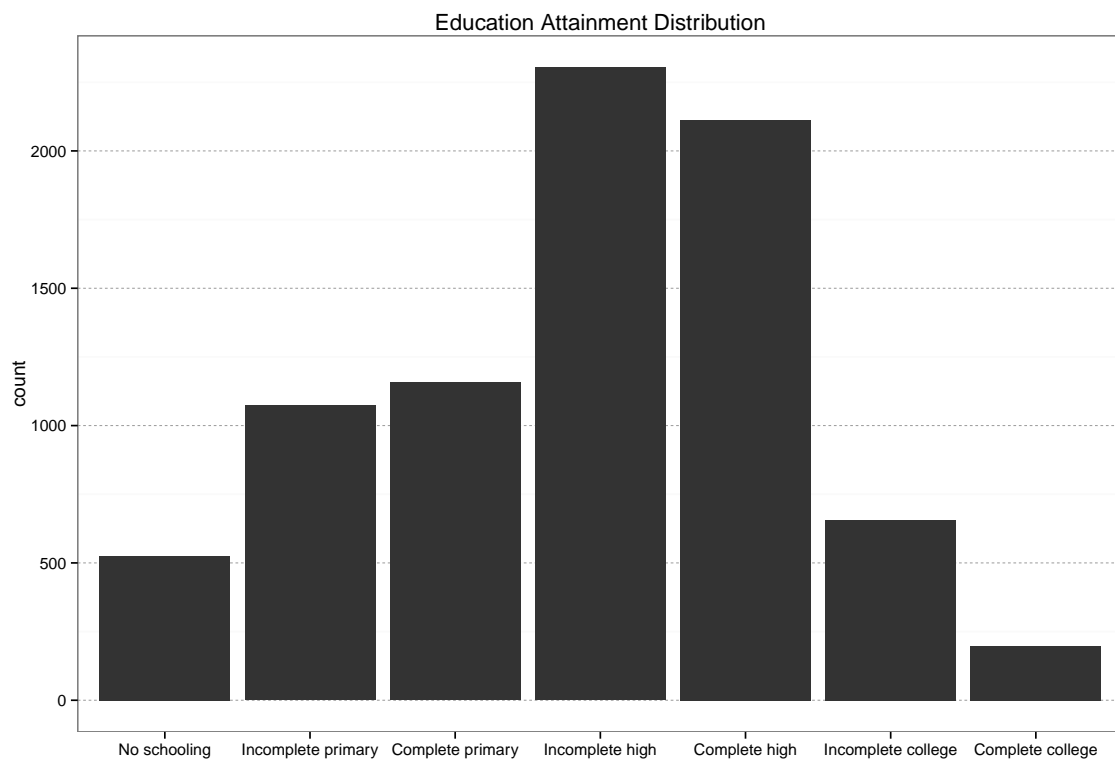


Figure 3: Sample Distribution of Educational Attainment (8,020 observations)

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 affected by the reform to black citizens from cohorts that were too old to benefit from the educational expansion.

Accordingly, we define those born in 1967 or later, who were 13 or younger when the reform was implemented, as fully "treated" (treatment value = 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 (treatment value = 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 half treatment, while individuals aged 14 and 16 are coded as receiving one-quarter and three-quarter dosages, respectively.²⁰ This coding scheme, shown graphically in Figure 4, defines *Secondary access*, our source of exogenous variation. This variable identifies the reduced-form effects of the reform under the assumption that black Zimbabweans on both sides of the reform cutoff are effectively identical, with the exception that only some cohorts were eligible to enjoy access to secondary education.

Figure 5 shows trends in education across cohorts, and provides cohort means in educational attainment. The mean education scale (in the top left) demonstrates that cohorts fully treated by the reform (i.e., born in 1967 or later) exhibit substantially higher levels of education compared to cohorts born in 1963 or earlier. The increase is large and almost equivalent to moving from complete primary to 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.

²⁰This approach to partial treatment closely follows [Bleakley \(2010\)](#).

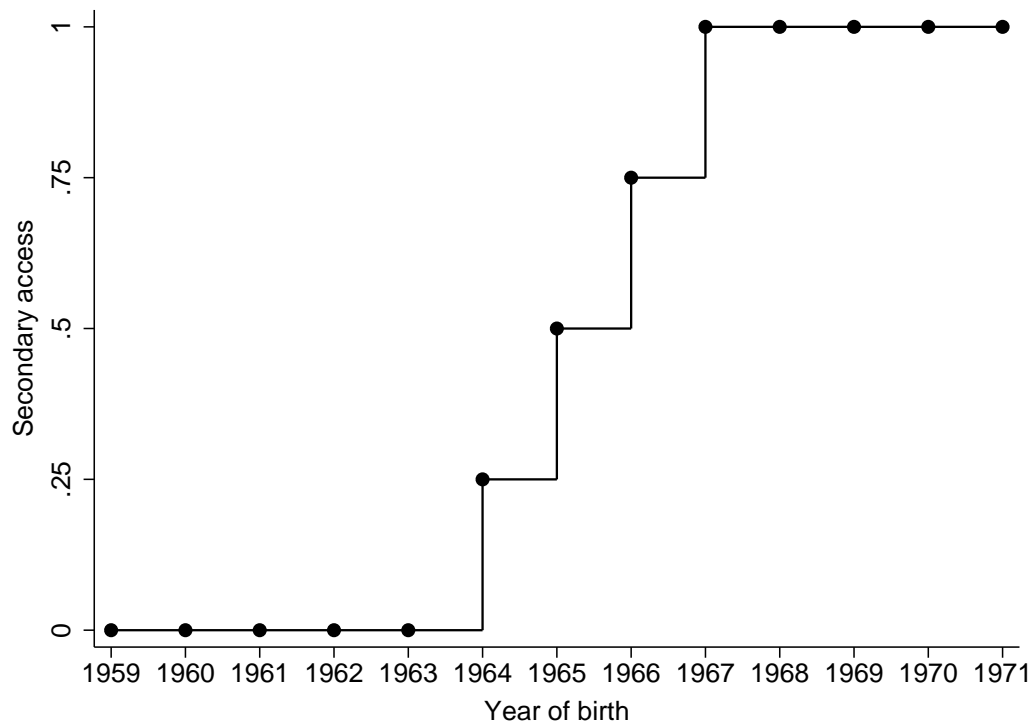


Figure 4: Operationalization of *Secondary access*, the Study's Key Treatment Variable

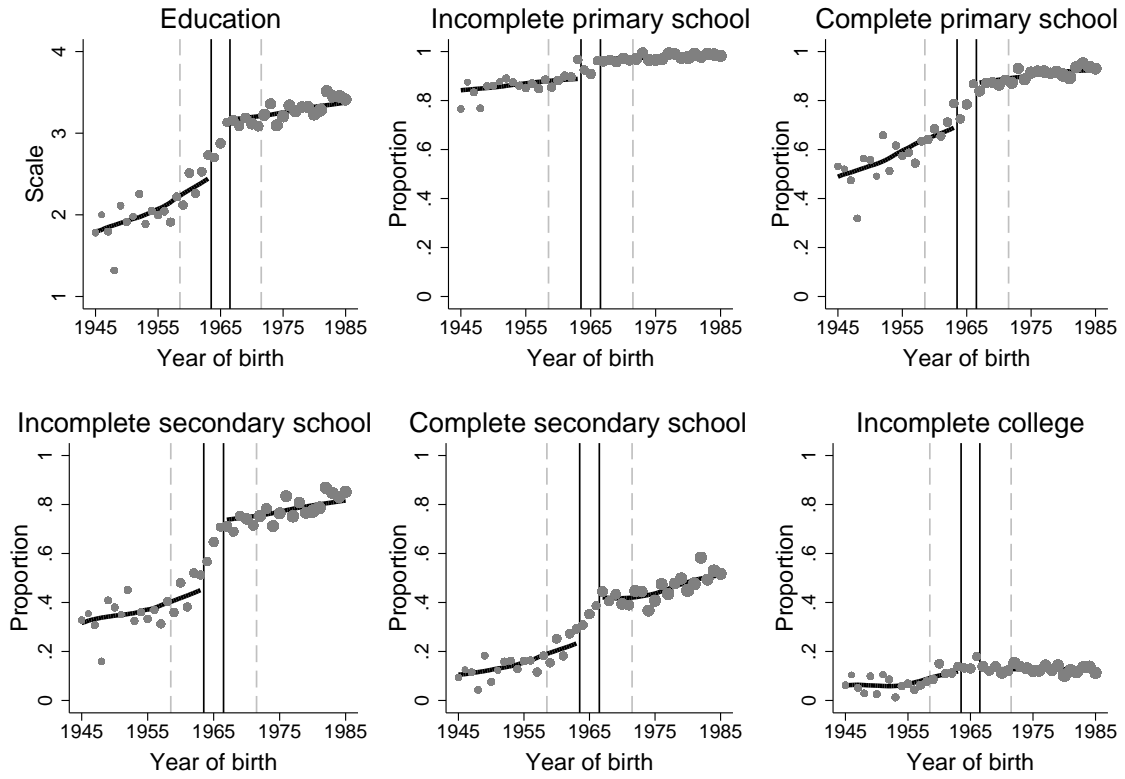


Figure 5: Trends in Educational Attainment by Cohort

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.

The fact that independence has undoubtedly brought about many other social, economic, and political reforms could potentially threaten causal identification. Since socialization processes operate differently at different stages of life (Alwin and Krosnick, 1991; Sears and Valentino, 1997), substantially older and younger constituents could vary in their response to political and cultural shocks, thereby violating our identification assumption. To maximize the comparability of treated and untreated respondents, our main analysis focuses on a “bandwidth” of five cohorts on either side of the reform cutoff years of birth (1963 and 1967). This allows us to better control for common shocks that may have affected younger and older cohorts differently.

We now assess the plausibility of our main identification assumption, that the untreated cohorts (in our sample of cohorts around the eligibility cutoffs) only differ from the treated cohorts with respect to their eligibility for secondary schooling. Although there is a slight long-run uptick in schooling for cohorts born too early to be affected by the reform, Figure 5 indicates that trends in education are relatively flat once we focus on the five cohorts on both sides of the reform’s cutoff point (inside the gray dashed lines). Below, we also document relatively flat trends in our political outcomes for cohorts on either side of the reform, which is consistent with the similarity of these cohorts along dimensions other than educational access. In our robustness checks, we demonstrate that trends are not driving our results by varying the size of the bandwidth, using placebo reforms, and including flexible cohort trends either side of the discontinuity (see also Agüero and Bharadwaj 2014, 490).

Furthermore, Figure 6 plots pre-treatment characteristics and district-level variables, comparing cohorts on both sides of the reform.²¹ The results suggest that our sample is balanced across treatment groups with respect to gender, age at the date of the survey, and district-level political aggregates. There is, however, a slight imbalance with respect to tribe: the first two plots point to a small difference in the proportions of Shona and Ndebele tribe members, Zimbabwe’s two largest

²¹The frequency of surveyed individuals is not affected by the treatment, suggesting that there is no differential migration across cohorts. Furthermore, the proportion of educated respondents in our surveys does not change after hyperinflation began in the mid-2000s.

tribes. We show below that our findings are robust to the inclusion of pre-treatment covariates, while the effects of access to education do not vary across tribes.

Estimation Strategies

Building on our 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 political effects of secondary education. Our first approach includes partially treated respondents. A key advantage of this approach is that leveraging differences in treatment intensity allows us to test whether common shocks associated with independence differentially affected cohorts with different levels of access to education. 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.²²

Access to public education, however, does not necessarily entail enrollment. Rather, the mapping from educational access—an opportunity that equally affects all cohort members—to an individual’s 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, we use access to secondary schooling to instrument for education.

²²Our results are robust to “double clustering” by both district and cohort (Cameron, Gelbach and Miller, 2011).

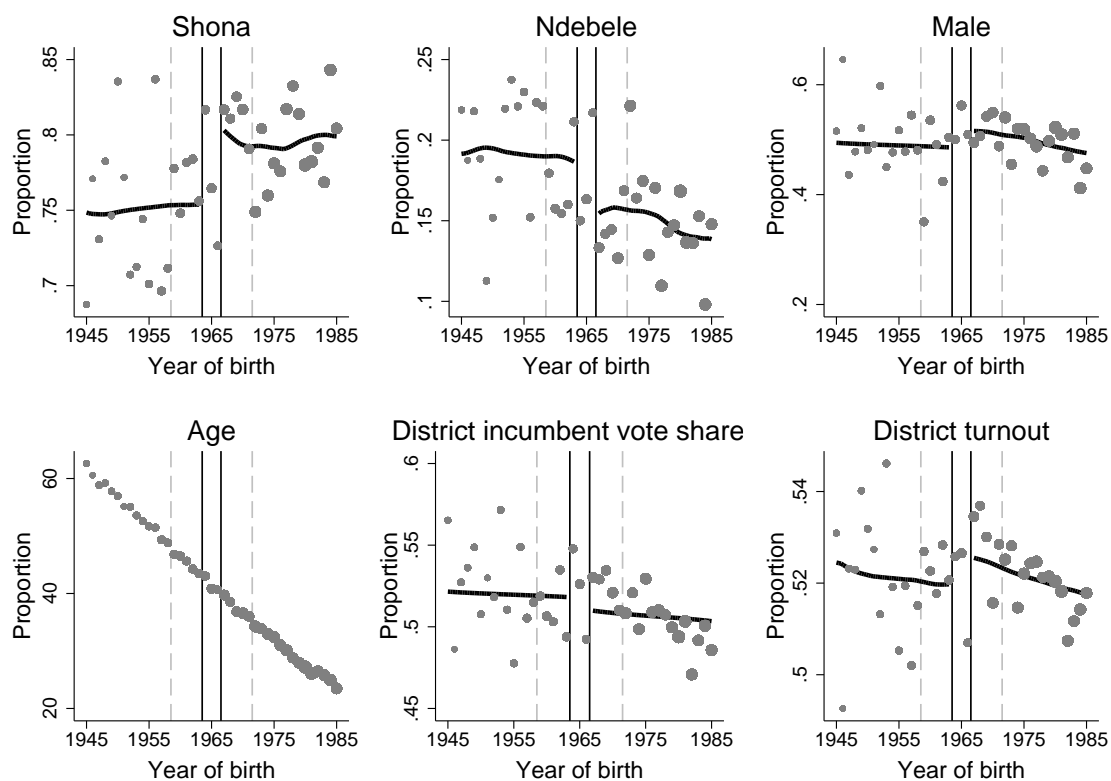


Figure 6: Trends in Pre-treatment Variables by Cohort

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.

Our IV analysis identifies the effect of education on Zimbabweans who received additional education because of the 1980 reform, but who would not have done so otherwise (Angrist, Imbens and Rubin, 1996). In the first stage, we estimate the effect of access to secondary education on a respondent's educational attainment:

$$Education_{icdt} = \delta Secondary\ access_c + \eta_t + \xi_{icdt}. \quad (2)$$

Taking the predicted values from this first-stage regression, we then estimate the following structural equation using two-stage least squares (2SLS):

$$Y_{icdt} = \beta Education_{icdt} + \eta_t + \varsigma_{icdt}. \quad (3)$$

Our coding of education follows that of 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.²³ Since any additional education may affect political behavior, we use the seven-point education scale (described above) as our endogenous independent variable. This allows us to retrieve consistent estimates of the average effect of an additional unit of education (Marshall, 2014).

IV estimation requires several additional assumptions. First, the relationship between instrument (secondary access) and the endogenous variable (education) must be strong. Indeed, the first-stage regression, reported in Table 1, produces a positive and highly statistically significant coefficient. The F statistic, which tests the relevance of the instrument in the first-stage regression, is 69, which far exceeds the standard critical value of 10 (Staiger and Stock, 1997). Second, the exclusion restriction requires that our instrument only affects political outcomes through increased

²³Intuitively, 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 that was induced to complete high school.

education. We discuss this assumption in greater detail below.²⁴

Our second approach drops all partially treated respondents, and thus compares only 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 approach allows us to focus on a sharp discontinuity in treatment assignment. By abstracting from partial treatment, we are able to implement a regression discontinuity (RD) design that relies on the weaker assumption that potential outcomes are smooth across the discontinuity.²⁵ Consistent results across both approaches should increase confidence in the study’s findings.

For this second approach, we again estimate Equation (1) to provide non-parametric RD estimates. Since access to schooling does not necessarily induce students to attend secondary school, we again estimate Equation (3) using 2SLS. This “fuzzy” RD design re-scales our reduced-form estimates to estimate the effect for black students who only remained in school because of the reform.

Education and Political Participation in Zimbabwe

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

²⁴There is no reason to suspect that monotonicity is violated. Even if there were some defiers, the predominance of compliers—as indicated by the large first stage—is unlikely to substantially change our estimates (de Chaisemartin, 2014).

²⁵By removing partially treated cohorts, we slightly abuse the RD design, because the running variable is truncated. Our RD requires that cohorts born in 1963 are comparable to cohorts born in 1967.

Table 1: Estimates of Education Reform on Educational Attainment

| | (1) Education | (2) Incomplete primary | (3) Complete primary | (4) Incomplete secondary | (5) Complete secondary | (6) Incomplete college | (7) 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Main Estimates

Contrary to the positive effects of education in advanced democracies (Sondheimer and Green, 2010) and democratic developing country contexts (Larreguy and Marshall, 2014), we find that in Zimbabwe, education substantially and significantly reduces levels of political participation. Column (1) in Table 2 reports a large reduction in political participation, as measured by our participation scale, across all estimation strategies. Relative to its sample mean of 0.64, Panel A shows that full exposure to secondary education reduces participation by around 10% (or one-fifth of a standard deviation). 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.²⁶

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 of the constituent variables. Instead, we find that a one-unit increase in access to secondary schooling and a one-unit increase in the education scale both cause a significant reduction in a set of non-contentious forms of political participation. In particular, our point estimates suggest that being exposed to the full treatment reduces voting by 8 percentage points, contacting one's local councilor by 6 percentage points, attending community meetings by 6 percentage points, and raising issues with others at a meeting by about 3 percentage points. Similarly, a one-unit increase in educational attainment reduces voting by 11 percentage points, contacting a local councilor 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 consistently significant.

²⁶Using indicators for each level of education reveals that each level of education further decreases participation until the effect plateaus at the college level.

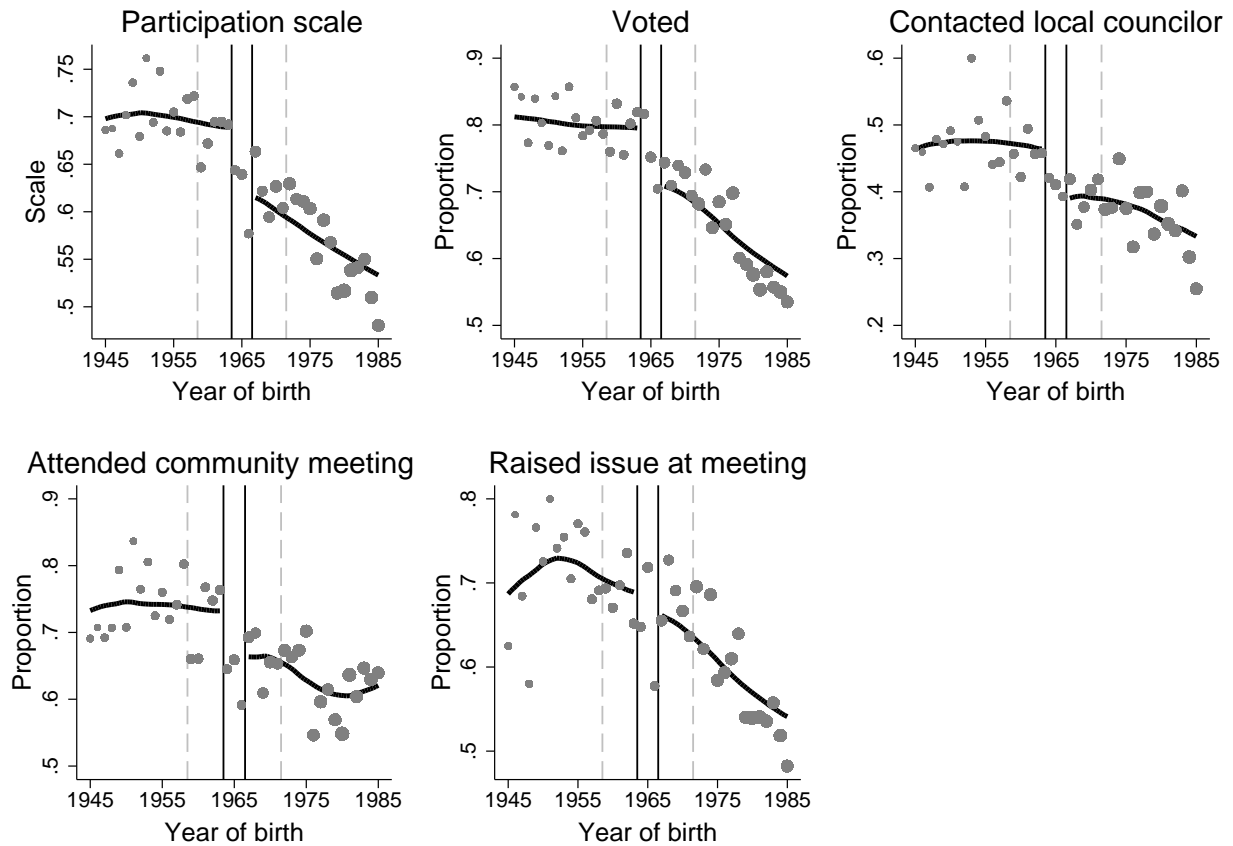


Figure 7: Trends in Political Participation by Cohort

Notes: Each gray dot represents the outcome mean 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.

Table 2: The Effects of Education on Political Participation

| | (1) Participation scale | (2) Voted | (3) Contacted local councilor | (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 (dummy) | -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 test the robustness of our results to different bandwidths. Panels A and B show that the reduced-form estimates are similar when we include either three or ten cohorts on either side of the reform eligibility threshold. Second, we show that our results are not driven by 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 cutoff. If trends were driving our results, we would expect to also find a reduction in political participation around the placebo reform. However, we find a significant reduction in participation for only one variable, raising an issue. We repeat this exercise and find no effects for placebo reforms in any year between 1960 and 1972.²⁷ Furthermore, Panel D shows that when we include 20 cohorts and cubic polynomial birth-year trends on either side of the reform cutoff, the reduced form RD estimates are robust.²⁸ Together, these tests support our identifying assumptions by showing that our results are not an artifact of specification choices or cohort trends.

As noted above, the most plausible confounding explanations relate to political differences between the cohorts immediately around the reform eligibility cutoff. First, one potential concern is a “first election” effect, such that respondents on different sides of the education reform behave differently because they first voted in different elections (Meredith, 2009; Mullainathan and 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 de-

²⁷The most recent placebo is 1972, which allows us to include five cohorts after the placebo reform including partially treated cohorts.

²⁸The inclusion of trends on either side of the discontinuity cannot be precisely estimated without extending the bandwidth. However, across all bandwidths, the point estimates have rather similar magnitudes.

Table 3: Robustness Checks

| | (1) Participation scale | (2) Voted | (3) Contacted local councilor | (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 (dummy) | -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 (dummy) | -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 3 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

crease 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, the Online Appendix shows that 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).

Although our design minimizes differences in citizen characteristics around the reform, 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 councilor slightly falls 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 point estimates increase.

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 behavioral 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 spillovers. However, if cohorts on either side of the reform cutoff interact with one another, spillovers are likely to reduce the effects of schooling as behavior 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 finally, 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 voters 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).²⁹

²⁹Comparing 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 eco-

Table 4: The Effects of Education on Political Participation, Before and After 2008

| | (1) Participation scale | (2) Voted | (3) Contacted local councilor | (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 (dummy) | -0.099*** (0.021) | -0.153*** (0.039) | -0.104*** (0.029) | -0.084** (0.034) | -0.054 (0.044) |
| Secondary access (dummy) × 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 |

Note: See Table 2.

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 education secondary statistically significant for respondents surveyed since 2009, while the difference in coefficients is statistically significant for voting and contacting a local councilor.

These results are also important because they cannot be easily reconciled with the alternative explanation that events which took place around independence affected age cohorts in different ways (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 people are more likely to be politically active because schooling provides the “skills people need to understand the abstract subject of politics, to follow the political campaign, and to research and evaluate the issues and candidates.”

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 and Brady, 1995). We

conomic crisis did not systematically differ by type of survey respondent. This suggests that such variables are not driving any differential response across these periods.

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*).³⁰ We also combine these three measures to produce an economic outcomes scale (*Economic scale*). The Cronbach's alpha for this scale is 0.41.

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 earn more money, 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. Furthermore, in the Online Appendix we show that none of the relatively permanent mechanisms for deliberate disengagement, such as interest in politics or access to information, changed between the pre- and post-2009 period. These results also sharpen our theoretical puzzle: despite greater economic resources, which should facilitate greater participation according to the current literature, we observe declining participation levels.

Education Increases 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*,

³⁰The poverty scale combines indicators for whether an individual has gone without food, medicine, or cash in the past year.

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 (dummy) | 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 |

Note: See Table 2

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 behavior, as informed citizens can more accurately assess government performance and the likelihood that participation can affect political change. We therefore test whether education both increases interest in politics and support for democratic institutions. Importantly, 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 that agrees or strongly agrees that understanding politics is not complicated. Third, *Discuss politics* is an indicator for the 73% of respondents that 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 organizations, are you against government closing news stations, are you against presidential discretion, are you in favor 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.

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 F statistic | 69.0 | 31.6 | 56.4 | 69.0 | 69.0 | 69.0 |
| Panel C: Regression Discontinuity (Reduced Form) | | | | | | |
| Secondary access (dummy) | 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 F statistic | 71.0 | 29.7 | 61.1 | 71.0 | 71.0 | 71.0 |

Note: See Table 2

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 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 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 councilors, 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.

Alternative Explanations

Finally, we eliminate alternative explanations of our argument of deliberate disengagement. 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

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 F statistic | 69.0 | 69.0 | 69.0 | 69.0 | 69.0 |
| Panel C: Regression Discontinuity (Reduced Form) | | | | | |
| Secondary access (dummy) | -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 F statistic | 71.0 | 71.0 | 71.0 | 71.0 | 71.0 |

Note: See Table 2

receiving a gift from a political party before the most recent elections.³¹ We also create an indicator, *Freedom to choose vote*, which proxies for pre-commitment to a party in exchange for some benefit.

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 signaling 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 election campaigns 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 around the discontinuity. 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 that they will be repressed or their belief that voting is monitored. Although the point 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

³¹This question has recently been used to proxy vote buying (Kramon, 2014).

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) |
| Secondary access \times Incumbent share | | | 0.047 (0.073) | |
| Secondary access \times 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 \times Ndebele | | 0.060 (0.075) | | 0.057 (0.090) | |
| Secondary access \times Violent events | | | | | -0.00001 (0.00002) |
| Observations | 918 | 918 | 918 | 918 | 1,847 |

Notes: See Table 8.

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).

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 cutoff 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. Demonstrating that more-educated citizens are likely to withdraw from the political sphere is important, because it may shed new light on some of the reasons why many initially liberalizing countries have gotten “stuck” in hybrid equilibria (Carothers, 2002). As such, our findings also contribute to theories of autocratic regime stability.

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 and West, 2014).

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