We examine the effects of national voter registration policies on voting and registration patterns with a large-scale experimental study. Together with Kenya’s electoral commission, we designed an experiment in which 1,674 communities were randomized to a status quo or treatment group, receiving civic education on voter registration, short message service reminders about registration opportunities, and/or local registration visits by election commission staff. We find little evidence that civic education improves registration. Local registration visits improve voter registration, a relationship that increases in poorer communities. Moreover, local registration increased electoral competition and vote preference diversity in down-ballot contests in the 2017 Kenyan elections. Our results suggest that status quo voter registration policies constrain political participation and competition, and that inexpensive policy changes may attenuate the effects of such constraints.
policies provides insight into how existing policies may shape the electorate.

Theoretically, election administration may shape citizen participation and electoral outcomes via three channels (Harder and Krosnick 2008): citizen ability (e.g., understanding eligibility requirements); motivation (e.g., social pressure or norms regarding participation); and difficulty (e.g., factors influencing the costs of participation). Election commissions may target these channels through decisions about where, how, and how often to register voters, as well as via their communication and education strategies. Geographic and neighborhood characteristics may moderate how such policies affect difficulty, motivation, and ability, and, in turn, participation.

To study how changes in voter registration policies affect voter registration, we designed a policy experiment with the IEBC to randomize information about and opportunities for voter registration. We developed three interventions, randomizing 1,674 polling stations into control and five treatment groups, implemented during November and December 2016. Control polling stations continued with the status quo, where citizens in the local community had to travel to a constituency election office to register to vote and received no additional information or prompts from the IEBC. The first intervention—localization—reduced the cost, inconvenience, and difficulty of registration by sending IEBC staff with mobile registration equipment to register citizens. The second intervention—canvassing—involves IEBC civic educators providing citizens living near selected polling stations with information on registration, potentially improving a citizen’s ability to participate by providing them with basic information about the benefits and requirements of registering to vote. The third intervention—short message service (SMS) reminders—entailed sending mobile phone messages to existing registered voters asking them to remind their unregistered friends and acquaintances about registration opportunities, leveraging social relationships and pressure to improve registration. The content of these interventions represent potential alternative strategies that the electoral commission could use to register voters. As we discuss in detail below, our experimental design allows us to learn about the causal effect of these interventions and their combinations, as well as how they operate across different contexts. In addition, collaboration with the IEBC enabled access to administrative voter registration records for measuring the effects of these interventions.

This article contributes in several ways to the burgeoning literature on how election administration shapes political behavior in fledgling democracies (De Kadt 2017; Ferree et al. 2020; Neggers 2018). First, we evince how changing centrally organized and implemented voter registration policies to improve access to the franchise affects voter registration and election outcomes. Like Ichino and Schundeln (2012), we focus on the preelection administrative process of voter registration. While that work examined registration-related fraud via random allocation of observers, our work shows how changing voter registration procedures affects voter registration patterns relative to the status quo. Consistent with research on costs of voter registration in Western contexts (e.g., Bhatti 2012; Bracconier, Dormagen, and Pons 2017; Brady and McNulty 2011; Dyck and Gimpel 2005; McNulty, Dowling, and Ariotti 2009; Stein and Vonnahme 2008) and the Harder and Krosnick (2008) theoretical framework, improving the accessibility of voting registration opportunities increases voter registration by about 2% over status quo polling stations. In contrast, observational evidence in Isaksson (2014) suggests that individual resources do little to explain variation in voter turnout, a result that may be explained by misreporting or social desirability bias in survey data (Adida et al. 2019; Kolstad and Wiig 2016).

Second, we find differences in effects by context. By combining satellite data with the spatial locations of polling stations, we blocked units on three prognostic covariates—poverty, population density, and distance to the constituency election office—allowing us to efficiently estimate treatment effects across subgroups (Moore 2012). Similar to Nickerson (2015) and consistent with Harder and Krosnick (2008), we find strong differences by context, with higher-poverty units seeing the greatest increases in registration. Localization increases voter registration by approximately 4.4% in the poorest areas, relative to 0.7% in the richest areas. In addition, our results span across urban and rural areas—a feature heretofore unexplored in the experimental literature—and show that distant, sparsely populated areas in particular benefit from reducing the difficulty of registration. These results suggest that some political inequalities relate to economic and geographical inequalities.

Third, we examine how impersonal SMS reminders of registration opportunities shape voter registration. Unlike previous work, we test the multiplicative nature of Harder and Krosnick (2008) directly. We find that when combined with localization, SMS reminders increase voter registration by 2.4% relative to the status quo, 20% more than localization alone. Without localization, SMS reminders have no effect. This contrasts with work showing the positive effects of SMS across a range of outcomes, including voter turnout (e.g., Dale and Strauss 2009; Malhotra et al. 2011; Marx, Pons, and Suri 2017), savings behavior (e.g., Karlan et al. 2016), and adherence to antiretroviral therapy (Pop-Eleches et al. 2011). However, our findings complement Bennion and Nickerson (2011), who find that SMS messaging increases voter registration in the United States, but only once an individual possesses a voter
registration form. This is analogous to our findings in the (very different) Kenyan context: an SMS intervention is effective, but only once citizens are confronted with a registration opportunity.

Fourth, we ask how civic education efforts affect voter registration, finding that information alone does not increase voter registration. Specifically, we find near-zero effects for civic education, even when voter registration costs are reduced to zero.\(^2\) We know of two other studies that explore the impact of civic education on registration. Mvukiyehe and Samii (2017) find no evidence that civic education via town hall discussions improved registration in Liberia. Bratton et al. (1999), in contrast, examine multiple civic education programs in Zambia and find a positive impact on registration. Both studies rely on survey-based outcomes to measure voter registration, in contrast to the administrative data upon which this study builds.

Finally, we measure the effects of localization on Kenya’s 2017 General Elections, which took place eight months after the interventions. Localization has a positive but insignificant effect on the total number of registered voters and votes cast at polling stations. In addition, localization decreased turnout rates, but this effect is indistinguishable from zero. This finding suggests that those registered during the experiment are less likely to vote. This result could be explained by a "double hurdle": localization affected the costs of registration, but not of getting to the polls on election day. We do find that local registration increased electoral competition and vote preference diversity in down-ballot contests.

In the next section we anchor this study in the Kenyan context, describing voter registration policies and barriers to registration. The experimental design section discusses the interventions, data, and empirical strategy. The following section presents the results. The final section gives the conclusion.

**ELECTORAL ADMINISTRATION AND BARRIERS TO VOTER REGISTRATION IN KENYA**

Social tension, mistrust, and violence accompanied both the 2007 and 2013 General Elections in Kenya, which played out against a deeper background of ethnic politics and violence (Boone 2011; Cheeseman, Lynch, and Willis 2014; Kanyinga 2009; Mueller 2008). The 2017 General Elections—held eight months after the close of the interventions studied here—proved a continuation of these themes. Relative to problematic biometric voter registration in the 2013 elections, improved implementation translated into a six-fold decrease in the number of records missing biometric information and improvement in election-day electronic voter identification (Carter Center 2013; ELOG 2017; IEBE 2017).\(^3\) Despite these improvements, issues regarding results transmission and transparency led the Supreme Court to annul the presidential election. The nullification issued from qualitative noncompliance with electoral laws, rather than a demonstration that irregularities affected the electoral outcome.\(^4\) The fresh elections, held in October 2017, were largely boycotted by the opposition. In the election-related analyses below, we use data from the August elections, as the October data were incomplete due to the boycott.

Existing studies of voter registration focus on contexts where third-party registration is the norm, such as France (e.g., Bracconier et al. 2017) or the United States (e.g., Nickerson 2015). Third-party registration decentralizes voter registration policies by enabling outside groups to register voters (Herron and Smith 2013). In contrast, Kenya’s IEBE is the sole body providing voter registration to citizens, an arrangement common in many developing countries. Specifically, the Constitution mandates that the IEBE provides continuous voter registration (CVR), giving citizens an opportunity to register throughout the year. In practice, this means that an IEBE staff member is available at a constituency election office to provide registration year-round.\(^5\)

Anecdotal evidence and preexperiment interviews with IEBE staff suggest that the CVR policy may generate political inequalities in several ways. First, the stationary approach to CVR puts the costs of voter registration on citizens.\(^6\) Traveling to register is costly, effectively barring poorer citizens from registering during CVR (IED 2007, xv).\(^7\) Furthermore, sparse population density may hinder information flows about, as well as citizen access to, voter registration opportunities when they do arise.

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\(^2\) In app. B, we give an overview of other studies that explore the impact of civic education on political behavior and knowledge, though these studies show little effect on behavioral outcomes like those studied here.

\(^3\) According to a legally mandated audit prior to the 2017 elections, Kenya’s enrollment rate—the percentage of individuals registered divided by those eligible—stood at 78% by a measure defining eligibility as the estimated number of individuals possessing national identification documents. According to that report, this puts Kenya in thirty-first place among African countries in terms of citizen enrollment on the register.

\(^4\) See Carter Center (2019) for a discussion of these legal debates.

\(^5\) At the time of this experiment, Kenya had 290 constituencies, each of which had one constituency election office from which the IEBE organized election-related activities in that constituency.

\(^6\) In the counties studied here, the average constituency (the political unit with an IEBE registration office) serves 93 polling stations, with a minimum of 45 and a maximum of 213 polling stations. The average polling station is 20 kilometers from the registration office.

\(^7\) We do not directly measure individual cost reductions in this article. However, both discussions with IEBE staff and grey literature on election administration in Kenya suggest that cost is a key barrier to registration. For instance, IED (2007, 12) reports: “Citizens sometimes must travel long distances to register, losing work hours and incurring heavy travel expenses.”
(IED 2002). Second, a lack of funding for mobile registration means that IEBC staff can only provide registration to a subset of relatively nearby communities. Third, political interests may sponsor IEBC voter registration drives in certain areas. This raises questions about the independence of registration opportunities, since politicians would theoretically be reticent to fund registration in areas where they have little political support.

Figure 1 complements this qualitative evidence with data. Using bivariate logistic regression, the figure shows that poverty rates, distance from the election office, and population sparsity are negatively related to voter registration at polling stations from March 16 to November 14, 2016—the CVR period before the onset of the experiment. These data suggest that the status quo voter registration policy systematically excludes poorer, more remote, and more isolated polling stations from recruitment onto the voter register. Our blocked design (discussed below) explicitly incorporates these three factors to explore heterogeneous effects of the interventions.


These problems are by no means limited to non-Western contexts. Writing on challenges of registering Black voters in the 1970s US South, a period where third-party registration was relatively uncommon, Lewis and Allen (1972) find problems strikingly similar to Kenya’s: “In most of the southern states, registration is conducted only at the county courthouse, requiring round-trip travel of 40 or 50 miles or more for residents in outlying areas of rural counties. The lack of dispersed registration centers, the reluctance of election officials to appoint deputy registrars, and the absence of mobile registration stations also contribute to the suppression of initiative on the part of potential black registrants.”

While the Kenyan context may be quite different than the United States, that does not necessarily mean that we should expect different behavioral responses. Individuals likely respond to decreased costs and additional information in similar ways, regardless of context. Rather, our contribution lies in a carefully planned experiment in both a different macrocontext (i.e., where election administration is centralized), as well as a large variety of microcontexts (via blocking). In the United States, cost reductions can be implemented by any number of organizations seeking to register voters. In Kenya, the electoral commission’s centralized control of registration means that they are uniquely responsible for shaping the registration costs of citizens. While we may see similar behavioral responses across contexts, the reasons for those changes and the counterfactuals that we can estimate via an experiment, are fundamentally different. Moreover, existing work on voter registration constraints focuses on urban contexts in high-income countries, whereas our results examine both rural and (relatively) urban
settings in a low- to middle-income country (e.g., Braconnier et al. 2017; Nickerson 2015).

Understanding how status quo voter registration policies affect who can and cannot register to vote strikes at questions of democratic fairness and access germane to political processes worldwide. These questions are especially pressing in fledgling democracies, like Kenya, where often a single organization conducts voter registration in the presence of multiple barriers to political participation. In the next section, we describe a research design conceived to address such inequalities.

**EXPERIMENTAL DESIGN**

Given CVR’s potential problems, we developed an experimental design with the IEBC to study how changes to the status quo may improve voter registration, particularly in poorer, more distant, and more sparsely populated communities. We study these problems in seven Kenyan counties, which were selected based on being sufficiently similar (by poverty, distance to registration office, and population density) and the level of government support in the previous election.10 These counties contain 3,828 polling stations. We randomly selected 1,674 polling stations within these counties, following a strategy that minimized the chances of spillovers, and blocked on poverty, distance, and population density.11

We define the units of analysis in this study as polling stations for several reasons. First, as in Nickerson (2015), there is no population register enabling us to identify and randomize across unregistered citizens. Nickerson (2015) addressed this problem by randomizing across streets. With no address or well-documented street system outside of large cities, this approach proved impossible. Polling stations provided a viable alternative. Second, the polling station is the smallest unit at which registration and election data are collected and available in Kenya. Third, polling stations are the natural randomization unit in this context, since registration policies are organized around polling stations and home registration is unavailable.

**The interventions**

In addition to CVR, the IEBC implemented three types of interventions between November 14, 2016, and December 23, 2016. First, selected polling stations were visited for two days by IEBC staff with portable voter registration equipment, which captures a citizen’s photograph, biometric information, and other required fields like gender, name, and date of birth. IEBC staff identified a location within approximately 250 meters of the selected polling station, setting up an IEBC banner and table for the registration equipment to register voters from 9 a.m. until 5 p.m. Citizens in surrounding communities were able to register at this location. We call this intervention *localization*.

The second intervention involved *canvassing*. IEBC staff visited the communities surrounding the polling station for two days to provide basic education on the voter registration process, to discuss the importance of electoral participation, and to answer any remaining questions. IEBC staff also provided information on the practicalities of voter registration, including the location of the nearest registration opportunity. To maximize external validity, the IEBC used its standard training and implementation protocol for canvassing by IEBC field staff. The canvassing usually involved one-on-one household-level discussions that lasted around ten minutes. If the opportunity arose, impromptu discussions with groups of people lasted from ten to thirty minutes, depending on the number of questions raised. The IEBC instructed staff to canvass from 9 a.m. to 5 p.m., working outward from the polling station into the nearby communities.12

Third, the IEBC sent SMS messages to existing registered voters at a polling station.13 In Kenya, cellphone coverage and ownership is high.14 While citizens are not required to provide a phone number upon registering to vote, many do.15 The message, which clearly identified the IEBC as the sender, asked citizens to encourage their unregistered friends and family to register and identified the nearest registration opportunity. Two SMS messages per person were sent over a two-day period. In total, the IEBC sent almost 300,000 messages via SMS. Because the SMS messages were sent only to already registered voters, this treatment could be considered conservative, relying on registered recipients sharing the information with an unregistered person.16

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10. County selection took place in collaboration with the IEBC to avoid perceptions of political bias. See app. C for additional details.

11. Details about county and polling station selection, and information about the selected counties, can be found in app. C. Ex post power calculations suggest that this study is well powered to observe even small effects (app. D).

12. The IEBC voter education manual can be found online: https://www.iebc.or.ke/uploads/resources/pdQMe3WKeV.pdf (accessed August 7, 2019).

13. The reason we target existing voters is because there is no phone registry listing unregistered citizens.

14. Information from the nationally representative 2016 Afrobarometer survey, for example, indicates that 98% of enumeration areas have cellphone coverage, and 85% of respondents personally own a mobile phone.

15. Of the 338,988 previously registered voters in the sampled polling stations, 147,277 (43%) of records listed a phone number, of which 143,704 (42% of all records) were valid and included in the bulk SMS intervention. Note that registered voters providing their mobile numbers may be systematically different (e.g., more likely to share information) than those not sharing their contact information.

16. The need for the information to be shared to be effective implies that the treatment effect is likely to differ across characteristics like a community’s network structure. We did not measure these characteristics.
Were the interventions implemented? Project documents kept by IEBC staff report that all polling stations received their assigned treatment. Independent monitors made unannounced visits to a random 10% of polling stations, finding, under reasonable assumptions given the mobility of canvassers, compliance of around 80% across treatment arms.17

Random assignment of the interventions
We randomly assigned treatment to polling stations following a two-by-three factorial design as in table 1. In order to minimize the effect of spillovers, we first sampled 1,674 polling stations from the sampling frame of 3,828 polling stations. Our sampling algorithm ensured that polling stations were at least 1.5 kilometers apart. On average, sampled polling stations were separated by 2.4 kilometers.18 Each factorial cell contained 279 polling stations. Pure control polling stations received no canvassing, SMS, or localization, representing the status quo CVR registration policy. For canvassing-only polling stations, citizens were provided with basic civic education and directions to the constituency election office for registration. For SMS-only polling stations, the text message reminded citizens to register to vote at the constituency office at any time.19 For localization-only polling stations, IEBC staff set up a portable registration site near the local polling station, enabling the local community to register on the spot without travelling to the constituency election office, thus reducing the distance (and travel costs) required to register. Polling stations assigned to both the localization and canvassing interventions received both treatments simultaneously on two consecutive days. During this intervention, canvassers directed citizens to the ongoing local registration opportunity. For polling stations assigned localization and SMS interventions, the SMS was delivered for two consecutive days at 6 p.m. the evening before the localization intervention began.20

but randomization implies that these should be balanced across treatment conditions.

17. Appendix E discusses monitoring and compliance in more detail. In less than 3% of interventions, implementation dates differed from those assigned. Our analyses presented in the results section use the assigned treatment dates, attenuating estimated treatment effects.

18. Additional details related to randomization and treatment assignment can be found in app. C.

19. The SMS, translated from Kiswahili: “Thanks for being a registered voter! Those unregistered can register at IEBC office (town/village). Please spread this message.” The SMS was customized to identify the location of the closest IEBC office where registration occurs.

20. The SMS, translated from Kiswahili: “Thank you for registering as a voter. IEBC will visit polling station . . . tomorrow to register voters. Please spread this message.” The SMS was customized to indicate the location of the local registration opportunity.

<table>
<thead>
<tr>
<th>Table 1. Polling Stations by Treatment Status</th>
</tr>
</thead>
<tbody>
<tr>
<td>Canvassing</td>
</tr>
<tr>
<td>Localization</td>
</tr>
<tr>
<td>No localization</td>
</tr>
</tbody>
</table>

Note. Number of polling stations by treatment status. SMS = short message service.

To measure treatment effects across different contexts, we grouped polling stations in blocks based on poverty, distance to the registration office, and population density.21 Blocking has several advantages. First, randomizing within blocks improves balance across treatments. Second, blocking allows efficient estimation of treatment effects within sub-groups (Duflo, Glennerster, and Kremer 2008).

Finally, ethical concerns are inherent in any field experiment. Given that we worked in collaboration with the IEBC, potential registrants were unaware that the intervention was part of an experiment. Rather, it was viewed as part of the operations of the IEBC. This approach ensured that citizens would respond normally to the intervention. Relative to the status quo, the interventions pose little risk to citizens. Finally, the experiment included counties showing both government and opposition support, ensuring political impartiality.

Data
The target population is voting age individuals unregistered for the 2017 General Elections at the onset of the interventions. Collaboration with the IEBC provided access to detailed data on individual registration records. The data comprise individual-level records of a person’s birth year, gender, cellphone number, the polling station where they wish to vote in the 2017 elections, and the day registration occurred. From these data, we generate outcome measures of registration at the polling station-day and polling station levels.

To measure the effects of the interventions on election day, we use publicly available data from the August 2017 Kenyan General Elections. Voters selected candidates for six different positions. Nation-wide, 8 candidates ran for president, 210 for governor, 256 for senator, 299 for county women representative, 1,893 for member of parliament, and 11,857 for ward representative. From the IEBC website, we collected turnout and vote outcomes at the polling station level for each election.

In addition, we used the spatial locations of polling stations to harvest spatial data on poverty rates, distance to the local

21. Specifically, we use multivariate continuous blocking as in Moore (2012).
constituency election office, and population density for each polling station. Appendix C discusses these data.

RESULTS
In this section, we present experimental results in three steps. First, we examine the effects of the interventions on voter registration during the intervention period. Second, we examine heterogeneous treatment effects across subgroups of poverty, distance, and population density. Third, we examine the downstream effects of the experiment on election day outcomes.22

Proximate impacts on registration
We first explore the proximate impact of the interventions on voter registration, focusing on the period from November 14, 2016, the onset of the interventions, to January 6, 2017, 10 days after the last day of the interventions. In total, excluding weekends, the data include 66,560 polling station days across the six treatments. We focus on this period—which falls during normal CVR operations—to test how simple deviations from status quo policies affect voter registration.

While localization is likely to be effective only on the two days of the intervention when registration is present, information from canvassers or via SMS may motivate action in the days after treatment. To capture this, the (preregistered) treatment variable defines the localization treatment as only the two days when the registration kit was at a randomly selected polling station. In contrast, the canvassing and SMS-only treatments are coded as the two intervention days plus a 10-day “tail.” The remaining polling station-days constitute the control condition.23

During the intervention period, polling station days assigned to the status quo condition saw few individuals register: approximately one individual every 22.7 days (0.044 individuals per polling station per day). In total, of the 66,560 polling station days assigned to the status quo condition, only 2,568 (3.86%) experienced nonzero registrations, 0.01% of 2013 registered voters.24 These low numbers are consistent with prior CVR results (IREC 2008).

Table 2 presents the estimated treatment effects of the interventions, using a variety of model specifications, which point to similar substantive conclusions. Column 1 presents results from regressing the total number of individuals registered on the six treatment conditions at the polling station-day level.

Column 2 adds block, polling station, and day fixed effects.25 Column 3 adds controls for poverty, distance from the registration office, and population density, and weights by the inverse of the polling station-specific inclusion probability to estimate population average treatment effects.

To better interpret effect sizes, column 4 measures the outcome as the number of registered individuals at a polling station day divided by the total number of registered individuals at that polling station in 2013.26 Coefficients in this column can be interpreted as the effect of a single day of intervention on voters registered on that day, as a percentage of 2013 registration.

Finally, in column 5, we collapse the temporal element of our data to the polling station level. Summing across the entire intervention period decreases statistical power but simplifies the interpretation of results and takes away the possibility of incorrectly defining the treatment.27 This column reports the effect of the two-day intervention on total registrations at the polling station during the entire intervention period as a share of 2013 registered voters.

Table 2 demonstrates that localization drives most registration increases. This suggests that costs and convenience, not information, constitute the main constraint to registration. For ease of interpretation, we focus on column 5, the most conservative estimates.28 We find little evidence that SMS or canvassing substantively increased the number of registrations compared to control communities.29 In contrast, localization

22. In app. F, we show that the randomization procedure was successful in ensuring substantive balance between treatment groups.

23. The 10-day tail and the asymmetry across interventions, although preregistered and based on discussions with the IEBC, is admittedly ad hoc. In app. G, we present results across a range of specifications, which point to the same substantive conclusions. We find no evidence for additional registrations beyond the tail of the original definition.

24. In app. H, we present summary information at baseline.

25. This is our preregistered equation: \( y_i = \alpha + \beta_1 \text{Canvassing} + \beta_2 \text{SMS} + \beta_3 \text{Local} \times \text{Canonical} + \beta_4 \text{Local} \times \text{SMS} + \beta_5 + \gamma_i + \phi_i + \epsilon_i \), where \( y_i \) is the total number of voters registered at polling station \( i \) on day \( t \). Local, Canvassing, and SMS are dummy variables and equal to one if polling station \( i \) received the respective treatment on day \( t \), and zero otherwise. \( \phi_i \) is a block fixed effect; \( \gamma_i \) is a polling station fixed effect; \( \phi_i \) is a vector indicating the day of the week to account for weekly cyclical patterns that might affect registration, such as market days; \( \epsilon_i \) is an error term clustered at the polling station.

26. Note that we are not able to measure the number of individuals registered by the eligible voting age population, because these data are not available at the polling station level.

27. The polling station-level equation is \( y_i = \alpha + \beta_1 \text{Canvassing} + \beta_2 \text{SMS} + \beta_3 \text{Local} + \beta_4 \text{Local} \times \text{Canonical} + \beta_5 \text{Local} \times \text{SMS} + \phi_i + \epsilon_i \). This equation is identical to the equation in n. 25, except for dropping weekday indicator variables and polling station fixed effects.

28. There are some differences in the effect of canvassing between cols. 1–4 and that in col. 5. In cols. 1–4, the effect of canvassing is statistically significant but substantively quite small, while that in col. 5 is indistinguishable from zero. Similarly, cols. 1–4 suggest a small increase in registration due to canvassing, either in isolation or combination with localization. However, these effects are minor relative to the difference between interventions with and without localization.

29. In app. B, we give a summary of other studies exploring the impact of civic education interventions. First, there is much variation in the type of interventions studied. Second, the average participant’s length of exposure in the intervention we study here is shorter than the interventions.
increased voter registration as a share of 2013 voters by 2%. While polling stations assigned to the status quo (CVR) saw an increase in registered voters by 0.4%, polling stations that received localization saw an increase of 2%. The effect is substantial and statistically significant (p < .01). The combination of localization and SMS is especially effective at increasing registration, leading to a 2.4% increase relative to the status quo. The difference in voter registration between areas with localization and SMS and localization (or localization plus canvassing) is statistically distinguishable from each other (p < .05).

The combination of localization and SMS is especially effective at increasing registration, leading to a 2.4% increase relative to the status quo. The difference in voter registration between areas with localization and SMS and localization (or localization plus canvassing) is statistically significant (p < .01). These results suggest that SMS reminders of a local registration opportunity provide one cost-effective way to increase voter registration.

Table 2. Impact of Interventions on Voter Registration

<table>
<thead>
<tr>
<th></th>
<th>No. of Regs</th>
<th>No. of Regs by 2013</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td><strong>Canvassing effect</strong></td>
<td>.052***</td>
<td>.059***</td>
</tr>
<tr>
<td></td>
<td>(.018)</td>
<td>(.019)</td>
</tr>
<tr>
<td><strong>SMS effect</strong></td>
<td>.001</td>
<td>-.002</td>
</tr>
<tr>
<td></td>
<td>(.008)</td>
<td>(.010)</td>
</tr>
<tr>
<td><strong>Localization effect</strong></td>
<td>3.170***</td>
<td>3.163***</td>
</tr>
<tr>
<td></td>
<td>(.301)</td>
<td>(.305)</td>
</tr>
<tr>
<td><strong>Localization + canvassing effect</strong></td>
<td>3.630***</td>
<td>3.627***</td>
</tr>
<tr>
<td></td>
<td>(.263)</td>
<td>(.266)</td>
</tr>
<tr>
<td><strong>Localization + SMS effect</strong></td>
<td>4.705***</td>
<td>4.701***</td>
</tr>
<tr>
<td></td>
<td>(.387)</td>
<td>(.391)</td>
</tr>
<tr>
<td><strong>Control average</strong></td>
<td>.0437</td>
<td>.0437</td>
</tr>
<tr>
<td>Preregistered</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Block FE</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Polling station FE</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Day FE</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Controls</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Weights</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Observations</td>
<td>66,880</td>
<td>66,880</td>
</tr>
<tr>
<td>R²</td>
<td>.222</td>
<td>.254</td>
</tr>
</tbody>
</table>

Note. Clustered, robust standard errors in parentheses (cols. 1–4). Unit of analysis for cols. 1–3 is PS day, and outcome is absolute number of registered voters. Columns 4 and 5 divide the outcome by number of registered voters in 2013, measured at PS day level and PS level, respectively. For localization, localization + canvassing, and localization + short message service (SMS), treatment are the two days at which the intervention took place. For canvassing-only and SMS-only, treatment are those two days at which the intervention took place plus the 10 following days. The remaining days plus polling station days assigned to the control areas are status quo. Full experimental sample involved 1,674 polling stations, based on data provided by IEBC in 2016. However, six polling stations were dropped due to operational reasons (e.g., stations were retired from service for the 2017 elections or had zero registered voters in 2013). This leaves us with 66,720 (1,668) observations in col. 4 (5). Level of analysis is PS day. FE = fixed effects. PS = polling station.

* p < .1.
** p < .05.
*** p < .01.

30. By election day, we find that the intervention leads to a positive but insignificant increase in total registered voters at the polling station level (see section on downstream effects on registration, turnout, and vote choice).
registration. However, information only appears to work in the presence of difficulty-reducing interventions like localization.

**Effect heterogeneity in poverty, distance, and population density**

The section on electoral administration and barriers to voter registration in Kenya revealed considerable heterogeneity across polling stations in poverty, distance, and population density. Previous studies in the developed world have found that socioeconomic status is positively correlated with political participation and interest in general (e.g., Verba, Scholzman, and Brady 1995), and voter registration in particular (Nickerson 2015). Distance from the polling place similarly hinders participation (Brady and McNulty 2011; Dyck and Gimpel 2005). Our blocked design makes it possible to examine the heterogeneous effects of the interventions on registration as a function of levels of poverty, distance to registration opportunities, and population density.

The top row of figure 2 shows the results of column 5 of table 2 for different levels of poverty. We calculate the average level of poverty for each of the 279 blocks, and then subset the blocks into five equally sized groups based on these block-level means. The blocks are organized from left to right increasing in poverty. The leftmost panel presents the results from the richest quintile of blocks, while the rightmost panel focuses on the poorest quintile. We highlight two main results. First, localization has a positive impact in all five subgroups. Second, the impact of localization is higher in poorer areas. The impact of localization is six times as large in the poorest quintile relative to the richest quintile (4.39% vs. 0.73%). This suggests that poorer communities in particular benefit from localization.

We find similar dynamics for distance and population density. The center row in figure 2 presents the effects of the interventions by quintiles of distance to the registration office, where the leftmost panel presents the results focusing on the quintile of blocks closest to the registration office, while the rightmost panel focuses on those farthest away. Similar to poverty, localization has a positive effect across all panels. Moreover, the effect of localization in the most distant polling stations is considerably larger as that in more proximate polling stations (2.26% vs. 1.59%).

Finally, the bottom row of figure 2 presents the results by quintiles of population density. The leftmost panel presents results for the densest quintile of blocks, while the rightmost panel focuses on the sparsest quintile. Again, localization has a positive effect across all panels. Furthermore, the effect in sparsely populated areas is almost six times larger than that in densely populated areas (5.34% vs. 0.99%).

These results suggest that registration is a much bigger problem among the poor, as well as those living in distant and sparsely populated areas. Changes in status quo registration policy are thus likely to have the most significant impact on citizens in such communities.

**Downstream effects on registration, turnout, and vote choice**

While localization has positive effects during the intervention period, what are the downstream effects of localization on election day? Increases in voter registration may increase turnout (Nickerson 2015). The interventions may also change the distribution of candidate support at a polling station by registering citizens with different preferences (De Kadt 2017). On the other hand, the effects presented thus far may be due to temporal displacement, that is, individuals who registered to vote due to the interventions would have registered later in the intervention’s absence.

Our intervention period represents a sliver of the entire five-year cycle of voter registration. Thus, the two-day interventions may not be of sufficient magnitude to affect election day outcomes. To address this, we combine polling stations receiving any localization—which induced virtually all of the total estimated effects during the intervention period—and compare them to polling stations that did not receive localization.

Figure 3 examines the effect of localization across all six races on election day outcomes: total registered voters, total turnout, turnout rate, vote margin, and preference diversity. Bars indicate 95% confidence intervals, and the dependent variables are standardized to enable comparison across outcomes. First, we explore the overall number of registered voters for the 2017 election relative to that in 2013. Kenyans have to register only once and the effects are thus the same across races. Relative to control, localization increased 2017 registration relative to 2013 by 1.4% or 0.052 standard deviations. Thus, localization temporally displaced the registration decision of some voters, causing them to register earlier than they otherwise would have. Relative to the 2% effect at the end of our intervention period, the estimated election day effect of localization becomes 1.4%. This implies that 0.6% of the overall increase consists of individuals who would have registered in the absence of the intervention. While these findings are consonant with our expectations, they are not statistically significant.

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32. These results contrast with De Kadt (2017), who shows that the expansion of polling stations in South Africa benefited richer citizens. We discuss this difference in more detail below.

33. This experiment was not originally designed to detect downstream effects; thus, the analyses in this section are not preregistered.
Second, we explore voter turnout and turnout rates. Because Kenyans vote for all six races at the same time (using separate ballots), the effects are again very similar across races. On average, localization increased voter turnout by around 0.04 standard deviations relative to control polling stations. However, localization depressed turnout rates, measured as the number of votes cast divided by the number of registered voters in 2017, by an average of 0.03 standard deviations. This result is consistent with the
notion of a “double hurdle”: localization relaxed constraints related to registration but not voting. Again, these results remain statistically insignificant.

Finally, we explore the impact of localization on vote margins and vote fractionalization.\textsuperscript{34} We find that localization led to a 2.8\% (0.09 standard deviation) decrease in vote margins at the polling station level in the women’s representative race ($p < .01$). This suggests that improving access to voter registration may increase political competition. Moreover, localization increased the diversity of expressed political preferences in the women’s representative race by 0.014 ($p < .05$) or 0.06 standard deviations. In the context of a tight two candidate race, this is equivalent to adding a third party candidate receiving 1.4\% of the vote. The increase in vote fractionalization (and decrease in vote margins) means that localization increases political competition by recruiting new voters that are more likely to support secondary candidates than voters recruited via status quo processes.\textsuperscript{35} The statistically significant findings, however, are confined to the women’s representative race. On the one hand, this makes theoretical sense: if the interventions were to have any effect on the kind of preferences recruited to the voter register, it would likely be preferences over newer, less-salient, and down-ballot contests like the women’s representative and senator. Both of these contests exhibit similar point estimates for vote margin and preference diversity. On the other hand, figure 3 presents 30 estimates, and one would expect, given the 95\% confidence intervals, just under two of 30 tests to be significant, simply by chance. Thus, we remain somewhat circumspect on the substantive interpretation of the effects of localization on the election.

\section*{DISCUSSION AND CONCLUSION}

In this article we explore two questions related to election administration in fledgling democracies. First, do status

\textsuperscript{34} Vote margin is measured as the percentage of votes for the winner minus the percentage of the votes for the second place candidate. Vote fractionalization is measured as one minus the sum of candidates’ squared vote shares.

\textsuperscript{35} As a reviewer noted, this may not necessarily be a normative good if it leads to more spoiler candidates.
quod policies for voter registration create systematic barriers to political participation? Second, if such barriers exist, how can they be overcome? We partnered with the Kenyan electoral commission to implement a large-scale experiment providing citizens with information about and access to voter registration to address these questions. We find little evidence that providing knowledge or reminders about registration encourages participation. Rather, we find that material costs related to registration functionally disenfranchise poorer, more distant, and more sparsely populated communities. Our study is perhaps most similar in spirit to Nickerson (2015), who finds that average registration increases by about 4.4% in response to a doorto-door registration drive. The magnitude we report is approximately half of this, although the effect size varies by context. The poorest subgroup in Nickerson (2015) reported a 7.3% increase in registration, relative to a 4.4% effect in the poorest block in this study. Interestingly, Nickerson notes that the average effect of 4.4% arises "in the contemporary [United States] setting where voter registration is moderately easy" (95). This raises the question as to whether the relatively modest results we observe (relative to Nickerson 2015) might be driven by the more onerous nature of Kenya’s voter registration process and the environment in which it is carried out.36

Our results have important implications for electoral administration and democracy promotion in the developing world. While there is limited evidence that civic education improves registration or turnout, existing research in lower-income contexts does find that civic education may improve knowledge and other forms of political participation.37 Finkel, Horowitz, and Rojo-Mendoza (2012), for instance, find that in-person civic education programs in Kenya led to increases in a broad measure of local—but not national—political participation. Similarly, Mvukiyehe and Samii (2017) find positive effects of experimentally allocated civic education interventions on both self-reported and behavioral measures of participation in Sierra Leone. Bratton et al. (1999) find similar results in Zambia. We contribute to this body of work by juxtaposing the benefits of civic education with alternative interventions on voter registration, finding that the largest benefits to voter registration may lie in changing convenience and cost, not providing civic education. One implication is that election administrators and donors should focus their efforts on cost-reducing measures like the localization of registration examined in our study. Doing so may increase political participation and, as a result, electoral competition.

Moreover, the localization results complement existing work showing how accessibility affects political participation (Bhatti 2012; Brady and McNulty 2011; McNulty et al. 2009). Our results present experimental evidence that existing voter registration policies may functionally disenfranchise certain populations in the developing world by making it more difficult for some citizens to register. While this effect may not be intentional, a lack of viable registration options disadvantages citizens living in poorer, more remote, and more sparsely populated areas. In some respects, these results contrast with the careful observational analysis in De Kadt (2017), which finds a 3%-5% increase in voter turnout across 15 years due to the expansion of polling stations in South Africa. Citizens in higher socioeconomic groups were particularly responsive to changes in access, in apparent contradiction to our results where citizens from poorer areas were more responsive. Two differing aspects of the studies may explain this seeming disparity. First, De Kadt (2017) studies how the addition of new polling stations generates participation, whereas our work examines random localization at existing polling stations. Thus, while we can estimate an average treatment effect representing a deviation from an existing policy, the estimates in De Kadt (2017) represent an estimate of an expansion policy which may serve urban (and richer) citizens with greater demand for additional polling station capacity. Second, the interventions tested here focus on unregistered citizens near existing polling stations, while the De Kadt (2017) study engages with a different population entirely: both registered subpopulations whose access was eased, as well as unregistered populations engaged by newly established stations. As a result of these differences, we are hesitant to suggest that our results contradict those in De Kadt (2017) but rather speak to the complicated nature of election administration as it relates to political behavior.

One objection to improving access to registration is that it is not cost effective. Localization, however, is relatively cost-effective. An average cost of $25 per localization day translates into a cost per registration of $8.33 (average) or $3.57 (highest poverty block). This compares favorably to status quo costs per registered voter of US$13.74 in an election year, with nonelection years significantly higher due to lower registration numbers but identical fixed costs (IREC 2008, 44). Adding SMS to localization improves this cost comparison, given the relative effectiveness and low cost of SMS bulk messaging relative to

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36. Appendix J reviews related work on voter registration and GOTV experiments, and discusses the magnitude of our results relative to the broader literature. It is also worth noting that, in contrast to Nickerson (2015), the localization intervention did not use door-to-door registration. Localization thus decreased the cost of the intervention, but did not bring it to zero.

37. See app. B for a careful exposition of other civic education studies. A robust literature from high-income countries suggests that face-to-face canvassing and voter education cause increased participation in those contexts as well (e.g., Braconnier et al. 2017; Green and Gerber 2015).
canvassing, bringing the electoral commission’s registration costs well within sustainable per registration cost targets (IFES 2005, 173). Moreover, the estimates assume hiring additional IEBC personnel; implementation with existing employees carrying out regular localization may drive costs even lower.

This study is not without limitations. Perhaps most pressing, the present study examines the effect of one relatively light-touch form of civic education, face-to-face canvassing, which has been effective in other contexts. In terms of Harder and Krosnick (2008), it may be that the reduction in difficulty due to localization may vastly outstrip the increases in citizen ability due to our civic information treatment, and that stronger versions of the latter may generate a detectable effect. Future work might consider variations on the civic education treatment studied here, varying the content of the educational material and the depth or frequency at which it is delivered. Second, our blocking strategy focuses on poverty, one important element of economic context. Recent work demonstrates that also another important element, economic inequality, may function to condition political preferences (Sands 2017). Future work might examine whether the effects of inequality on political preferences extend to formal political participation. Finally, voter registration in fledgling democracies may be conditioned by ethnic or religious contexts, local institutions, and networks. Future work could explore more complex designs aimed at differentiating effects due to social-contextual factors from those due to geography.

Finally, to what extent do the results from this study generalize beyond our study site? There are good grounds to expect external validity with respect to other polling stations in the seven counties since our polling stations were drawn randomly from larger populations. In appendix K, we compare the national distribution of poverty and population density with that of our sampled polling stations. Although the sample excludes the extreme polling stations, it overlaps with the vast majority of the national distributions. Furthermore, our results suggest that scaling-up the policy to even poorer and more sparsely populated areas may lead to even higher registration gains. Would our results replicate beyond Kenya?

We have little direct empirical leverage over this question. It is worth noting, however, that the environment of our study is similar to that found in many developing countries on some key dimensions. In most developing countries, policy decisions related to election administration are centralized; state capacity and bureaucratic outreach remain weak; and citizens are poor, living far away from registration opportunities or in sparsely populated areas. For example, among the 46 countries classified by the World Bank as sub-Saharan Africa, Kenya ranks as only the 30th least densely populated country. In other words, the factors central to this study are clearly a more general phenomenon in the developing world.

ACKNOWLEDGMENTS

We thank Cornelius Lupao, Hannah Melville-Rea, and Cole Tanigawa-Lau for research support. We thank Jim Alt, Daniel Posner, Kerstin Fisk, Guy Grossman, Macartan Humphreys, Ryan Moore, Vincent Pons, and Kelly Zhang for comments. Thanks to participants attending presentations at EPSA, APSA, NYU Abu Dhabi, and Leuven University for feedback. This research could not have been carried out without the consistent support of Kenya’s Independent Electoral and Boundaries Commission, particularly Andrew Limo, Decimah M’mayi, Michael Oyalo, William Kahindi, the Regional Election Coordinators, and the Constituency Election Coordinators.

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38. For the cost of one canvasser sent to a polling station, we could send over 2,000 SMS messages, which would cover almost four average sized polling stations.

39. In addition, these estimates are likely an upper bound, given that our intention-to-treat analysis probably underestimates the actual effect of localization.

40. We do not have data on the locations of the constituency offices in constituencies not included in our study. Thus, we do not present sample-to-population comparisons to our third blocking variable, distance to the constituency election office.

41. Evrensel (2010) mentions similar voter registration problems related to cost/distance-based disenfranchisement in Ghana (pp. 2, 14), DRC (pp. 58, 81, 91, 95), Mozambique (pp. 224, 228), and South Africa (in 2004; p. 345), suggesting that the broader problems explored here are not uncommon.

42. These are 2017 population density estimates. See https://data.worldbank.org/.