



Effects of education on political engagement in rural Burkina Faso

Elodie Djemai^a, Michael Kevane^{b,*}

^a Université Paris-Dauphine, Université PSL, LEDa, CNRS, IRD, Place du Maréchal de Lattre de Tassigny, 75775 Paris Cedex 16, France

^b Santa Clara University Dept. of Economics Santa Clara, CA 95053, USA



ARTICLE INFO

Article history:

Accepted 8 January 2023

Keywords:

Education

Voting

Natural experiment

Africa

Burkina Faso

ABSTRACT

Many African countries are both consolidating their democratic institutions and continuing to expand mass primary schooling. In this context, citizens may be interested in the broad general effect of education on political engagement. Recent social science work estimating this effect has not arrived at consensus, with researchers suggesting the relationship may be context dependent, and could vary from positive to negative. We apply an instrumental variable (IV) approach, using Afrobarometer surveys in Burkina Faso over the 2008–2019 period, merged with data on the timing of school establishment at the village level. Individual schooling attainment is instrumented by whether a school was established in the village of residence when the person was seven years old. The data is finer than recent papers that estimate the relationship using national-level quasi-experiments where education access changed across birth cohorts or where an indirect proxy measure of education access varied across regions and birth cohorts. We find that the relationship appears to differ by gender: men exhibit a substantial negative effect of education on engagement, while women exhibit no sizable relationship. The null effect for women may be due to low power, as there is less variation in education outcomes for women in rural areas. The results suggest that gender may be an important mediator of the direction and magnitude of the complex relationship between education and political engagement in politics with low overall levels of schooling.

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1. Introduction

The advent and spread of mass schooling in the 19th and 20th centuries overlapped, to some degree, with the movement towards open, democratic societies where many citizens became eligible to vote in free, fair, and secret ballot elections. There is evidence that the two processes were linked, in many places, as educators incorporated civics into school curricula, as elected official expanded public spending on schools, and as electoral processes and limits on the franchise were reformed (Gallego, 2010; Paulsen, Scheve, and Stasavage, 2021). It may also have been the case that rulers in non-democratic localities invested in education, and this eventually spurred movements towards democratization (Paglayan, 2021). Social scientists have thus been interested in the relationship between the acculturation that children and young adults undergo in formal schooling and their expressions of political engagement later in life. Citizens who are more educated may be acculturated to view political engagement as an obligation and virtue of citizenship. They may be more likely to engage in critical thinking, which may influence their decisions about the nature

and extent of their political engagement. They may be acculturated to disengage with politics, as schooling may condition young people to embrace an existing non-democratic political order. Moreover, varying political contexts may make political engagement more or less rewarding for educated citizens relative to uneducated citizens.

The acculturation and changed incentives to be politically engaged induced by formal schooling may vary by gender, in different societies and in different contexts. Communities with social norms that proscribe women from working outside the home or household farm may both discourage their education and their political engagement. Indeed, many early democracies, such as the United Kingdom, the United States, and France, limited the vote to men for more than a century. While after independence most former colonies adopted universal suffrage, in practice in many countries social norms discouraged women's participation (Amoateng, Kalule-Sabiti, and Heaton, 2014; Iyer and Mani, 2019; Prillaman, 2017; Tripp, 2001). The discouragement of women's political participation may have had significant consequences in terms of allocating local and national budgets across different public goods and policies, and increasing the influence of traditional leaders, who often have supported patriarchal norms (Chattopadhyay and Duflo, 2004; Clayton, 2014; Gottlieb,

* Corresponding author.

E-mail address: mkevane@scu.edu (M. Kevane).

Grossman, and Robinson, 2018; Hudson, Bowen, and Nielsen, 2015). Moreover, democracies with significant competition across parties may have political parties responding more to women's differing patterns of political engagement (Giné and Mansuri, 2018; Hern, 2020; Wantchekon, 2003; Yoon, 2020).

Many studies on political engagement find a general gender gap in favor of men being more likely to participate than women, even though the gap tends to disappear in high-income countries. Using the 2005 Afrobarometer surveys, Coffe and Bolzendahl (2011) found a significant gender gap in collective action engagement (attending a community meeting and joining others to raise an issue), but no gender gap in terms of registration to vote, in most of the 18 countries considered. Isaksson, Kotsadam, and Nerman (2014) found evidence along the same lines; pooling the Afrobarometer survey data from 20 African countries, women tended to be less likely to vote and to join with others raise an issue, the gender gap being four times larger in the former than the latter. They suggested that clientelism, restricted civil liberties, economic development, and gender norms were important drivers of the participatory gender gap. In the case of Burkina Faso, Ozdemir, Ozkes, and Sanver (2021) found no significant relationship between gender and either voter registration or turnout in Burkina Faso.

The question addressed in this paper is not whether women and men have different behaviors on average, and what accounts for that gender gap, but whether the effects of education, on political engagement, vary by gender. Estimating the relationship between schooling and political engagement, and how that link might vary by gender, is difficult, since individual schooling attainment may be correlated with unobserved individual or community propensities to be politically engaged. That is, both schooling attainment and engagement may be influenced by unobserved variables. For example, localities with community-level norms favoring engagement for men and against participation by women might also be strongly in favor of formal education for men and not so much for women. Communities more likely to be politically engaged may also be more likely to provide, through collective action processes, opportunities for education. Individuals with a higher propensity to be civically engaged may decide to pursue more education.

We use five waves of Afrobarometer surveys collected in Burkina Faso over the 2008–2019 period (Afrobarometer, 2021) to estimate the effect of education on political engagement in rural Burkina Faso, separately for men and for women. Our estimation approach builds on that of André and Maarek (2022), who used original survey data on political engagement in Mali. Their survey also recorded the timing of school establishment in the locality of residence of the individual. They thus construct a “treatment” variable, indicating whether an individual was of school age before or after the establishment of the village school. This can then be used as an instrumental variable. The first stage is estimated as a regression discontinuity design. In our estimation, individual-level data from the Afrobarometer is merged with administrative data on school establishment from 1900 to 2004 and with geographic data on the locations of all villages in Burkina Faso. As there are several respondents from each gender for each location in the sample, and the respondents are of different ages, the method controls for likely persisting locality-level relationships between possibly gendered local preferences of valuing schooling and local preferences about political participation.

We conclude, from the range of estimates, that the magnitude of the relationship between education attainment and political engagement is quite negative for men, and apparently negligible for women. As with most statistical analysis of observational data, there is a veritable “garden of forking paths” regarding choices made by researchers (Gelman and Loken, 2014). We therefore pre-

sent a variety of specifications to ensure that results are reasonably robust. The sizable negative relationship for men may simply be an unlikely and possibly idiosyncratic artifact of the particular sample, and the lack of statistically significant relationship for women may be indicative that the statistical test lacks power. The Afrobarometer sample for Burkina Faso is about 4,300 individuals observed in rural areas, with about half in each gender. We find that this sample size is likely large enough for estimation techniques such as instrumental variables in a repeated cross-section setting to detect reasonable sized effects for the sample of men. For women, however, there is less variation in the education outcome, and so the statistical test of the relationship is somewhat under-powered for relevant effect sizes.

The remainder of the paper is organised as follows. Section 2 discusses the empirical literature on the relationship between schooling and political engagement. Section 3 presents the context of Burkina Faso regarding schooling access and achievement over time and democratic elections. Section 4 describes the Afrobarometer survey and the school location data. The estimation strategy is presented in Section 5 and the regression results appear in Section 6. Extensive robustness checks are presented in several appendices. Power considerations are investigated in Section 7. Section 8 supplements the analysis of the effects of education on political engagement with an analysis of the effects of education on possible mechanisms, namely perceptions about democracy and corruption, trust in politicians, and living conditions. Section 9 concludes.

2. Schooling and political engagement

Schooling attainment may or may not be related to various indicators of political engagement, and the direction of causality may run both ways (Acemoglu et al., 2005; Diwan and Vartanova, 2020; Paglayan, 2021). In correlational analysis with data from African countries, Mattes and Mughogho (2009:17) suggested that: “Education makes an extremely limited contribution to political participation. Educated respondents are more likely to contact formal officials (a more individualistic form of participation), they are no more or less likely to vote, identify with a political party, join civic groups, get involved in community affairs or protest.” Isaksson, Kotsadam, and Nerman (2014) found that the level of education was not significantly related to the probability of voting in the previous election while it was significantly correlated with the response to a question about whether the person had “raised an issue” with others in the community. The size of the correlation did not differ by gender.

Correlational analysis, however, can be a very biased method to estimate the relationship, whether in aggregate or disaggregated by gender. Le and Nguyen (2021) suggest that findings of positive, negative, or no correlation between education and participation should be understood as emerging from three processes. First, there might be causal channels running from changed attitudes and values, acquired as a result of schooling, to changed political engagement. Second, the relationship might emerge from more indirect mechanisms, such as where education changes to social networks or economic positions of those with higher attainment, and these in turn induce changed participation. Third, there might be confounders, in that other variables are leading to both greater education and greater participation.

Formal education in most societies is designed to deliver both critical reasoning skills and a bundle of attitudes, ideas, or memes that acculturate young people. This bundle when unpacked might consist of patriotism, civic virtues, idealism, nationalism, knowledge about history and society, elitism, class consciousness, tolerance, respect for differences in values, etc. Important in these

attitudes might be whether, as a citizen, a person thinks that democracy is an ideal or desirable form of governance.

A person might also develop different approach to understanding the quality of existing governance, and appreciate what improvements might be attainable. Attitudes towards and understanding of corruption, the abuse of public office for private gain, might be shaped by schooling. Analogously, a person might change their confidence in the integrity of public officials. Education might change the level of trust that people have about whether politicians and bureaucrats are effective public servants.

To these attitudes might be added change in executive-level cognitive capabilities, psychological development of feelings of self-efficacy, empowerment, and confidence. Some education programs are intended to inculcate a *meta*-habit of considering a variety of contradictory thoughts (ideas, or meanings) and approaching decisions by thinking and evaluating them in the light of contextual knowledge; that is, specific attitudes and preferences to be acculturated are not intended but rather are emergent properties. Moreover, schooling is also about obedience and discipline: showing up on time, completing assignments on time, taking tests, being quiet when the teacher is talking, not questioning the teacher, etc.

Because of the complexity and bundled nature of schooling, political psychology does not have a set of presumptively valid hypotheses concerning the general effect of formal schooling on political engagement (Basu, 2002; Bruch and Soss, 2018; Lieberman and Zhou, 2020; Wolak, 2020).

Indirect mechanisms or channels for effects of education attainment on participation are similarly multi-faceted. There is a temporal remove between the occasion of schooling and the occasions of political engagement. Schooling occurs during a person's youth, typically from age 6 to 18, and, for some, into university. Political engagement for most people starts after age 18, when people become eligible to vote, and continues through life, and indeed evolves through life. As Larreguy and Marshall (2017:400) noted: "it is important to differentiate the skills and values learned at school (...) from downstream effects such as increased income, community interactions, and empowered social status." Education attainment by age 18 influences the path of life experiences and lifelong learning of a person, which in turn influences the person's (usually) unobserved mindset as an older person (which is likely still influenced by their mindset developed during their years of pre-adult schooling). The reasoning skills, and the bundle of attitudes, capabilities, and ideas acquired during schooling presumably initiate a trajectory of continuity and change of mental habits, predispositions, and knowledge components over the adult life-cycle. Schooling clearly changes, for example, the economic and social opportunities for individuals. An effect of these changed opportunities is likely higher lifetime income. Higher income levels may change the mental calculus of opportunity costs of engagement and preferences for political parties who may have platforms that address the interests of different income categories. Moreover, age and life experiences produce wisdom (e.g., the common saying in West Africa that when an elder dies it is as if a library had burned to the ground). The trajectory of acquisition of wisdom is presumably influenced by schooling attainment. The capability and habit of reading, for example, might be expected to change the wisdom of a person. Literate people, for example, have relatively easier access as adults to newspapers and books, which may be very efficient transmitters of memes, thus shaping mindsets.

To go beyond the correlational analysis, controlling for the confounded relationship between education attainment and political engagement has been an important concern of applied empirical work in this area. Methodologically, Card (1995), Card (1999), Currie and Moretti (2003), and Duflo (2001) pioneered the

approach of using school establishment as an instrumental variable (IV) for educational attainment, in work examining the returns to schooling. Several studies in lower-income contexts with low schooling attainment have leveraged the greater credibility of these recent approaches to estimate the effects of education on political engagement. Some have used significant schooling reforms as natural experiments, and some have used the staggered timing of schooling establishment, across villages or regions, relative to birth cohorts close in age, to identify causal effects. Several relevant studies are summarized in Table 1, which lists the country, the identification strategy, key sample criteria and approximate sample size, and our understanding of the main result.

Most relevant for this paper, André and Maarek (2022) used original individual-level survey in Mali that recorded education outcomes, political engagement, timing of school establishment, and covariates proxying for family standing. They used a donut regression discontinuity design to credibly estimate causal effects among people aged 40 and more. The establishment of a village school in Mali substantially increased schooling attainment of the "treated", those of school age at the time of establishment, relative to those who were too old to attend the new school. The higher education attainment that resulted from greater access to schools then increased participation in village associations and involvement in local political life, although it did not have substantial effects on voter registration or likelihood of voting. They found that ever attending school significantly increased the likelihood of being a member of a neighborhood association by 30 percentage points. Schooling had no significant effect on participation in elections, as measured with a four-point scale referring to the elections of 2007 and 2009 and to both registration and voting behavior. André and Maarek (2022) also found that greater education access among people coming of school age after a school was established increased the proportion of local elected politicians. Moreover, they suggested that the effects of schooling were heterogeneous, and thus unequalizing, in that better-off families saw a larger effect of schooling on participation, including in terms of becoming local political leaders. When they estimated their effects separately by gender, the point estimates of the effect of education on political engagement for women were generally larger in absolute value (most were positive) than those for men. However, the estimated coefficients were no longer statistically significant, perhaps due to the smaller sample sizes.

Three of the studies noted in the table have used Afrobarometer survey data to investigate the causal effect of education on political participation. Croke et al. (2016) found that the expansion of secondary schooling in Zimbabwe in 1980 led to reduced political engagement. Larreguy and Marshall (2017), on the other hand, estimated that the expansion of primary schooling in Nigeria in 1976 was associated with increased political engagement. The contrasting findings of the two papers suggest that the relationship may vary by political context: educated people are perhaps more likely to participate when participation is meaningful in that political leadership is perceived as willing to accept a peaceful transfer of power through elections. Larreguy and Liu (2022) found that the relationship between education attainment and political engagement in Senegal indeed varied depending on the timing of the Afrobarometer survey round, positive during periods of democratic erosion and negative during periods of ordinary single-party politics where electoral *alternance* was unlikely.

Two of the other studies in the table similarly leveraged national education reforms to more credibly estimate how schooling affects political engagement. Dang (2019) estimated that additional schooling increased engagement in Vietnam, while Parinduri (2019) found no statistically significant relationship in Indonesia.

Table 1
Various identification strategies for estimating effect of education on political engagement.

Study	Country	Identification strategy ^a	Sample ^b	Main result ^c
André and Maarek (2022)	Mali	RDD-IV; Staggered local (village-level) school establishment	Include only people over age 40, exclude “partially treated.” N ~ 950.	Ever attending school increased the likelihood of being a member of a neighborhood association by 30 percentage points. Schooling had no significant effect on participation in elections, as measured with a four-point scale referring to two different elections and to both registration and voting behavior.
Croke, Grossman, Larreguy, and Marshall (2016)	Zimbabwe	RDD-IV; 1980 expansion of secondary school access.	Bandwidth of five cohorts on either side of the partially treated (1963–1967). N ~ 1,842.	Education measured on a seven-point education scale. Participation scale the sum four measures of involvement. One-unit increase in education reduced participation scale by ~ 15 % relative to sample mean of 0.65, reduced probability of voting by 12 percentage points, lowered probability of attending a community meeting by 9 percentage points, and reduced probability of raising an issue by 6 percentage points (latter not statistically significant).
Larreguy and Marshall (2017)	Nigeria	DD-2SLS; 1976 expansion of primary school access; differential intensity of treatment by regions.	Exclude cohorts born before 1950. N ~ 16,000.	One-unit increase in education measured by a six-category scale based on the respondent's highest level of education had positive effect on 10 measures of political engagement (significant in 8 cases). A one-unit increase in education raised likelihood of voting by 22 percentage points, 21 pp for attending community meeting, and 19 pp for being an active association member.
Larreguy and Liu (2022)	Senegal	DD; Early 2000 s expansion of middle school access; differential intensity of treatment by regions.	Exclude cohorts born before 1970. N ~ 3,000.	High-intensity exposure to reform, relative to low-intensity regions, increased index of political participation (voting in the last election, joining others to raise an issue, or contacting local or national politicians) relative to participation of pre-reform cohorts, but only for people surveyed during periods of democratic erosion.
Dang (2019)	Vietnam	RDD-2SLS; 1991 compulsory schooling reform requiring enrolment for children under 15.	15 years bandwidth on either side of cutoff (age 15 in 1991). N ~ 1,450.	An additional year of schooling increased the likelihood of paying attention to political topics or to treat political issues as daily concerns by about 6–12 percentage points, and increased the likelihood of participating in political activities or of being a member of a political organization by about 6–8 percentage points.
Parinduri (2019)	Indonesia	RDD-2SLS; 1978 one-time extension of school-year by 6 months as government changed academic year.	Optimal bandwidth estimated using algorithm. N ~ 3,100.	No significant effects on whether voted or what criteria used when voting (i.e., “factors voters considered important”).

^a We note the main identification strategy. Some studies presented results from several identification strategies.

^b We note approximate sample size for main estimations. Estimation samples vary across identification strategies implemented and specifications. E.g., some specifications exclude cohorts that were partially treated by reform.

^c We note a basic main result. Studies had multiple specifications and robustness checks, with varying estimates of coefficients of interest.

3. Schooling and democratic elections in Burkina Faso

Burkina Faso may be a good setting to estimate the effects of education on engagement and thus add to the growing evidence base. Access to primary schools, among the lowest in the world, accelerated in the early 1980 s. The political context over the 1991–2020 period was at the “semi” end of semi-authoritarianism, with gradual and significant liberalization of political space after a 1998 social movement protested the assassination of independent journalist Norbert Zongo, and especially after a street uprising in 2014 that ousted longtime president Blaise Compaoré (Andrews and Honig, 2019; Raffinot et al., 2015). Women have had both lower levels of schooling and lower levels of political participation (as measured by voting participation and other indicators) compared to men. Women representation in the upper levels of politics (as national candidates or members of the national assembly) remains low (Hagberg, Kibora, et al., 2021; Rouamba and Descarries, 2010).

Burkina Faso (formerly Upper Volta) became an independent country in 1960, after more than 60 years of French colonial rule. Complex political machinations led to the election in the national territorial assembly of Maurice Yaméogo as president prior to independence. Yaméogo then proceeded to dismantle open electoral competition, replacing it with a one-party state (as in many other countries of the era). General Sangoulé Lamizana toppled Yaméogo in 1966 through a bloodless coup, following urban unrest and strikes. There were several legislative and presidential elections,

and several constitutional referenda, during the unstable period of quasi military-civilian rule that lasted 25 years from 1966 until 1991. In 1991, President Blaise Compaoré, who had taken power with others in a military coup in 1983, initiated a gradual but apparently steady transition to civilian rule. Compaoré retired from the military, and most leadership posts were given to civilians. His semi-authoritarian regime was, however, backed by a strong presidential guard unit of the military, and endured until a popular uprising in 2014. A brief transition government with joint civilian-military leadership handed power back to an elected civilian government in January 2016. A low-level insurgency that began in the north of the country has led to more than 1.5 million displaced people by 2022, and disrupted the 2020 presidential and legislative elections in several regions in the north and east. President Roch Marc Christian Kaboré, reelected in 2020, was ousted in a coup d'état in January 2022, in turn followed by a second coup in September 2022. Both coups were met with apathy among the citizenry (Alloukoutoui and Koussoubé, 2022; Engels, 2022; Sama, 2022), in stark contrast to a September 2015 failed coup attempt that produced significant street protests.

For presidential elections, turnout during the Compaoré regime was fairly low: 25 % in 1991, 56 % in 1998, 58 % in 2005, and 55 % in 2010. Turnout was similar for national legislative elections held in 1992, 1997, 2002 and 2007 (Elischer, 2013; Loada, 2006; Loada and Santiso, 2002; Santiso and Loada, 2003). The 2006 countrywide municipal elections where rural councils were first elected had turnout of 49 %. The joint legislative and municipal elections of

2012 had a turnout rate of 76 %. Turnout was 60 % in the first post-Compaoré presidential and legislative elections of 2015, with 3,309,988 voting out of 5,517,015 registered voters. Turnout was considerably lower in the presidential and legislative elections of 2020, with 2,972,590 votes recorded for 5,918,844 registered voters, for turnout of about 50 %.

There has been limited quantitative research on the determinants of voter registration and turnout in Burkina Faso. [Ozdemir, Ozkes, and Sanver \(2021\)](#) used original survey data to estimate the determinants of registration and voting in the 2015 elections in Burkina Faso. They suggested that socio-demographic variables such as income, education, and marital status were significant determinants of voter registration, but only ethnicity was a significant determinant of turnout on election day.

Burkina Faso had relatively low rates of primary and secondary schooling into the 1990s, typical for former French colonies of the Sahel region. The French colonial authorities had little inclination to spread schooling widely. Some factions in the colonial administration opposed the Catholic missionaries who sought to expand schooling. The missionaries were constrained, in any case, by the limited number of White Fathers taking up missionary work in the colony. When Protestant schools started spreading in the 1930s, the Catholic missionaries often allied with sympathetic administrators to slow the spread of Protestant schools. By 1960, the territory had about 400 public and private primary schools, with a population of about 5 million people.

After independence, the first presidents, Maurice Yaméogo, himself a former schoolteacher, and Sangoulé Lamizana, who had only finished primary education level (typical for ex-colonial military officers), appeared to have been relatively uninterested in investing in schooling. Around the late 1970s, the groundwork for more rapid expansion of primary schools was laid. The military coups and period of interregnum in the early 1980s were followed by the 1983 revolutionary regime of Captain Thomas Sankara, who might have been expected to accelerate even further the expansion of schooling. However, Sankara was opposed by one of the two important teachers unions, and he fired more than 1,000 teachers in 1984, slowing the rate of primary school expansion from what it might have been.

Because primary schooling is not yet universal, and secondary schooling remains rare, a literature has emerged exploring parental decision-making regarding the choice to send children to school or have children remain at home and work in household activities such as farming ([Compaoré and Ouédraogo, 2007](#); [Jean-François, 2007a](#); [Jean-François, 2007b](#)). This work has found that production profitability and risk, and the return to child labor, are significant determinants of regional variation in schooling enrolment and attainment ([Kazianga, 2012](#); [Kazianga and Makamu, 2017](#)). Evaluations of the BRIGHT school program in northern Burkina Faso convincingly demonstrated, though, that access to schooling was a key determinant of schooling enrolment and attainment ([Ingwersen et al., 2019](#); [Kazianga, Levy, et al., 2013](#)).

We use the terms education and schooling interchangeably. In some countries and regions, religious schooling with curricula regulated by the government has been as important as public, secular schooling. Burkina Faso has seen both Christian-oriented private schools and Muslim oriented private schools (*écoles Franco-Arabes, madrasas*), and so potential differential effects might have been, and possibly remain, significant. However, the large majority of primary schools have been and remain secular public schools in the entire country.

There has been no quantitative work, that we are aware of, examining the relationship between education and political engagement for Burkina Faso. We should note, however, that student unions and associations, at the secondary and tertiary levels, have been very active in protest movements ([Bianchini and](#)

[Korbéogo, 2008](#); [Mazzocchi, 2006](#); [Sory, 2012](#)). Ethnographies of students also often remark on the politicization of students ([Bianchini, 2018](#); [Mazzocchi, 2006](#)).

4. Data description

4.1. The sample

We use five rounds of the nationally representative Afrobarometer survey collected in Burkina Faso (2008, 2012, 2015, 2018, 2019). The sampling design was in two steps. First, enumeration areas were randomly selected, and then in each sampled enumeration area, eight respondents were randomly selected to participate in the survey. 1200 respondents were surveyed in each round, for a total of 6,000 respondents. For each of the enumeration areas included in the survey rounds, we observe the name of the locality and the GPS coordinates. This enables us to match the localities to the school list, and so determine whether there is a primary school in the sampled villages and if so, the date of establishment.

The analysis sample for this paper consists of rural residents only, and those born on or after 1950, which leaves a sample of 4,520. We exclude residents of large towns and cities (the two principal large cities of Ouagadougou or Bobo-Dioulasso account for about 70 % of those excluded) because respondents in almost all urban areas were born after school founding dates, which in most towns and cities were before 1940. We include the few towns that saw their first schools established after 1940. The sample is further restricted to drop young respondents born after 1999 for whom it was not known whether their village of residence had a school established between 2006 and 2009, when they turned age 7, and some of whom were too young to vote at the time of the respective survey. We thus have a sample size of 4,432 respondents.

The Afrobarometer surveys are repeated cross sections, and the sample is independently drawn in each wave, so that the probability of observing an enumeration area in more than one round is rather small. It turns out that we observe a total of 525 villages: 474 villages are observed only once, 38 villages are observed in two rounds, 7 in three rounds, 6 in four rounds and 0 in five rounds. If instead we use the GPS coordinates to define the enumeration area, we observe a total of 537 unique GPS points: 500 are observed only in one round, 33 are observed in two rounds, 4 in three rounds, and 0 in four or five rounds. (We note that the main conclusions of the paper still hold when weighting the observations in the first-stage and second-stage regressions by weights equal to the square root of the number of respondents per village.). [Fig. 1](#) shows the locations of each village included in at least one of the five survey rounds.

4.2. School data

Data on the year of school opening comes from a document, *Répertoire des écoles publiques et privées du Burkina Faso en 2004–2005* (called *Répertoire* hereafter) produced by the *Direction des études et de la planification* of the *Ministère de l'enseignement de base et de l'alphabétisation* (MEBA) in March 2006. The document contains a listing of 6,913 public and private primary schools in the country, whether the school was urban or rural, the number of students and classrooms in each school, and the opening date of the school. There are no missing values in the report for founding dates. The number of schools listed accords exactly with another annual report produced by the MEBA on the number of schools in the country in 2005. It seems unlikely that any established pri-

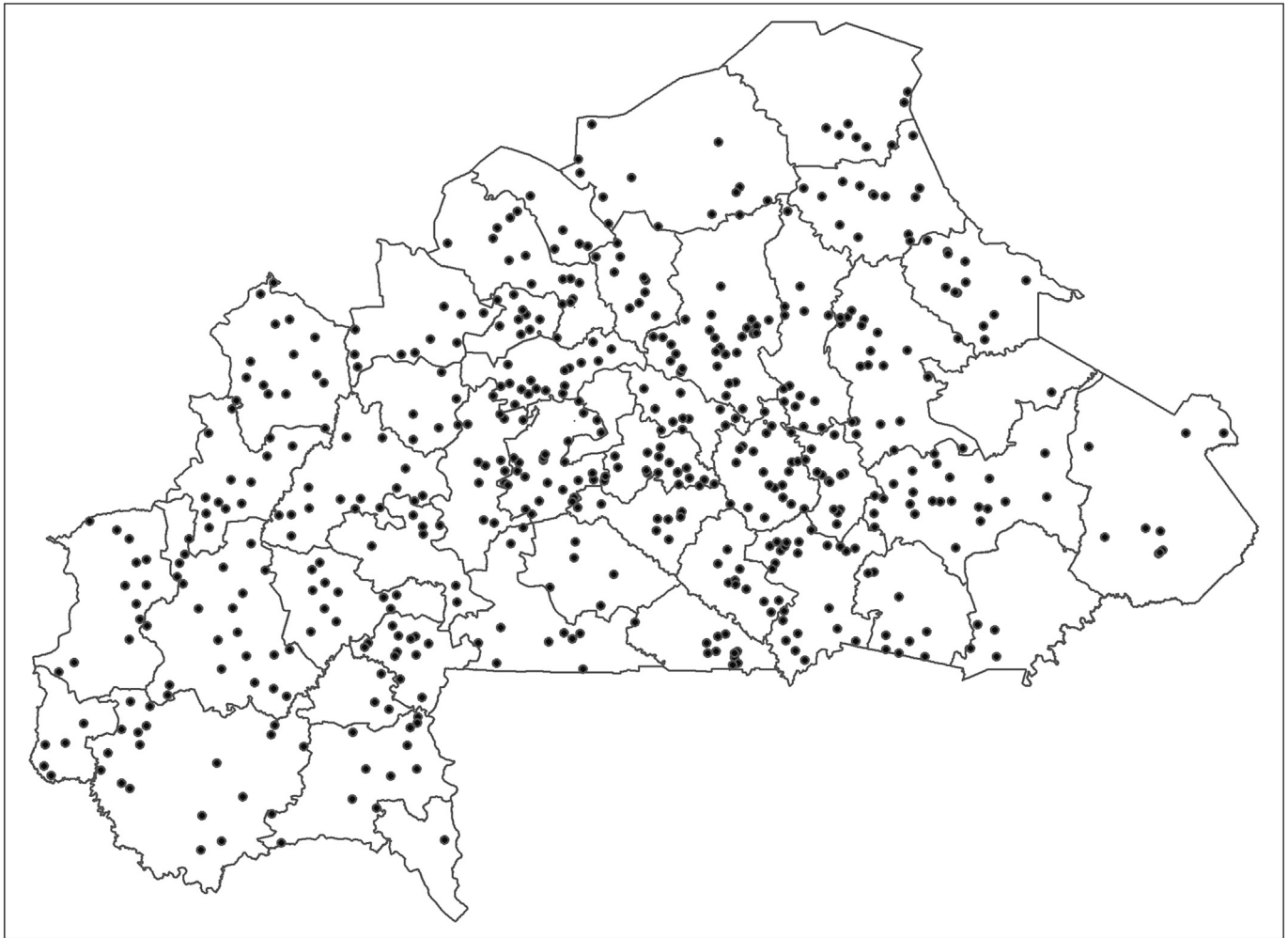


Fig. 1. Location of enumeration areas included in Afrobarometer surveys, Burkina Faso Note: Afrobarometer, five rounds (2008, 2012, 2015, 2018, 2019). Province boundary lines are shown (45 provinces). As discussed in text, most villages are observed in only one round of the survey.

mary school, whether public or private, would have not been included in the list.

Over the 6,913 schools, 85.5 % were public schools, 7.0 % arabic or franco-arabic schools, 4.2 % private secular schools, and 3.0 % private Catholic or Protestant schools. There were six levels in primary education in Burkina Faso, but most rural schools only had three classrooms, with each classroom teaching to two levels. In our data, if we restrict the sample of schools to the 5,447 rural schools, in 2005 about 15 % had six classrooms (a few had more than six), about 8 % had between four and five classrooms, about 50 % had three classrooms, 13 % had two classrooms, and 14 % had a single classroom.

Over the period 1898–1955, 194 schools were established in the country, while from 1955 to 2004 and additional 6,719 schools were established. There was a break in the time series around 1980 when school establishment accelerated. (Statistical tests of breaks in the time series at the national level and by region confirm the clear visual pattern.) Fig. 2 shows that the regional pattern of school establishment over the period 1955 to 2004 was quite varied. Many regions did see acceleration around 1980, but some did not. Regions in the southwest of the country (Cascades, Centre-Sud, and Sud-Ouest) and in the east and northeast of the country (Centre-Est, Est, Sahel) saw fewer schools established in general, and took longer to accelerate. Several core regions saw dramatic

acceleration around 1980 (Boucle du Mouhoun, Centre-Nord, Centre-Ouest, Haut-Bassins, Nord).

Online Appendix A describes the *Institut géographique du Burkina Faso*(IGB) geo-location data for all villages in the country, and the extensive data cleaning that was necessary for the school list to be matched to both Afrobarometer respondent villages and to geo-coordinates. The online appendix also describes some of the issues in merging the various files.

We first merge the Afrobarometer data with the IGB list of geo-located villages. That is then merged with the *Répertoire*. We calculate two measures of access. First, we use the timing of the establishment of a primary school in the person's village itself. The "treatment" variable takes on value one if there was a primary school in the village of residence when the person was seven years old (and thus eligible for schooling). A second more complex treatment measure is the weighted sum of schools around the village when the person was seven years old, where the weights are the inverse of the square of the distance. For every Afrobarometer village and every year after 1950, we construct a variable equal to the number of primary schools within 30 km of the village, weighting each school by the simple Haversine distance between the village and the school. The weight for each school is the inverse of the distance squared. (We add 1 km to each distance, to handle the 0 km cases.).

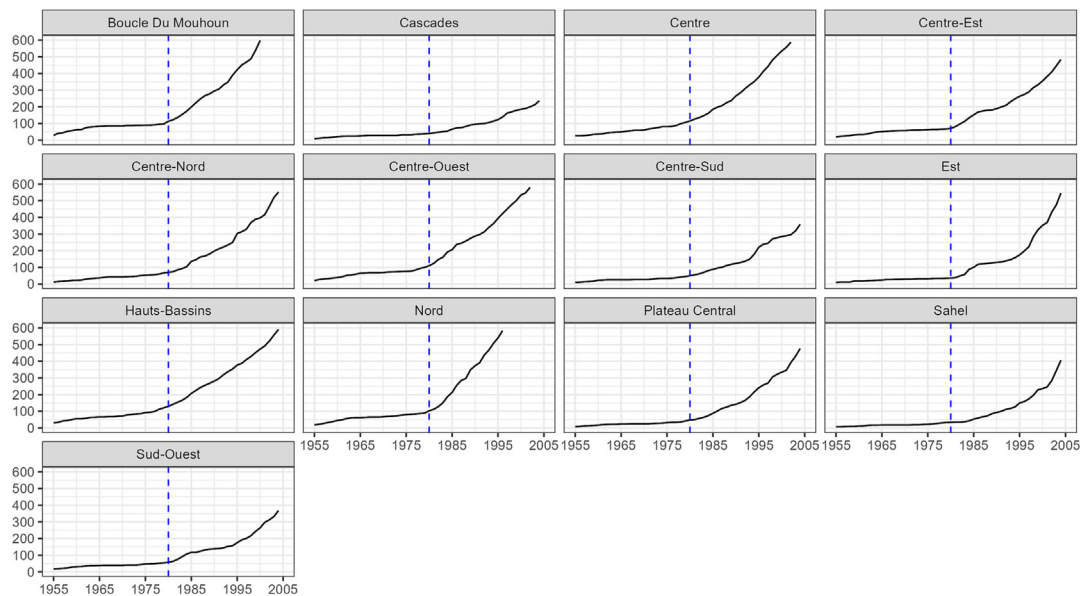


Fig. 2. Cumulative number of schools established in Burkina Faso, by region, 1955–2004) Note: The 194 schools established before 1955 are counted as 1955 in the graph. Source: *Répertoire des écoles publiques et privées du Burkina Faso en 2004–2005* produced by the *Direction des études et de la planification* of the *Ministère de l'enseignement de base et de l'alphabétisation* (MEBA) in March 2006.

4.3. Descriptive statistics

4.3.1. Education

The main explanatory variable of interest is education attainment. In the Afrobarometer, respondents are asked to report their level of education on a 10-point scale: 0 no formal education, 1 some informal education, including Koranic schooling, 2 some primary education, 3 primary completion, 4 some secondary, 5 completed secondary, 6 some post-secondary, 7 some university, 8 graduation from university level, and 9 some post-graduate work. The left panel of Fig. 3 shows the distribution of the original scale.

We change this initial scale in two ways. First, we top-censored the scale because only 1.3 % of the respondents report a level strictly higher than secondary school completion. Secondary school completion alone in rural areas also remained very low (1.9 %) through the time period considered in the paper (here completing secondary stands for the seven years after the primary school). Second, even though the proportion of respondents who report informal education is sizable (12.1 %), we do not retain this distinction in the further analysis. Informal education in this context refers usually to informal sites of Islamic instruction (often called *maktab*, *kuttāb*, or *daar*), where students learn to memorize the Qur'an and write in Arabic. Students in *maktab* also learn the *hadith* and of course life lessons from their imam. Our scale for education level is then a six-point scale: 0 no formal education or some informal education, 1 some primary education, 2 primary completion, 3 some secondary, 4 completed secondary, 5 some post-secondary, some or complete university, and for some post-graduate. The right panel of Fig. 3 shows the distribution of these indicators of attainment.

We also construct three dummy variables: a dummy variable for having some education; a dummy variable for whether the person completed primary school; and a dummy variable for whether the person had some secondary or more.

Table 2 reports descriptive statistics for rural residents born after 1950, pooling the survey rounds. The average values are also calculated by gender. The Afrobarometer sampling strategy sought gender balance in every enumeration area, but there are more females than males in the analysis sample of rural respondents

born before 1950. In terms of schooling attainment in rural areas, about 32 % of the respondents had some schooling, 21 % had completed primary, and 16 % of respondents had some secondary. The median education level was zero, as indicated in Fig. 3, and so the mean education level was quite low, 0.73 (a level of 1 corresponds to some primary education). Women had lower schooling than men, in the sample.

As shown in Fig. 4, the low overall average education levels mask large differences across birth cohorts. In the left panel, the proportion of respondents having done some schooling, by 3-year birth cohorts, is plotted for the 1950–1999 cohorts. Men consistently were about 10–20 percentage points more likely than women to have had some schooling. The right panel gives the average education attainment level, by gender and birth cohort. Again, the discrepancies by gender are visible. For the sake of comparison, education attainment levels and completion rates in urban areas, that are not included in the analysis sample, were considerably higher than in rural areas. In urban areas, for the Afrobarometer sample, overall about 60 % of respondents had completed primary, and 25 % had completed secondary. The proportions of respondents who had completed secondary education went from close to zero for those born during the 1950s to 30 % for those born in the 1990s in urban areas, but only about 5 % for those in rural areas, even when born in the 1990s.

4.3.2. Access to school

The variable *School before age 7* in Table 2 is a dummy variable taking on value one if there was a primary school in the village of residence established before the person was seven years old. Here the schools considered are either public or private primary schools. On average, about 42 % of rural respondents born after 1950 had schools in their villages when they were age seven. The variable *Number of schools, weighted by distance* is the number of primary schools in a radius of 30 km around the village of residence at the time the person was age seven, weighted by dividing by the distance squared (we add 1 km to the distance so as to not divide by 0). This is 1.06 for the entire sample, suggesting that on average over the time period the typical person had reasonable access to school not far from their village (the number varies considerably

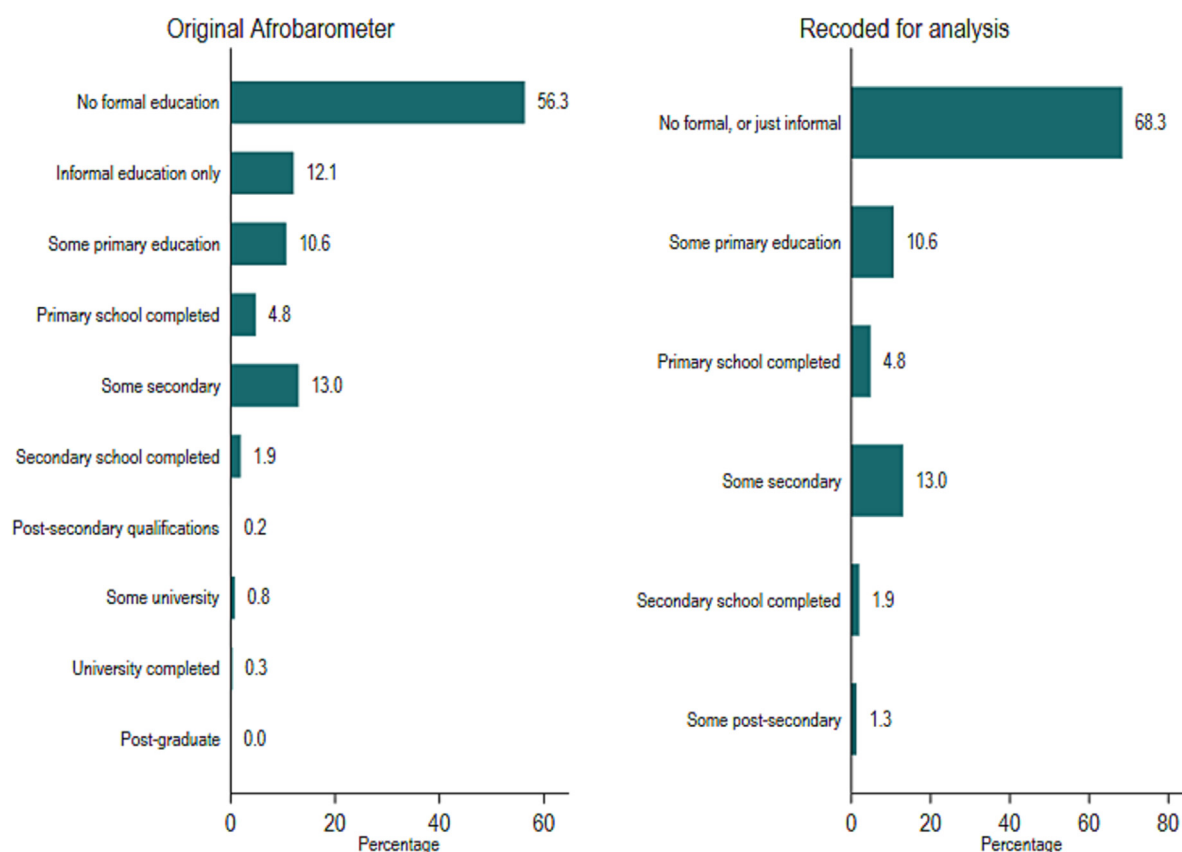


Fig. 3. Distribution of scale of education level in Afrobarometer sample, original and recoded for analysis Note: Afrobarometer data, various rounds. Sample: Rural residents born after 1950.

Table 2
Descriptive statistics of sample, according to gender.

	(1) All	(2) Male	(3) Female	(4) p-value
Year of birth	1977.70 (11.87)	1975.11 (12.16)	1980.20 (11.03)	0.000
School before age 7	0.42 (0.49)	0.39 (0.49)	0.46 (0.49)	0.000
Number of schools, weighted by distance	1.06 (1.86)	0.96 (1.78)	1.16 (1.94)	0.000
Closest school distance (in km.)	5.39 (7.06)	6.18 (7.54)	4.62 (6.49)	0.000
Some schooling	0.32 (0.47)	0.36 (0.48)	0.28 (0.45)	0.000
Completed primary	0.21 (0.41)	0.24 (0.43)	0.18 (0.39)	0.000
Some secondary	0.16 (0.37)	0.18 (0.39)	0.14 (0.35)	0.001
Education level	0.73 (1.25)	0.84 (1.33)	0.63 (1.16)	0.000
Index of engagement	2.04 (1.04)	2.30 (0.92)	1.79 (1.09)	0.000
Voted in recent election?	0.74 (0.44)	0.81 (0.40)	0.68 (0.47)	0.000
Member of community group?	0.44 (0.50)	0.48 (0.50)	0.41 (0.49)	0.000
Participate in community meeting?	0.68 (0.47)	0.77 (0.42)	0.59 (0.49)	0.000
Join with others to raise an issue?	0.61 (0.49)	0.71 (0.45)	0.51 (0.50)	0.000
Observations	4332	2129	2203	

Notes: Afrobarometer Burkina Faso data, various rounds, rural residents, birth year after 1950. Standard deviations in parentheses. Being a member of a community group only observed in rounds 4–7. Final column gives the p-value for *t*-test of difference in means between the two groups.

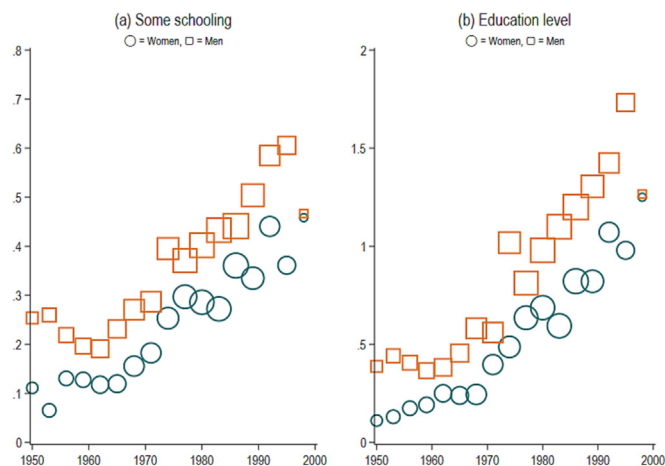


Fig. 4. Rate for having done some schooling, and average education level, by gender and binned birth year cohort, Burkina Faso. Note: Afrobarometer data, various rounds, rural residents, birth year after 1950. Respondents grouped into 3-year bins by birth year, for 1950–1999 birth cohorts. Markers are sized by number of respondents; there are fewer respondents in the early and later birth years.

over time and by village). The average age of males was about five years higher than females. Since women are on average younger in the sample, it is not surprising that the values of these schooling access variables are higher. There are more schools proximate to the village of residence at age seven of women than of men. Likewise, the table reports the distance, in kilometers, to the nearest school. This averages 5.4 km overall, and is higher for men and lower for women, again due to the difference in average age of the two kinds of respondents.

4.3.3. Political engagement

The Afrobarometer survey contains a variety of measures of political engagement. We focus on an index that aggregates three measures of engagement. The measures are: (1) did the person vote in the previous election; (2) had the person participated in a community meeting (scaled from 1 to 4); and (3) whether the person met with other people to raise an issue. The two questions about participating were: *Here is a list of actions that people sometimes take as citizens. For each of these, please tell me whether you, personally, have done any of these things during the past year. If not, would you do this if you had the chance: Got together with others to raise an issue? Attended a community meeting?* The participation questions are recoded to be dummy variables.

The question of whether the respondent voted in an election referred to the last election that took place before each survey round. In round 4 (2008), individuals were asked about the legislative elections of May 2007; in round 5 (2012) about the presidential elections of November 2010; in round 6 (2015) about the legislative and municipal elections of 2012; and in rounds 7 and 8 (2018 and 2019) about the presidential and legislative elections of 2015. The question had a large number of possible responses, ranging from the person being too young to vote, to not having registered to vote, to having decided not to vote, to not finding the polling station, to actually voting. We have recoded the answers to be a simple dummy variable for whether the person voted. If the person indicated in response that they were too young to vote, and the person's age was 21 or younger at the time of the survey, we have recoded that variable as missing, since the question is not pertinent. If they said they were too young to vote but they were older than 21 at the time of the survey, we have recoded them as not voting. About 130 (or 3.7 % of respondents in the analysis sample) were indeed too young to have voted. The index of engagement is the simple sum of the three dummy variables.

In addition, we shall also examine a question about the extent of membership in a community group (scaled from 1 to 4), that was asked in rounds 4–7 but not in round 8. The question about membership in a community group was as follows: *Let's turn to your role in the community. Now I am going to read out a list of groups that people join or attend. For each one, could you tell me whether you are an official leader, an active member, an inactive member, or not a member: Member of voluntary association or community group.* This membership question is also recoded to be a dummy variable that encompasses all the first three degrees of membership.

About 74 % of respondents (who were not too young to vote) in rural areas said they had voted (see Table 2). There is little variation over time. About 74 % of the respondents said they had voted in round 4, 72 % in round 5, 73 % in round 6 and about 75 % in rounds 7 and 8. Other forms of political engagement were less common. Overall, about 68 % said they had participated in a community meeting; 61 % said they had “joined with others to raise an issue”; and 44 % of respondents said they were members of a community group (this question is only available in rounds 4 to 7, not in round 8). Men were much more politically engaged than women, by these measures, for the sample respondents.

4.4. Graphical relationship between education and political engagement behavior

Fig. 5 shows graphically that education level is negatively correlated with the index of engagement, for both women and men, if one does not control for any confounds. The plot displays the mean index of engagement according to the mean education level in each birth year cohort. As can be seen, the different birth cohorts have very different mean levels of education, and also different mean levels of the index of engagement. Women have lower levels of education and lower levels of political engagement compared to men.

There are several possible confounds in Fig. 5 that are important to recall. First, mean education levels for the cohort is closely correlated with the birth year of the cohort. Older generations are on average less educated, as seen earlier in Fig. 4. Different birth cohorts have more than just differences in education affecting their propensity to be civically engaged. Older people may have attained greater civic-mindedness by virtue of their life experiences, on average, and so may be more motivated to be politically engaged. Second, the round of the Afrobarometer survey is not controlled for, and if the voting component of the index is considered, even though the overall turnout levels are rather similar over time, the composition of the voters might differ across different elections. Third, education is endogenous and may be correlated with propensity to be politically engaged, and so any estimate of the relationship may be biased.

An instrumental-variable approach with geographic and birth cohort dummy variables might be more credible in estimating the causal effect of education on political engagement. The remainder of the paper explores the issue of determining whether the substantial variation in political engagement is plausibly attributed to the effects of attending school (and perhaps having been differently acculturated and having different economic and social opportunities), and whether this effect has varied by gender.

5. Estimation strategy

We wish to estimate an average causal effect of educational attainment on the propensity to be politically engaged in Burkina Faso, separately for men and for women. Let Y be the index of political engagement (or alternatively the various components of the index, such as having voted in a recent election, having joined with

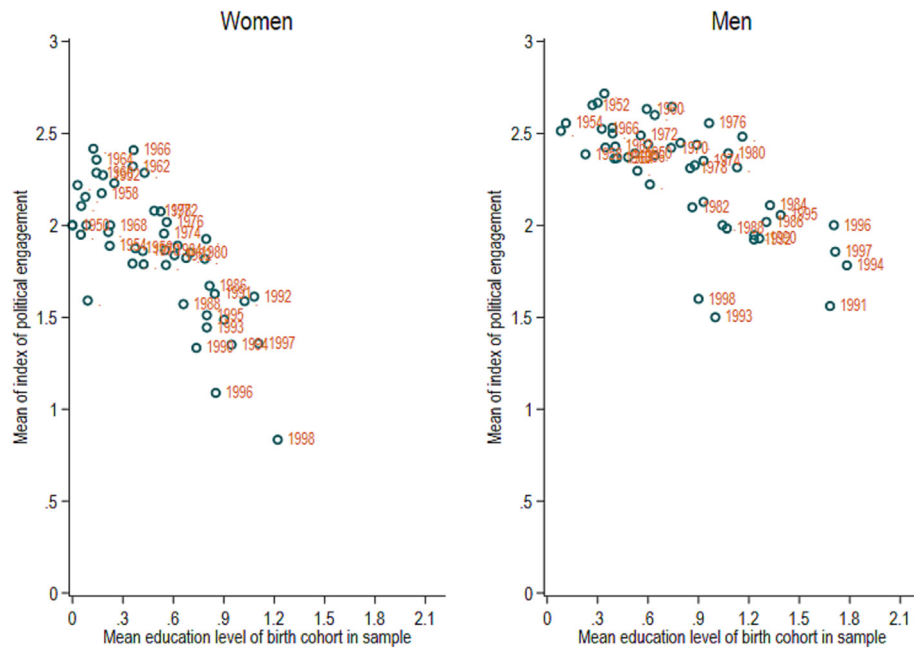


Fig. 5. Mean of index of political engagement by gender, according to mean education level, by birth cohort, Burkina Faso Note: Afrobarometer data, various rounds, rural residents born after 1950. N = 4,263. Respondents grouped by birth year. There are fewer respondents in the early and later birth years. Select markers are labelled by birth cohort.

others to raise an issue, or having joined a community group). Let E be a measure of education attainment. E may be measured in several ways: (1) having been enrolled in primary school; (2) having completed primary school; (3) number of years of education completed, etc.

If education were randomly distributed across individuals, we could estimate the average effect of education on political engagement using the following model:

$$Y_{itv} = \alpha + \beta E_{itv} + \gamma X_{itv} + \varepsilon_{itv} \quad (1)$$

We observe Y , E , and X , for each individual i born in year t living in village v . X includes a set of covariates, such as dummy variables indicating survey rounds. Each survey makes reference to a different election, and this may matter in the individual decision to be politically engaged. We create a binary variable taking value one if the respondent is surveyed after the ouster in 2014 of President Blaise Compaoré and zero otherwise. We estimate the regressions separately for men and for women.

Schooling attainment is evidently not randomly distributed across the population, and likely to be associated with unobserved personal, family, village, and generational characteristics that might also determine political engagement (Baux, 2007; Jabbarian et al., 2022; Kazianga, 2012; Jean-François, 2007b). We therefore adopt an instrumental variable (IV) estimation strategy to correct for likely omitted variable bias (Card, 1995; Card, 1999; Currie and Moretti, 2003; Duflo, 2001).

The main IV, denoted by Z , is a binary variable that takes value one if the respondent lives in a village where there was a school when she was eligible for school (i.e. seven years old), and zero if the respondent lives in a village where there was no school when she was of school-age. We also use as IV the number of primary schools around the village, each school weighted by the inverse of the square of distance from the village. The Afrobarometer data unfortunately does not include place of birth or childhood, so current residence is used as a proxy for location during schooling years. Data on migration status is not collected in the Afrobarometer. A concern about self-selection is reasonable: perhaps individ-

uals or parents who desire high education attainment move to village shortly after a school is established, thus the educated in a village are different for other reasons compared with those in the village just before the school was established. The bias introduced may not be large, however. Burkina Faso historically had very low migration rates compared with other countries. Furthermore, we restrict our sample to the rural respondents, so the estimates may not suffer from the bias that would arise due to rural-urban migration. Below, we check the robustness of the main findings by including commune-level measures of migration patterns.

The IV model, expressed as a two-stage model, is:

$$\begin{cases} E_{itv} = \varphi + \mu Z_{itv} + \pi X_{itv} + v_{itv} \\ Y_{itv} = \alpha + \beta \hat{E}_{itv} + \gamma X_{itv} + \varepsilon_{itv} \end{cases} \quad (2)$$

For the identification approach to be valid, the IV has to be correlated with the level of education and not correlated with the error term of the second-stage equation ε . The first correlation is discussed when presenting the results from the first-stage estimations in Section 6.1. The second requirement is the exclusion restriction: the instrument should have no effect on the outcome other than through the first-stage channel. The exclusion restriction is conditional on all covariates, and does not assume the unconditional orthogonality of the instrument and the outcome. It is therefore important to control for community-level variables to account for possible endogenous placement of the schools and variation across cohorts as education levels increased over time. All of the regressions will control for location dummies, alternatively defined to be at the commune or province levels, and year of birth dummies, either as annual dummies or five-year birth cohort dummies. Ideally the location dummy variables would be at the village level and the birth cohort would be at the annual level, but as noted earlier there are, for most villages, only 8 observations per village, and so few respondents “straddle” the treatment of being of school age before and after the establishment of the school. We return to this issue below. The actual estimation we perform then is given by Equation (3):

$$\begin{cases} E_{itvl} = \varphi + \mu Z_{itvl} + \pi X_{itvl} + \delta_t + \theta_l + v_{itvl} \\ Y_{itvl} = \alpha + \beta \hat{E}_{itvl} + \gamma X_{itvl} + \delta_t + \theta_l + \varepsilon_{itvl} \end{cases} \quad (3)$$

where we observe Y , E , Z , and X , for each individual i born in year t living in village v located in locality l (commune or province). The birth cohort and the locality dummy variables are denoted by δ_t and θ_l respectively. We therefore exploit two sources of variation: birth cohort and location. We observe within-birth cohort variation as people from the same cohort had different access to primary schools as they lived in different locations. We observe within-location variation as people from a given location are from different birth cohorts, and some were too old to benefit from school construction, while others were of school age or were born after the school was built and thus had access to the school.

Hudson, Hull, and Liebersohn (2017) labelled this approach as ‘instrumented difference in differences’ (DDIV). DDIV estimates reflect the impact of education attainment on the final outcomes for the individuals who would not have attained much schooling had access remained difficult in their locality. It is an estimate of the average response of those who are treated. In other words, it consists in a IV strategy where the first-stage equation relies on a difference-indifference style estimator of the effect of the treatment on the endogenous variable. Duflo (2001) is a well-cited example of the DDIV approach. Other examples, as discussed in Section 2, include Croke et al. (2016), Larreguy and Marshall (2017), and Larreguy and Liu (2022). Our work follows closely the approach of André & Maarek (2022), who estimated the effects of schooling by leveraging an original dataset that included the year of establishment of a school in the village of each person in the sample.

Marshall (2016) suggested that researchers should use a continuous measure of education, such as number of years of education or highest education level, rather than a binary variable that would capture a particular level of education (e.g. having completed primary school, or secondary school). The policy reform or the construction of new school induces children to go to school and their enrolment in primary school then conditions their likelihood of pursuing each additional year of education. As a result, one could argue that the construction of primary school has an effect not only on the propensity to be enrolled in primary school but also in higher levels of education. Thus, the exclusion restriction is not satisfied if we restrict the measure of education to having attended or completed primary school, because the construction of the school affects political engagement by other channels than just completing primary (i.e. by continuing on to secondary).

However, a drawback of restricting attention to continuous measures, given the data we have, is that using the education level assumes that any one-unit increase in education induces a rise or decrease in the probability of political participation by β percentage point. For instance, everything else being equal, the difference between those who have no education and those who attended without completing primary school is equal to the difference between those who attended without completing secondary school and those who had completed secondary school. This assumption is rather strong, and we thus consider both the “continuous” education attainment variable and several binary education outcomes.

6. Results

6.1. Effects of school establishment on schooling outcomes

Table 3 presents descriptive statistics (means and standard deviations) of relevant variables where respondents are grouped according to whether they were of age to be eligible for school

(seven years old) before or after a school was established in their village of residence. This variable is the main “treatment” variable in the analysis. Treated respondents are those for whom there was a school, when they were seven years old, in their village of current residence. The table makes clear that treated respondents, both women and men, had substantially larger education outcomes; primary completion rates, for example, were 28 % compared with 10 % for women, and 40 % compared to 14 % for men. Engagement outcomes also differed, again both for women and for men. Those of school age after a school was established scored lower on the engagement index: 1.69 compared with 1.87 for women, and 2.15 compared with 2.39 for men. All of the components of the index were similarly lower, for both men and women (not reported). Those treated were less likely to vote, less likely to participate in a community meeting, and less likely to join with others to raise an issue. Also, those educated were less likely to be a member of a community group. The differences in average outcomes do not, however, account for cohort effects. Respondents born after schools were established are of course on average younger, by about 12 years. Neither do the simple differences in means account for geographic differences and survey round effects.

We turn, then, to multivariable regression analysis. We examine first the magnitude of the relationship at the individual level between education attainment and being “treated.” We have two different measures for being treated. One is there being a school established in the village of current residence when the respondent was of an age eligible for primary schooling, which is seven years old in Burkina Faso. The other is the number of schools around the village when the respondent was age seven, each weighted by the inverse of the square of the distance from the school to the village.

Tables 4 and 5 present in a compact display the regression coefficients of interest for a variety of specifications and for four outcome variables, for men only and then for women only. Standard errors in all specifications are clustered at the village level. The four outcome variables are a binary variable for whether the person had some formal education, a binary for having completed primary school, and a binary variable for having attended at least some years of secondary school, and lastly the scale of schooling attainment, ranging from 0 to 5 as displayed in Fig. 3. All specifications include a dummy variable for whether the respondent is surveyed during one of the post-Compaoré rounds.

We refer to the specifications as: (1) No cohort or locality controls; (2) Birth year cohort and province dummy variables; (3) Birth year cohort and commune dummies; (4) Birth year cohort and village dummy variables. The specifications differ in terms of the geographic variables that control for unobserved heterogeneity. Dummy variables for geographic areas may be at the commune level (for the 209 communes in the data, the median number of respondents is 16, while the minimum is 5 respondents) or at the province level (45 clusters with median size of 99 respondents). We can also use the village level as the geographic locality even though there are a small number of respondents in each cluster (526 rural village clusters, with median number of respondents of 8, maximum of 32, and minimum of 5). Separating by gender means there are typically only 4 respondents in a gender-village cluster, and adding year of birth cohort dummy variables further reduces the possibility of variation, to the point that there are typically zero or one observation per sub-group. (In the online appendix, we report results for a less restrictive specification in which the birth year cohort dummy variables are replaced by five-year birth cohort dummy variables. Results are stable.)

The various approaches find sizable and statistically significant effects of school establishment on schooling outcomes. For the first specification, with neither geographic nor birth cohort dummy variables, the effects are substantial. Including geographic and

Table 3

Descriptive statistics of sample, according to whether school eligible after or before school established, according to gender.

	(1) All	(2) School exists when age 7	(3) No school when age 7	(4) p-value
Panel A: Women				
Some schooling	0.28 (0.45)	0.40 (0.49)	0.17 (0.38)	0.000
Completed primary	0.18 (0.39)	0.28 (0.45)	0.10 (0.30)	0.000
Some secondary	0.14 (0.35)	0.23 (0.42)	0.07 (0.26)	0.000
Education level	0.63 (1.16)	0.95 (1.34)	0.36 (0.90)	0.000
Index of engagement	1.79 (1.09)	1.69 (1.09)	1.87 (1.08)	0.000
Observations	2203	1005	1198	
Panel B: Men				
Some schooling	0.36 (0.48)	0.55 (0.50)	0.24 (0.43)	0.000
Completed primary	0.24 (0.43)	0.40 (0.49)	0.14 (0.35)	0.000
Some secondary	0.18 (0.39)	0.32 (0.47)	0.09 (0.29)	0.000
Education level	0.84 (1.33)	1.40 (1.55)	0.49 (1.02)	0.000
Index of engagement	2.30 (0.92)	2.15 (0.99)	2.39 (0.86)	0.000
Observations	2129	829	1300	

Notes: Afrobarometer Burkina Faso data, various rounds, rural residents, birth year after 1950. Standard deviations in parentheses. Final column gives the p-value for t-test of difference in means between the two groups.

Table 4

Effects of school in village or near village, when individual of school age, on education outcomes, various specifications, men only.

	(1)	(2)	(3)	(4)
Education measure	Some schooling	Completed primary	Some secondary	Education attainment
Panel A: Dummy variable of school in village				
No cohort or locality controls	0.303*** (0.025)	0.248*** (0.024)	0.216*** (0.022)	0.886*** (0.079)
Birth year, province	0.249*** (0.027)	0.192*** (0.024)	0.152*** (0.023)	0.702*** (0.080)
Birth year, commune	0.201*** (0.028)	0.164*** (0.025)	0.133*** (0.024)	0.595*** (0.081)
Birth year, village	0.119*** (0.045)	0.103** (0.041)	0.074* (0.038)	0.384*** (0.128)
Panel B: Number of schools weighted by proximity				
No cohort or locality controls	0.083*** (0.008)	0.075*** (0.006)	0.069*** (0.006)	0.263*** (0.024)
Birth year, province	0.071*** (0.008)	0.061*** (0.006)	0.052*** (0.006)	0.215*** (0.024)
Birth year, commune	0.068*** (0.009)	0.057*** (0.009)	0.052*** (0.009)	0.206*** (0.032)
Birth year, village	0.032*** (0.011)	0.019* (0.010)	0.025** (0.010)	0.077*** (0.030)

Notes: Each coefficient and standard error in the table is for a separate regression, according to the specification (row) and outcome measure (column) and measure of schooling access (panel). For Panel A the coefficient is that of the dummy variable indicating if a school was present, for Panel B the coefficient is on the variable representing the count of schools within 30km, each weighted by the inverse distance squared, where distance is the measure in kilometers of the distance from the village of residence to the village of the school. Standard errors, clustered at the village level, are in parentheses. All specifications include dummy variable for post-2014 survey rounds (after popular uprising leading to democratic transition), and birth cohort and geographic dummy variables as indicated, and are estimated with Stata command `reghdfe`. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data is from Afrobarometer survey, Burkina Faso, rural residents born after 1950, various rounds, men only, and administrative data on year of school establishment, and excluding those with missing response to components of index of political engagement. $N = 2100$.

birth cohort dummy variables reduces the magnitudes of the estimated coefficients, suggesting that places and birth cohorts that had relatively more respondents attain higher levels of schooling after a school was established were also more likely to be places where respondents of school age before a school was established attained high schooling (perhaps by going to school in neighboring villages, or because these villages attracted educated people to settle in the villages). The coefficients in column 1 of Panel A, are similar in magnitude to the coefficients estimated for Mali in [André and Maarek \(2022\)](#), who found that having a primary school

increased the probability of school enrolment by about 0.22 (that is by 22 percentage points) among their sample of individuals aged 40 years or more.

Even when controlling for year of birth and village, the effects are statistically significant and sizable, for men. In [Table 4](#) it can be seen that having a school in the village when a young boy is seven years old is associated with an increase in the probability of having some education of 0.12 (that is, by 12 percentage points), increasing the probability of completing primary school by 0.10 (10 percentage points), and increasing the

Table 5

Effects of school in village or near village, when individual of school age, on education outcomes, various specifications, women only.

Education measure	(1)	(2)	(3)	(4)
	Some schooling	Completed primary	Some secondary	Education attainment
Panel A: Dummy variable of school in village				
No cohort or locality controls	0.236*** (0.022)	0.181*** (0.020)	0.156*** (0.019)	0.602*** (0.063)
Birth year, province	0.195*** (0.022)	0.149*** (0.020)	0.123*** (0.018)	0.486*** (0.060)
Birth year, commune	0.149*** (0.026)	0.099*** (0.023)	0.073*** (0.019)	0.328*** (0.066)
Birth year, village	0.032 (0.041)	−0.000 (0.034)	−0.005 (0.031)	0.011 (0.100)
Panel B: Number of schools weighted by proximity				
No cohort or locality controls	0.063*** (0.008)	0.053*** (0.008)	0.048*** (0.008)	0.176*** (0.026)
Birth year, province	0.054*** (0.008)	0.046*** (0.007)	0.041*** (0.007)	0.151*** (0.023)
Birth year, commune	0.047*** (0.009)	0.043*** (0.007)	0.036*** (0.008)	0.137*** (0.024)
Birth year, village	0.019** (0.009)	0.019* (0.011)	0.025** (0.013)	0.068** (0.033)

Notes: Each coefficient and standard error in the table is for a separate regression, according to the specification (row) and outcome measure (column) and measure of schooling access (panel). For Panel A the coefficient is that of the dummy variable indicating if a school was present, for Panel B the coefficient is on the variable representing the count of schools within 30km, each weighted by the inverse distance squared, where distance is the measure in kilometers of the distance from the village of residence to the village of the school. Standard errors, clustered at the village level, are in parentheses. All specifications include dummy variable for post-2014 survey rounds (after popular uprising leading to democratic transition), and birth cohort and geographic dummy variables as indicated, and are estimated with Stata command `reghdfe`. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data is from Afrobarometer survey, Burkina Faso, rural residents born after 1950, various rounds, women only, and administrative data on year of school establishment, and excluding those with missing response to components of index of political engagement. $N = 2162$.

measure of attainment by 0.38. This is with the most “demanding” specification, with village dummy variables and birth year cohort dummy variables. The effect on completing secondary is not significant in that specification, but is substantial in the other specifications.

In Panel B the estimated magnitudes of the effects are similar. A one unit increase in the inverse-distance weighted number of schools in proximity is associated with a 3–8 percentage point increase in the probability of having some education (depending on the specification), and a similar sized increase in the probability of completing primary school.

The effects are smaller for women, as can be seen in Table 5. The birth year and village dummies specifications do not yield statistically significant coefficients for the female sample. For the birth year cohort and commune locality specification, having a school in the village when a young girl is seven years old is associated with an increase in the probability of having some education of 0.15 (that is, by 15 percentage points), increasing the probability of completing primary school by 0.10 (10 percentage points), and increasing the measure of attainment by 0.33. The effect on having some secondary is 0.07 (about half the size of the effect for men). In Panel B for women the estimated magnitudes of the effects are also smaller than for men. A one unit increase in the inverse-distance weighted number of schools in proximity is associated with about a 5-percentage point increase in the probability of attaining some education, and a similar increase in the probability of completing primary school, in the birth year cohort and commune dummies specification.

The effects of school access being smaller for women compared to those for men is unsurprising. Even if access to school reduces the cost of schooling, there is still a school enrolment cost for the families, that might favor their boys in a context of resource constraints and in the absence of compulsory school enrolment. In Nigeria, Larreguy and Marshall (2017) find that the first-stage effect of the universal primary education reform on education level is 40 % lower for women. This need not always be the case. Djemai, Samson, and Renard (2022) found that the differential effect of the

Zimbabwe 1980 reform by gender was the opposite: greater for women than for men mostly because primary school became compulsory and the level of education was greater for men than for women before the reform.

This first-stage is crucial for the credibility of second-stage estimates of the effect of schooling levels of individuals on their political engagement. We report in Online Appendix B, Tables B1 and B2, the F-statistics for excluded instruments for the various specifications. We see that the F-statistics are generally quite large, and many above the various rules of thumb proposed in the past, except for the case of the birth year and village dummies specification, where the F-statistic is quite small. This last specification clearly exhibits a “weak instruments” problem, and so we do not use it in estimating the second stage.

In Online Appendix B we also report event study graphs that provide one visual check for the parallel trends assumption that is used to gauge the validity of difference-in-differences estimation. The event study graphs plot the coefficients with specifications that include ten leads and ten lags, with the furthest lead and lag accumulating the school establishment dummy prior to or after the lag or lead. As can be seen, coefficients on lagged indicators of school establishment are not significant. Those respondents in villages who were school age eligible well before a school was established were not seeing rising education levels, relative to those of similar age in villages where a school would be established even later, or not at all. Coefficients on schooling outcomes for those who were around seven years old or younger at the time of schooling establishment are positive and with confidence intervals above zero. The effect seems to diminish, though, as those who are school-age-eligible more than five years prior to when a school was established do not see as large or as statistically significant effects.

There are many other variable coding and specification variations possible for these first stage regressions. For example, we could have used locality levels of commune, province and village, but aggregated the birth cohort by using 5-year birth cohort dummy variables. Results are highly consistent with the results

presented here (Online Appendix C). In addition, some specifications could exclude the partially treated, defined as those who were 8, 9 or 10 years old when the school was established in the village. This group could have benefited from the newly established school if they started school later in age or if they switched from being enrolled in a school that was further away from their village to the newly established school. For the latter, the cost of being enrolled is reduced and continued enrollment in primary school is likely to be exogenously increased as a result. Alternatively, one could assign partial schooling access to those older than 7 when schools were established in the village or in the vicinity and include that variable separately. First-stage regression results when the partially treated are excluded from the sample are reported in [Online Appendix Table D1](#).

Being of school age after a school was established could be interacted with a dummy variable for whether the person was of school age after 1980 (when government promotion of schooling accelerated). When the locality cluster is the commune or province, time-varying or fixed covariates at lower geographic levels could be included to control for some potential confounds (e.g., distance to commune village seat, usually the commercially important village of the area with more public infrastructure; village population over time). Characteristics of schools (public, private, how many classrooms) might also be controlled for. In general, the “first stage” results yield similar estimates of the effects of schooling access on education outcomes in other specifications and variable coding choices (results not reported).

For further validation of the first stage, we include in Online Appendix E the results of using the Probit specification for the three first-stage regressions that have binary outcome variables. The marginal effects are basically all statistically significant and the same magnitudes as in the linear probability model. We also discuss the two-way fixed effects (TWFE) specification and various recent modifications to that approach, in the context of the structure of the Afrobarometer sample. Similarly, we explain in the appendix why a regression discontinuity approach does not work well with the Afrobarometer data in our case.

We turn, then, to estimates of the effects of education attainment on political engagement.

6.2. Effects of schooling outcomes on political engagement

The expansion of primary school infrastructure appears to have significantly increased the education attainment and primary completion rates of residents of rural areas. In this section, we explore the effects of education on the index of political engagement. We present the results using the specifications with various combinations of location dummies along with birth year cohort. We do not consider in the rest of the paper the specifications that control for birth year and village dummies as the instrumental variables (school in village or number of schools near village) exhibit low explanatory power in the first stage.

Before turning to the IV estimates, we note that in [Online Appendix F, Table F1](#) we present the results from the naive estimation of the correlations between the index of political engagement and the education measures (the coefficients in OLS equation with only the control for post-2014 survey round). The table also includes the coefficients from the simple OLS for each component of the index, and for the indicator of being a member of a community group (rounds 4–7 only). In almost all cases, the relationship with education is negative, and is mostly significant for women. [Table F2](#) presents the reduced-form estimates with the binary treatment variable for having a school in the village when school aged in Panel A, and the weighted number of schools in Panel B. Column 1 reports the estimated coefficients for the whole sample, and Columns 2 and 3 for men and women respectively. Access to

primary schools is negatively related with the index of political engagement, and significantly so, in the specification with no cohort or locality controls. When controlling for cohort and locality dummies, the negative effect persists more often for men. We do not rely on the reduced-form estimates because we cannot reasonably assume perfect compliance. Among the treated, 46 % have some education: 53 % among the male sample and only 39 % among the female sample. As already mentioned earlier, as primary school is not compulsory, when a school is built in a village, many children are still out of school.

[Table 6](#) presents the IV estimates with first stage according to the various specifications of [Table 4](#) and with outcome variable the index of political engagement, for men only. Panel A shows results for the instrumental variable of whether there was a school established in the village of residence prior to the respondent being school age eligible. Panel B shows results for the instrumental variable indicating the number of schools in proximity to the village of residence, when the respondent was school age eligible. Each panel reports the coefficients of interest for Equation (3), estimated with a different education measure (as indicated in the columns). The specifications vary in the controls for birth cohort and locality. A dummy variable for the later post-2014 survey rounds is also included in all specifications.

In general, the results are consistent across the different measures of education and specifications. For men, the effect of education is negative and often statistically significant with the birth and locality controls. That is, the results suggest that education, instrumented by access to school, was associated with lower levels of political engagement. The magnitudes are fairly large: in the specifications without birth and locality controls, the effects of having some education, completing primary, or completing secondary are all associated with reductions in the index of engagement of about one unit; while once birth cohort and commune dummies are controlled for the effects of the education dummy variables falls to about 0.5 units. The engagement index is on a three-point scale, so an effect of 0.5 units is substantial. Note that because the estimated coefficients are lower in the within-province model in Panel A, compared to the within-commune model, they lose their significance, even though the standard errors remain fairly small. For specifications that use the weighted number of schools in the proximity as IV, the education effects remain negative, and are generally statistically significant (to varying degrees) when using the birth cohort and geographic control variables.

For women, as reported in [Table 7](#), for specifications with the dummy for school access as the IV, the effects are statistically significant when not using any geographic controls and birth cohort controls. In that specification, the effects are negative and basically the same magnitude as for men. For the specifications with birth cohort and locality controls, however, results are generally not statistically significant. Point estimates are generally considerably smaller in absolute value than those for men, and are sometimes positive. For specifications that use the IV of weighted number of schools, the estimated coefficients are significant and roughly the same magnitude as for men, for the birth year and province specification, but not significant (and smaller in magnitude) for the commune locality specification.

We implement a number of additional tests to check the robustness of our second-stage results. We first control for five-year cohort dummy variables instead of the birth year dummy variables in [Online Appendix Table C3](#). Second, we drop the partially treated respondents from the estimation sample in [Online Appendix Table D2](#). In both robustness exercises, results are similar: the effects are more negative for men than for women. In the within-province specification, coefficients turn out to be significant for men for both instrumental variables when partially treated respondents are excluded from the sample.

Table 6

Effects of education on index of political engagement, with various measures of education outcomes instrumented with indicators of access to schooling, for men only.

	(1)	(2)	(3)	(4)
Education measure	Some schooling	Completed primary	Some secondary	Education attainment
Panel A: IV dummy variable of school in village				
No cohort or locality controls	−0.763*** (0.160)	−0.938*** (0.200)	−1.070*** (0.239)	−0.262*** (0.056)
Birth year, province	−0.292 (0.197)	−0.385 (0.254)	−0.476 (0.328)	−0.106 (0.070)
Birth year, commune	−0.454* (0.269)	−0.563* (0.331)	−0.685 (0.419)	−0.155* (0.091)
Panel B: IV number of schools weighted by proximity				
No cohort or locality controls	−0.959*** (0.198)	−1.069*** (0.255)	−1.149*** (0.279)	−0.303*** (0.074)
Birth year, province	−0.505** (0.229)	−0.588** (0.274)	−0.691** (0.333)	−0.166** (0.078)
Birth year, commune	−0.552* (0.291)	−0.655* (0.358)	−0.723* (0.411)	−0.183* (0.099)

Notes: Dependent variable is index of political engagement, the sum of three dummy variables (whether voted in the prior election, whether participated in a meeting, and whether joined with others to raise an issue). Data on the outcome is from the Afrobarometer Burkina Faso data, rural residents born after 1950, various rounds, men only. Coefficients of the education measure are reported and their standard errors, clustered at the village level, are in parentheses. All specifications include a dummy variable for two post-2014 survey rounds (after popular uprising leading to democratic transition), and birth cohort and geographic dummy variables as indicated, and are estimated with Stata command ivreghdfe. N = 2080. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 7

Effects of education on index of political engagement, with various measures of education outcomes instrumented with indicators of access to schooling, women only.

	(1)	(2)	(3)	(4)
Education measure	Some schooling	Completed primary	Some secondary	Education attainment
Panel A: IV dummy variable of school in village				
No cohort or locality controls	−0.706*** (0.211)	−0.921*** (0.278)	−1.069*** (0.320)	−0.277*** (0.083)
Birth year, province	−0.031 (0.282)	−0.040 (0.369)	−0.049 (0.449)	−0.012 (0.113)
Birth year, commune	0.155 (0.405)	0.234 (0.614)	0.318 (0.839)	0.071 (0.185)
Panel B: IV number of schools weighted by proximity				
No cohort or locality controls	−1.099*** (0.182)	−1.303*** (0.243)	−1.428*** (0.288)	−0.393*** (0.072)
Birth year, province	−0.646** (0.280)	−0.753** (0.336)	−0.840** (0.372)	−0.229** (0.102)
Birth year, commune	−0.402 (0.411)	−0.443 (0.454)	−0.518 (0.515)	−0.138 (0.140)

Notes: Dependent variable is index of political engagement, the sum of three dummy variables (whether voted in the prior election, whether participated in a meeting, and whether joined with others to raise an issue). Data on the outcome is from the Afrobarometer Burkina Faso data, rural residents born after 1950, various rounds, women only. Coefficients of the education measure are reported and their standard errors, clustered at the village level, are in parentheses. All specifications include a dummy variable for two post-2014 survey rounds (after popular uprising leading to democratic transition), and birth cohort and geographic dummy variables as indicated, and are estimated with Stata command ivreghdfe. N = 2127. * p < 0.10, ** p < 0.05, *** p < 0.01.

A threat to validity of the IV estimates, as mentioned earlier, comes from migration. Migration might result in measurement error. If most people have migrated between childhood and survey year, the treatment variable is poorly measured. Currently living in a village with or without a school established before age seven may or may not be correlated with whether you were living in a village with or without a school when you were a child. This might be true especially for rural women, who often move from one village to a neighboring village upon marriage. The second treatment variable that captures the weighted average of the distance to schools is less affected by this issue. When accounting for all the schools located within 30 km of a location, it is very likely that the schools near the native village of the married women are accounted for if we assume that men marry women whose villages are not too far away.

A second migration issue is that families with a propensity to be politically engaged might migrate to villages that have established schools. We address the potential bias due to a selection effect of migration by following De Luca and Verpoorten (2015). In their

analysis of civil conflict and political participation in Uganda, also using Afrobarometer data, they checked whether selective migration was driving their results by controlling for district out-migration due to insecurity (using migration data from a different survey). We similarly modify the specifications of the main analysis by adding migration rates at the commune level in two of our three main specifications: with no locality or birth cohort controls, and with province and birth cohort controls (we cannot estimate the coefficients of the commune dummy variables as the migration rates are defined at the commune level). We use three different data sources to measure migration rates. The first source is the 2003 Demographic and Health Survey from which the proportions of respondents who have migrated at adulthood is computed in each sampled cluster. The second source is the 1996 census, which reported for each village the number of people born in the village and those not born in the village. The third source is the electoral registry of 2012, publicly available from the national electoral commission, which listed for every registered voter whether they were born in the village in which they were registered.

Adding these measures of migration, at the commune level, might generate different estimated coefficients if migration rates were somewhat time invariant and were strongly associated with education and political engagement. Instead, results are robust (see Online Appendix Tables G1 - G8). The first-stage results are robust to this augmented version of the equation. We find that the estimated coefficients of the effect of education on engagement are similar with or without controls for migration rates. For both men and women, negative and significant coefficients are found in regressions with no cohort or locality controls. Once birth year and province dummies are included, the effects remain non-significant for women for any IV, while for men, the effects are negative and significant when using the weighted number of schools as instrument and lose their significance when using the school access dummy as IV (as in the models without commune-level migration rates).

6.3. Effects of school outcomes on the components of index of engagement

We also estimate the causal effect of schooling on each of the underlying four indicators of political engagement, and present the results in Table 8 for the male sample and in Table 9 for the female sample. Both tables have as outcome variables whether the person voted in the previous election (Panel A), met with other people to raise an issue (Panel B), was a member of a community group (Panel C, for rounds 4–7 only) and participated in a community meeting (Panel D). The original survey questions on membership, participation, and joining with others had four responses, here they have been recoded to be binary variables. Each panel reports the coefficients of interest for Equation (3), estimated with

a different education measure (as indicated in the columns). The specifications vary in the controls for birth cohort and locality. A dummy variable for the post-Compaoré survey rounds is also included in all specifications. Both tables report the estimated coefficients when the IV is whether there was a school established in the village of residence prior to the respondent being school age eligible.

In general, the results are consistent across the four outcome variables and different measures of education. For all four outcomes, the specification without birth cohort and locality dummy variables yields negative and usually statistically significant coefficients of the education variable. That is, the estimations suggest that education, instrumented by access to school, is associated with lower levels of political engagement. These results hold for both men and women. For instance, having been enrolled in school decreases the probability of voting by 22 percentage points for men (column 1, Panel A of Table 8) and, very similarly, by 12 percentage points for women (column 1, Panel A of Table 9).

Once cohort and locality controls are introduced, however, the estimated coefficients are never statistically significant for women. For men, some of the individual components have statistically significant negative coefficients. For two of the outcomes (membership in a community group and participation in community meetings), the coefficients remain negative and significant when using geographic controls (province or commune) and year of birth cohort controls. But for the other two specifications and outcomes (voting behavior and joining with others to raise an issue), the instrumented effect of education is not significant. These results are roughly similar across all four measures of education.

These estimated effects of education on the components of political engagement are reasonably similar with the use of the

Table 8

Effects of education on the four measures of political engagement, with various measures of education, and with presence of school in the village when school eligible as instrumental variable, men sample.

	(1)	(2)	(3)	(4)
Education measure	Some schooling	Completed primary	Some secondary	Education attainment
Panel A: Voted in the previous election				
No cohort or locality controls	−0.216*** (0.063)	−0.267*** (0.077)	−0.304*** (0.091)	−0.075*** (0.022)
Birth year, province	0.111 (0.080)	0.139 (0.105)	0.181 (0.132)	0.038 (0.029)
Birth year, commune	0.108 (0.117)	0.130 (0.144)	0.164 (0.178)	0.036 (0.040)
Panel B: Joined with others to raise an issue				
No cohort or locality controls	−0.223*** (0.077)	−0.272*** (0.096)	−0.313*** (0.112)	−0.076*** (0.027)
Birth year, province	−0.152 (0.101)	−0.196 (0.132)	−0.248 (0.169)	−0.054 (0.036)
Birth year, commune	−0.180 (0.134)	−0.220 (0.167)	−0.271 (0.209)	−0.061 (0.046)
Panel C: Member of a community group				
No cohort or locality controls	−0.343*** (0.094)	−0.427*** (0.121)	−0.498*** (0.141)	−0.121*** (0.033)
Birth year, province	−0.298** (0.127)	−0.407** (0.178)	−0.536** (0.241)	−0.112** (0.048)
Birth year, commune	−0.397** (0.177)	−0.541** (0.256)	−0.705** (0.347)	−0.148** (0.068)
Panel D: Participated in a meeting				
No cohort or locality controls	−0.323*** (0.070)	−0.398*** (0.088)	−0.453*** (0.104)	−0.111*** (0.024)
Birth year, province	−0.250*** (0.093)	−0.328*** (0.122)	−0.408** (0.160)	−0.090*** (0.033)
Birth year, commune	−0.383*** (0.134)	−0.473*** (0.165)	−0.577*** (0.215)	−0.130*** (0.045)

Notes: Afrobarometer Burkina Faso data, rural residents born after 1950, five rounds (except in Panel C, rounds 4–7), men only. Coefficients of the education measure are reported and their standard errors, clustered at the village level, are in parentheses. All specifications include dummy variable for two post-2014 survey rounds (after popular uprising leading to democratic transition), and birth cohort and geographic dummy variables as indicated, and are estimated with Stata command ivreghdfe. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9

Effects of education on the four measures of political engagement, with various measures of education, and with presence of school in the village when school eligible as instrumental variable, women sample.

	(1)	(2)	(3)	(4)
Education measure	Some schooling	Completed primary	Some secondary	Education attainment
Panel A: Voted in the previous election				
No cohort or locality controls	−0.122 (0.088)	−0.159 (0.115)	−0.185 (0.133)	−0.048 (0.034)
Birth year, province	0.165 (0.115)	0.215 (0.152)	0.262 (0.186)	0.066 (0.047)
Birth year, commune	0.163 (0.171)	0.246 (0.263)	0.334 (0.361)	0.074 (0.079)
Panel B: Joined with others to raise an issue				
No cohort or locality controls	−0.266*** (0.095)	−0.347*** (0.125)	−0.403*** (0.144)	−0.105*** (0.037)
Birth year, province	−0.051 (0.130)	−0.066 (0.170)	−0.081 (0.206)	−0.020 (0.052)
Birth year, commune	0.015 (0.189)	0.022 (0.285)	0.030 (0.387)	0.007 (0.086)
Panel C: Member of a community group				
No cohort or locality controls	−0.253** (0.116)	−0.337** (0.154)	−0.374** (0.170)	−0.100** (0.046)
Birth year, province	−0.087 (0.159)	−0.115 (0.211)	−0.134 (0.244)	−0.035 (0.064)
Birth year, commune	−0.077 (0.243)	−0.104 (0.328)	−0.132 (0.414)	−0.032 (0.102)
Panel D: Participated in a meeting				
No cohort or locality controls	−0.317*** (0.097)	−0.414*** (0.127)	−0.481*** (0.146)	−0.125*** (0.038)
Birth year, province	−0.145 (0.126)	−0.189 (0.164)	−0.230 (0.198)	−0.058 (0.050)
Birth year, commune	−0.022 (0.184)	−0.034 (0.278)	−0.046 (0.378)	−0.010 (0.084)

Notes: Afrobarometer Burkina Faso data, rural residents born after 1950, five rounds (except in Panel C, rounds 4–7), women only. Coefficients of the education measure are reported and their standard errors, clustered at the village level, are in parentheses. All specifications include dummy variable for two post-2014 survey rounds (after popular uprising leading to democratic transition), and birth cohort and geographic dummy variables as indicated, and are estimated with Stata command `ivreghdfe`. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

weighted number of schools as the IV, as reported in [Online Appendix Tables H1 and H2](#). The only difference is that for women, coefficients in the within-province models are now significant in three components out of four.

7. Geographic and birth cohort dummies, and power considerations

Overall, we find that the causal effect of education on political engagement is likely negative for men, and zero for women. For men, the effect is negative for the index of engagement and for two of the four separate measures of political engagement. For women the results appear to be zero effects on all of the measures of engagement, once controls are included. The results suggest that for Burkina Faso there is little reason to think there is a sizable positive relationship between education levels of a person and their likelihood of being politically engaged (as measured in the Afrobarometer surveys). The “null” results for women could be due to misspecification and to lack of statistical power to detect a sizable effect, so we turn to those issues.

As noted earlier, estimation results with a specification including birth year cohorts and geographic dummy variables at the village level are not reported here, because the great majority of villages only have 8 observations. With dummy variable for birth cohort, and split by gender, most observation cells are empty. The estimated coefficients in such specifications are close to zero and never statistically significant. If there were 80 observations in each village, presumably the only specification that mattered would be the one with village level dummy variables. Other empirical analyses using the Afrobarometer data recognize this problem

of small samples in each locality and the trade-offs faced in statistical analysis (Briggs, 2019; Cha, 2020; Choi et al., 2021; Knutsen, Kotsadam, Olsen, and Wig, 2017; Knutsen and Kotsadam, 2020; Konte and Vincent, 2021; Watkins, 2022).

This raises the general issue of statistical power. The standard definition of power is one minus the probability of Type II error, where Type II error is when the “true” β in Equation (3) is different from zero but we fail to reject the null hypothesis that the effect is zero (Campbell and Gustafson, 2018; Parinduri, 2019; Walker et al., 2017). We may evaluate the power of the estimation approach using the R package `ivmodel`, developed by Kang et al. (2021). The package implements the power formula developed by Freeman, Cowling, and Schooling (2013). The formula appears to be appropriate to our setting. Kang et al. (2021) use as their example data the analysis of Card (1995), who estimated the income returns to schooling with an instrumental variable measuring distance to a college; the sample size he used, about 3,000 observations, was similar to that used here.

Credible power calculations require reasonable effect sizes. Table 1 suggests that the magnitudes of other studies estimating the effects of education on political participation are close to those estimated here. Coefficients of about 0.30 in absolute value for binary schooling variables, and 0.15 for unit increases of attainment variables, are in the range of estimated effects (mostly with smaller samples). As we have seen in Table 6 with the index of political engagement as outcome, the estimated coefficient for men is about −0.50 for the two binary indicators of some schooling and completed primary and about −0.15 for the measure of education attainment. In Table 8, with the components estimated separately, the estimated coefficients are about −0.30 for the schooling binary variables, and about −0.10 for the measure of education attainment.

Figs. 6 and 7 show that for three education indicators (some schooling, completed primary, and education level), and with whether there was a school in the village at the time the person was eligible for school as the instrumental variable, the estimation appears to be adequately powered for relevant effect sizes for the sub-sample of rural men surveyed in the Afrobarometer, but not for the sub-sample of women. The different effect sizes (here not standardized, but rather the magnitude of the coefficient) are on the x-axes, and the power is indicated on the y-axis.

In Fig. 6 when education is measured by having some schooling, we exceed 80 % power if the size of the effect on the political engagement index is in the -0.7 to -0.4 range, in specifications with no geographic or birth year controls. We have about 80 % power for -0.7 to -0.5 range range with province dummies and birth year cohorts, and with commune dummies and five-year birth cohorts we are below 80 % power for effect sizes below

-0.7 . When the education measure is whether the person completed primary, power declines, since there is less variation in the sample in this indicator. But power remains above 80 % for relevant coefficient magnitudes in specifications with no geographic or birth year controls. Specifications with commune dummies and five-year birth cohorts are under-powered for coefficient effects below -0.7 . When the interval variable for education attainment is used as the education indicator, the effect is for a one unit increase in the indicator, and the estimated coefficient range as seen in Table 6 was between -0.5 and -0.1 . Power appears to be adequate for coefficient magnitudes of -0.3 or greater, since there is considerably more variation in this indicator of education. Overall, these specifications appear to have reasonable power to detect economically and politically relevant effects.

The picture in Fig. 7 is quite different when we calculate power using the female sample. Despite the sample being slightly

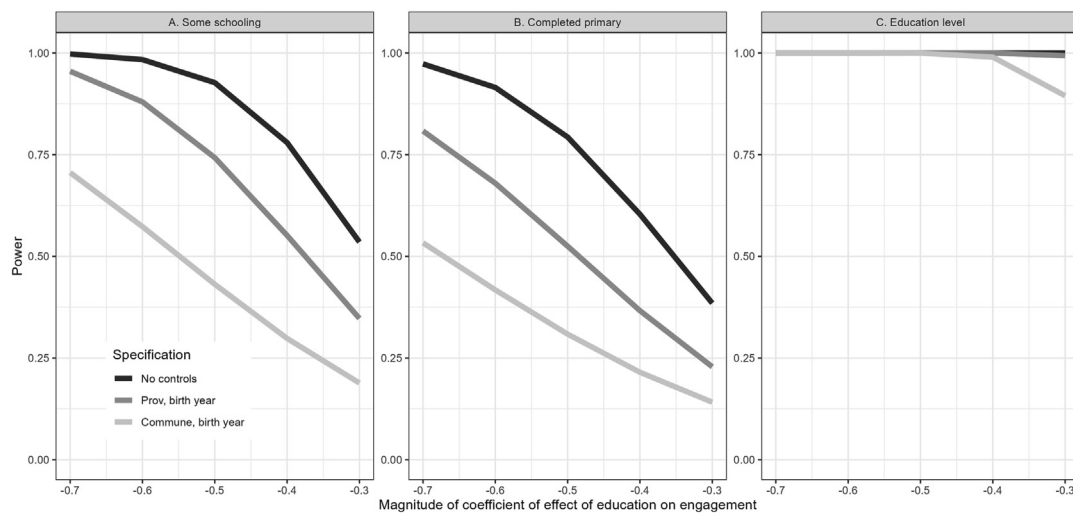


Fig. 6. Power calculations for different education variables (endogenous) using schooling access as instrument and outcome variable the index of political engagement, male sample Notes: Power calculated under various effect sizes (magnitudes of coefficients) using R package ivmodel developed by Kang et al. (2021), using Afrobarometer data, various rounds, rural residents born after 1950, men only (N = 2,080).

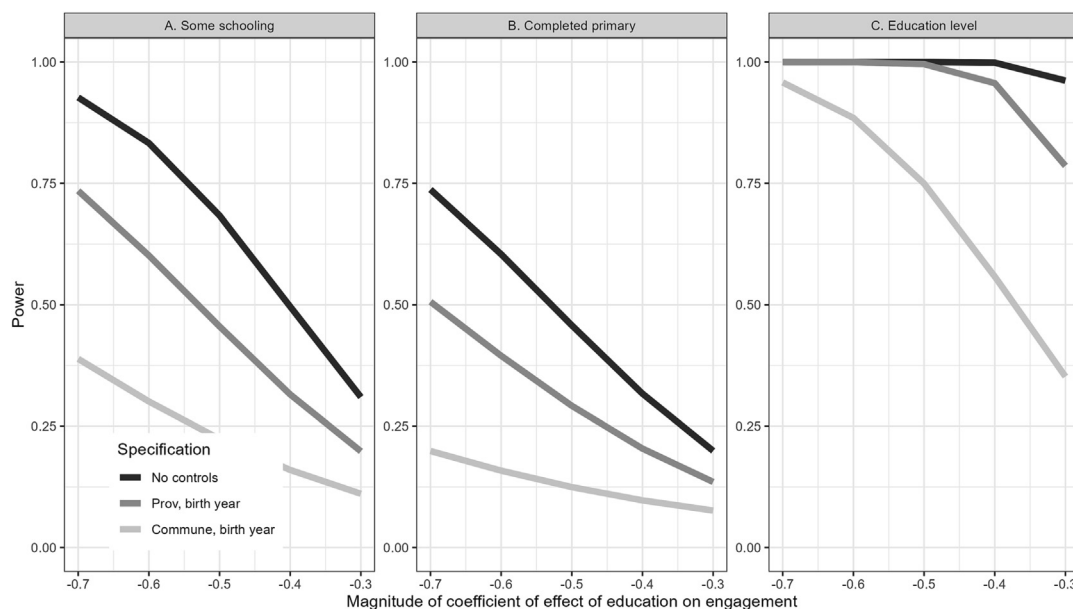


Fig. 7. Power calculations for different education variables (endogenous) using schooling access as instrument and outcome variable the index of political engagement, female sample Notes: Power calculated under various effect sizes (magnitudes of coefficients) using R package developed by Kang et al. (2021), using Afrobarometer data, various rounds, rural residents born after 1950, women only (N = 2,127).

larger (2,127 observations as opposed to 2,080 for men), power is lower for the binary education variables. Power is only adequate for the very large effect sizes for some of the specifications (i.e., the specification with no controls). When the interval variable for education attainment is used as the education indicator, power appears to be adequate for coefficient magnitudes of -0.3 or greater, in the specification with province and birth year cohort dummy variables. But for the others, there simply is not enough variation in education outcomes, within birth and geographic units, to estimate the coefficient of interest with much precision.

Online Appendix I contains similar power analysis for the regressions with the separate components of the engagement index, for men (the results for women are quite similar). The analysis suggests that specifications with the education level variable have considerably more power than those with the dummy variables for schooling attainment, that specifications with no geographic or birth cohort controls are adequately powered, while specifications with geographic and birth cohort controls are only powered for larger effects.

The power to estimate a given effect size (i.e., the magnitude of the coefficient of how education affects political participation) depends on a number of factors. First, the sample size. Second, the extent of variation in the explanatory variable. Third, complex covariate correlations. So, the female sample may have less power than the male sample even though it is larger if the extent of variation in the explanatory variable is smaller. And this is indeed the case in most countries. Quite a bit fewer women, as a proportion, have any schooling. Intuitively we would have less confidence in estimates of the relationship for women, than if the sample had a much greater proportion of women with schooling. Another cause of differential power, given similar sample size, could be significant multicollinearity for women and not for men, meaning that there would be less power to detect an effect, given the same sample size.

8. Possible mechanisms or channels that explain why there is a negative effect for men and no effect for women

The negative effect of education attainment on political engagement for men, and the zero effect for women, may be explained in many ways, as suggested in Section 2. We now explore some of the possible mechanisms through which education affects political participation.

Let us consider a variable W that may act as a mechanism or mediator. The role of W can be assessed in two complementary ways: either in estimating the second stage of Equation (3) using W as the outcome variable instead of the index or the components of political participation, or in estimating Equation (3) adding W as a control variable. In the former case, education being a significant predictor of W supports the hypothesis that W acts as a mechanism. In the latter case, if W is significant and the effect of education on political participation is reduced in size, W mediates the education effect.

We estimate how education might have affected a number of mediating outcomes in the instrumental-variable model as above. Theory and previous papers suggest that attitudes towards democracy, perceptions about the current political regime or socioeconomic status may influence individuals' decisions to be politically engaged.

8.1. Attitudes towards democracy

We first consider attitudes and preferences regarding democracy as the preferred system of government, and views about the

functioning of democracy in Burkina Faso. These attitudes now are the outcome variables, instead of the responses about political engagement. We use the same specification as in the previous sections, with the presence of a school in the village of residence when the person was school eligible as an instrumental variable for the respondent's level of education. The analysis may offer insight into why more educated rural men are less likely to be politically engaged.

In large-sample analyses of World Values Survey and Afrobarometer, more education is typically found to be associated with more positive attitudes towards democracy as the "better" system of government (e.g. Aquino, 2015; Chong and Gradstein, 2015; Fuchs-Schündeln and Schündeln, 2015), though the correlations are not credibly identified as causal. In the case of Malawi, Evans and Rose (2007) used the Afrobarometer survey collected in 1999 and found that education was positively correlated with preferences for democracy and the rejection of nondemocratic alternatives. This finding was corroborated in cross-country analysis using a set of Afrobarometer surveys in Evans and Rose (2012) and García-Peñalosa and Konte (2014), where the support for democracy was found positively correlated with the education levels and was lower among women compared to men.

Respondents to the Afrobarometer in Burkina Faso were asked whether they were satisfied with the democracy in the country at the time of the survey, using a Likert scale. Over the five rounds of the survey, 11 % respondents were not satisfied at all, 31 % not satisfied, 39 % rather satisfied, and 19 % very satisfied. Panel A in Tables 10 and 11 uses as the dependent variable a binary variable equal to one if the respondent declared being satisfied or very satisfied, and Panel B uses the categorical variable. In both cases, satisfaction is significantly lower for educated men compared to less educated men, for all three specifications. For women, education does not significantly influence the level of satisfaction toward the level of democracy.

In addition, respondents were asked about their views about the level of democracy in Burkina Faso at the time of the survey, using a four-point scale: not a democracy, a democracy with minor concerns, a democracy with major concerns, or a full democracy. 8 % of the respondents considered that Burkina Faso was not a democracy; at the other extreme, 28 % viewed the country as a full democracy. Panel C uses the 4-point scale perception measure as dependent variable. Once again, the effect of education for men is negative and significant across all specifications and all measures of education. For women, the effect is negative as well. It is significant, however, only in the first specification with no birth and locality controls.

Lastly, people were also asked about their preferences for democratic or nondemocratic regime. Over the five rounds, 10 % reported that they preferred non-democratic regimes, 74 % democratic regimes, and 16 % declared that the type of regime did not matter for people like them. Panel D shows the IV coefficient of education on the probability of declaring a preference for a democratic regime. There is no significant effect, not surprisingly given the limited variation in the survey responses.

Overall, the main results are that more educated men appeared to be less satisfied with the level of democracy in Burkina Faso than less educated men. Moreover, educated men were less likely to see Burkina Faso as a full democracy, but there was no significant effect of education on their preference for a democratic regime. For women, on the other hand, education was not a significant and consistent predictor of attitudes towards democracy.

8.2. Functioning of public institutions

A second set of possible mechanisms concerns the perceptions about the functioning of public institutions in Burkina Faso. In

Table 10

Effects of education on the attitudes, with various measures of education, and with presence of school in the village when school eligible as instrumental variable, men sample.

	(1)	(2)	(3)	(4)
Education measure	Some schooling	Completed primary	Some secondary	Education attainment
Panel A: Satisfied with the level of democracy				
No cohort or locality controls	−0.243*** (0.074)	−0.296*** (0.089)	−0.339*** (0.104)	−0.083*** (0.025)
Birth year, province	−0.321*** (0.103)	−0.418*** (0.134)	−0.526*** (0.175)	−0.114*** (0.036)
Birth year, commune	−0.308** (0.143)	−0.390** (0.184)	−0.481** (0.230)	−0.105** (0.049)
Panel B: Satisfaction [1;4]				
No cohort or locality controls	−0.522*** (0.137)	−0.634*** (0.166)	−0.728*** (0.193)	−0.178*** (0.046)
Birth year, province	−0.669*** (0.199)	−0.873*** (0.262)	−1.099*** (0.340)	−0.237*** (0.070)
Birth year, commune	−0.661*** (0.271)	−0.837** (0.353)	−1.033** (0.440)	−0.226** (0.093)
Panel C: Views about democracy in Burkina Faso				
No cohort or locality controls	−0.460*** (0.134)	−0.548*** (0.160)	−0.623*** (0.183)	−0.154*** (0.045)
Birth year, province	−0.678*** (0.191)	−0.858*** (0.248)	−1.059*** (0.315)	−0.234*** (0.066)
Birth year, commune	−0.799*** (0.284)	−0.957*** (0.353)	−1.151*** (0.438)	−0.260*** (0.093)
Panel D: Preferences for democracy				
No cohort or locality controls	−0.088 (0.068)	−0.108 (0.083)	−0.123 (0.094)	−0.030 (0.023)
Birth year, province	−0.011 (0.086)	−0.014 (0.112)	−0.018 (0.140)	−0.004 (0.030)
Birth year, commune	0.066 (0.121)	0.083 (0.151)	0.100 (0.186)	0.023 (0.041)

Notes: Afrobarometer Burkina Faso data, rural residents born after 1950, various rounds, men only. Coefficients of the education measure are reported and their standard errors, clustered at the village level, are in parentheses. All specifications include dummy variable for two post-2014 survey rounds (after popular uprising leading to democratic transition), and birth cohort and geographic dummy variables as indicated, and are estimated with Stata command `ivreghdfe`. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11

Effects of education on the attitudes, with various measures of education, and with presence of school in the village when school eligible as instrumental variable, women sample.

	(1)	(2)	(3)	(4)
Education measure	Some schooling	Completed primary	Some secondary	Education attainment
Panel A: Satisfied with the level of democracy				
No cohort or locality controls	−0.068 (0.095)	−0.088 (0.123)	−0.101 (0.143)	−0.026 (0.037)
Birth year, province	0.023 (0.126)	0.030 (0.164)	0.037 (0.202)	0.009 (0.051)
Birth year, commune	0.292 (0.213)	0.441 (0.334)	0.636 (0.511)	0.134 (0.101)
Panel B: Satisfaction [1;4]				
No cohort or locality controls	−0.214 (0.180)	−0.276 (0.233)	−0.319 (0.269)	−0.083 (0.070)
Birth year, province	−0.168 (0.241)	−0.219 (0.315)	−0.269 (0.387)	−0.067 (0.097)
Birth year, commune	0.250 (0.379)	0.378 (0.583)	0.544 (0.859)	0.115 (0.177)
Panel C: Views about democracy in Burkina Faso				
No cohort or locality controls	−0.294* (0.163)	−0.371* (0.206)	−0.428* (0.237)	−0.112* (0.062)
Birth year, province	−0.331 (0.222)	−0.407 (0.274)	−0.490 (0.329)	−0.127 (0.085)
Birth year, commune	−0.197 (0.357)	−0.267 (0.481)	−0.354 (0.635)	−0.083 (0.149)
Panel D: Preferences for democracy				
No cohort or locality controls	−0.017 (0.092)	−0.022 (0.117)	−0.025 (0.135)	−0.007 (0.035)
Birth year, province	−0.036 (0.122)	−0.047 (0.158)	−0.056 (0.189)	−0.014 (0.048)
Birth year, commune	0.223 (0.183)	0.330 (0.274)	0.442 (0.374)	0.099 (0.082)

Notes: Afrobarometer Burkina Faso data, rural residents born after 1950, various rounds, women only. Coefficients of the education measure are reported and their standard errors, clustered at the village level, are in parentheses. All specifications include dummy variable for two post-2014 survey rounds (after popular uprising leading to democratic transition), and birth cohort and geographic dummy variables as indicated, and are estimated with Stata command `ivreghdfe`. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the five waves of Afrobarometer surveys, respondents were asked about the extent to which they trusted a set of institutions, and their perceptions about whether leaders were involved in corruption.

The level of trust in institutions was measured on a 4-point scale (not at all, just a little, somewhat, a lot) for the president, parliament, the local governmental council, and the Independent Electoral Commission (CENI). Overall, 72 % of respondents declared that they trusted the president a lot, 67 % the parliament, 70 % their local governmental council, and 68 % the CENI. We constructed an index equal to the sum of the response “trust a lot” for these four institutions; 45 % of respondents had an index equal to 4, 16 % had an index equal to 0, and the average was 2.6.

Respondents were also asked about their perceptions about corruption for specific groups of people. The question was formulated as follows: “How many of the following people do you think are involved in corruption, or haven't you heard enough about them to say?”. The possible responses were: none, some of them, most of them, all of them, don't know/haven't heard. We focus on the responses about corruption of (i) the president and officials of the presidency, (ii) the representatives of parliament, and (iii) elected municipal councilors. We recode responses to be a binary variable for the respondent believing that all of them are involved in corruption. Among our sample, 28 % of the respondents think that all local councils are involved in corruption, 27 % for the president and officials of the presidency, 31 % for the representatives of parliament. We summarize the responses in an index taking values from 0 to 3. The mean of the index is 0.75; most respondents (61 %) think that none of these three groups are such that all their members are involved in corruption.

Each of these two indices, one for trust in institutions, one for perceptions about corruption, is used as outcome variable in Panels

A and B of Tables 12 and 13. We use the same instrumental variable specification as in the main analysis. Results are consistent with the causal effects of education on political engagement. Trust in politicians apparently decreases with education for both men and women. The effects are significant among the female sample for every specification. For men, the effects lose their significance when controlling for the birth year dummy variables along with the commune dummy variables. Regarding perceived corruption, the level of education has no effect for women, while for men, the effects are significant only in the specification with no controls for cohort or locality and are positive (more educated men think politicians are more likely to be corrupt).

These results are consistent with the idea that education decreases trust in politicians, that in turns decreases political participation. This channel could be related to more pessimism in the usefulness of the citizen's actions, or lower perceptions of the benefits of their actions if they perceived politicians as untrustworthy and corrupted.

8.3. Socio-economic status

A third set of mechanisms is related to socio-economic status, influenced in all likelihood by education, and presumably a reason for changing levels of political engagement. Increased income is one of the “downstream effects” mentioned in Larreguy and Marshall (2017). The Afrobarometer survey does not collect consumption or income data. We instead use two measures: whether the person had a paid job and whether the house of the respondent had a solid roof. 75 % of the rural sample respondents have a solid roof and only 12 % have a paid job.

Results from the second-stage regressions are shown in Panels C and D of Tables 12 and 13 for men and women respectively.

Table 12

Effects of education on the trust, corruption, living conditions, with various measures of education, and with presence of school in the village when school eligible as instrumental variable, men sample.

Education measure	(1) Some schooling	(2) Completed primary	(3) Some secondary	(4) Education attainment
Panel A: Index of trust in politicians [0;4]				
No cohort or locality controls	−0.672*** (0.237)	−0.820*** (0.285)	−0.943*** (0.330)	−0.229*** (0.080)
Birth year, province	−0.738** (0.322)	−0.951** (0.412)	−1.204** (0.529)	−0.260** (0.113)
Birth year, commune	−0.688 (0.466)	−0.835 (0.569)	−1.038 (0.709)	−0.230 (0.156)
Panel B: Index of perception about corruption [0;3]				
No cohort or locality controls	0.334* (0.173)	0.407* (0.210)	0.469* (0.242)	0.114* (0.059)
Birth year, province	0.376 (0.237)	0.483 (0.303)	0.613 (0.385)	0.132 (0.083)
Birth year, commune	0.501 (0.358)	0.608 (0.434)	0.756 (0.542)	0.167 (0.120)
Panel C: Have a paid job				
No cohort or locality controls	0.262*** (0.059)	0.320*** (0.072)	0.368*** (0.081)	0.090*** (0.019)
Birth year, province	0.329*** (0.076)	0.422*** (0.099)	0.534*** (0.126)	0.116*** (0.026)
Birth year, commune	0.311*** (0.105)	0.381*** (0.128)	0.468*** (0.157)	0.105*** (0.034)
Panel D: Have a solid roof				
No cohort or locality controls	0.447*** (0.078)	0.518*** (0.095)	0.603*** (0.113)	0.147*** (0.026)
Birth year, province	0.502*** (0.114)	0.602*** (0.146)	0.778*** (0.197)	0.166*** (0.039)
Birth year, commune	0.455*** (0.146)	0.514*** (0.172)	0.657*** (0.225)	0.147*** (0.047)

Notes: Afrobarometer Burkina Faso data, rural residents born after 1950, various rounds, men only. Coefficients of the education measure are reported and their standard errors, clustered at the village level, are in parentheses. All specifications include dummy variable for two post-2014 survey rounds (after popular uprising leading to democratic transition), and birth cohort and geographic dummy variables as indicated, and are estimated with Stata command `ivreghdfe`. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 13

Effects of education on the trust, corruption, living conditions, with various measures of education, and with presence of school in the village when school eligible as instrumental variable, women sample.

	(1)	(2)	(3)	(4)
Education measure	Some schooling	Completed primary	Some secondary	Education attainment
Panel A: Index of trust in politicians [0;4]				
No cohort or locality controls	−0.944*** (0.300)	−1.231*** (0.387)	−1.429*** (0.448)	−0.371*** (0.116)
Birth year, province	−1.096*** (0.407)	−1.433*** (0.531)	−1.744*** (0.646)	−0.440*** (0.161)
Birth year, commune	−1.000* (0.597)	−1.507* (0.895)	−2.050* (1.222)	−0.454* (0.267)
Panel B: Index of perception about corruption [0;3]				
No cohort or locality controls	0.084 (0.196)	0.110 (0.256)	0.128 (0.297)	0.033 (0.077)
Birth year, province	0.212 (0.264)	0.277 (0.345)	0.337 (0.419)	0.085 (0.106)
Birth year, commune	−0.258 (0.385)	−0.388 (0.587)	−0.528 (0.807)	−0.117 (0.177)
Panel C: Have a paid job				
No cohort or locality controls	0.233*** (0.064)	0.305*** (0.083)	0.354*** (0.093)	0.092*** (0.025)
Birth year, province	0.233*** (0.086)	0.307*** (0.111)	0.374*** (0.133)	0.094*** (0.034)
Birth year, commune	0.232** (0.116)	0.353* (0.180)	0.484* (0.247)	0.106** (0.052)
Panel D: Have a solid roof				
No cohort or locality controls	0.467*** (0.099)	0.617*** (0.136)	0.704*** (0.158)	0.183*** (0.040)
Birth year, province	0.514*** (0.134)	0.704*** (0.191)	0.863*** (0.240)	0.212*** (0.056)
Birth year, commune	0.503** (0.198)	0.730** (0.299)	1.008** (0.444)	0.223** (0.090)

Notes: Afrobarometer Burkina Faso data, rural residents born after 1950, various rounds, women only. Coefficients of the education measure are reported and their standard errors, clustered at the village level, are in parentheses. All specifications include dummy variable for two post-2014 survey rounds (after popular uprising leading to democratic transition), and birth cohort and geographic dummy variables as indicated, and are estimated with Stata command ivreghdfe. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Results go in the expected direction: education increases the probability of having a paid job and the probability of living in a dwelling with a solid roof. The coefficients are positive and significant for every measure of education and every specification as the controls for birth year and location are added. These results are consistent with the hypothesis that education increases socio-economic standing, that increases the opportunity cost of political participation, and finally that reduces the likelihood of political participation.

9. Conclusion

Political engagement by citizens influences, to some degree, economic policy making, especially in the provision of public goods and the choices made regarding policies that have broad rather than concentrated benefits (Khemani et al., 2016). Political engagement also sustains and strengthens democratic institutions, and maintains institutional legitimacy.

The relationship between education and political engagement has been a perennial concern in political economy, as democratic institutions around the world ebb and flow. Recent empirical work in low-income countries has suggested that the relationship may be quite variable, both in terms of different social groups exhibiting different tendencies and in terms of tendencies changing significantly depending on the political context. A few papers have leveraged randomized control trials to examine the relationship (Friedman et al., 2016; Kuenzi, 2006). Some papers approach the relationship at a more aggregated level (Acemoglu et al., 2005; Diwan and Vartanova, 2020; Paglayan, 2021). A growing literature uses individual-level survey data and quasi-experimental identification strategies (Croke et al., 2016; Dang, 2019; Kuenzi, 2006;

Larreguy and Liu, 2022; Larreguy and Marshall, 2017; Parinduri, 2019).

Our approach to estimating the relationship builds on Card (1995), who used data on the geographic location of educational institutions to implement an instrumental variables estimation strategy, and closely resembles the more recent work of André and Maarek (2022), who used data on the timing of school establishment in villages in Mali. We matched information about the timing of establishment of primary schools at the village level to nationally-representative Afrobarometer survey data from Burkina Faso. The timing of school establishment relative to the age of each respondent provides an instrumental variable for the individual's education attainment. Our results suggest that formal schooling in the context of Burkina Faso, with low overall education levels typical of many poor African countries, probably has a clear negative effect on political engagement for men, and either a zero or small negative effect for women. The effect for men appears to be that completing some schooling reduces an index of engagement by about half a standard deviation. This would seem to be a fairly substantial effect. The lower overall levels of schooling attainment for women means our estimation approach is under-powered for women, for the effect sizes that seem to be common in the literature. Of course, there is variation in the estimates (in magnitude and statistical significance) across specifications.

There are possible threats to the internal validity of the findings. One of the most important threats is selective migration. If the establishment of a school in a village caused households with children who were of school age to migrate to a village, and those households were less likely to be politically engaged, the negative effect could be due to a migration pattern, and not to a direct or indirect effect of schooling attainment. We cannot control for

migration directly using the Afrobarometer survey data. We do, however, incorporate several indicators of migration rates at the commune level, and find that the estimated coefficients do not change much, suggesting that selective migration varying at the commune level is not accounting for the negative effect for men and small or zero effect for women.

In terms of external validity, the finding of a negative relationship for men, and a zero or small negative effect for women, may be specific to the data used here, and indeed the literature on this relationship makes clear that estimates of the relationship with data from other countries and other electoral contexts varies. It is worth speculating about the difference between our estimates for Burkina Faso and those of André and Maarek (2022) for Mali. The two neighboring countries share similar socio-economic characteristics. Both have similarly sized, and young, populations of about 20 million. They both are near the bottom of the Human Development Index, Burkina Faso ranked 178 and Mali 180. The poverty headcount ratio at national poverty lines was about 40 % in 2018, for both countries. Life expectancy at birth in 2020 was 62 and 60 years old for Burkina Faso and Mali, respectively. The national literacy rates among the adult population aged 15 and above were dramatically low at 39 % in Burkina Faso and 31 % in Mali in 2018 (source: World Development Indicators from the World Bank).

Despite these similarities, André and Maarek (2022) estimate a positive relationship between education and engagement, while we estimate a negative relationship. There are numerous possible explanations for the difference. For example, the political contexts at the time that surveys asked about political engagement were different. The Mali sample was asked about engagement in the 2007 to 2011 period, a time of apparent consolidation of democracy and before the civil war that erupted in 2012. The Burkina Faso samples covered the period before and after the Compaoré regime.

Contexts differ in many other ways. A handful of studies have compared the social structures of Burkina Faso and Mali, examining and explaining salient differences. Johnson (2020) and Johnson (2022), for example, explored how different colonial experiences shaped some of the attitudes and expectations of women differently in the two countries. Hagberg and Koné (2019) compared the political trajectories of several women political leaders. Bonnassieux (2002) suggested that cotton farmer producer groups had different trajectories, with those in Mali being more politically engaged in opposition to state capitalism, while those in Burkina Faso having been co-opted by the privatized cotton companies. These studies provide the beginnings of a more comprehensive comparative politics, and perhaps may be extended throughout the region, and provide a basis for attributing differences in the relationship between education and political engagement to differences in certain key economic and political structures and institutions.

Similarly, we hope that other researchers will build on the results of this paper and that of André and Maarek (2022), to eventually permit better understanding of the effects of secondary and tertiary schooling on political engagement.

Given the continued expansion of formal education, and the pronounced trend away from democratic governance in many West African countries with recent military-led coups d'état, the finding that neither educated men nor women increase their political engagement, relative to those uneducated, should be cause for concern for citizens of Burkina Faso and other countries of the region. The differential gendered effects of education on participation moreover suggest that democracy and civic engagement promotion efforts might allocate resources to determining whether targeting women (or men) may be more (or less) cost-effective in terms of impact per dollar spent. The differential gendered effects

further suggest that the content of civic and political engagement modules in school curricula and materials and actual pedagogy practices of teachers might be examined for gendered messaging or responsiveness.

Data availability

The authors do not have permission to share data.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Acknowledgements

We are grateful to the editor and the anonymous referees, and to Clément d'Haultfoeuille for insightful discussions at an early stage of the paper, to participants at the Santa Clara University Economics Department seminar and participants at the Oxford Center for the Study of African Economies annual conference poster session for helpful comments, and to Horacio Larreguy, and Pierre André for comments on an earlier draft. All errors of course remain responsibility of the authors.

Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.worlddev.2023.106184>.

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