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Deterring or Displacing Electoral Irregularities? Spillover Effects of Observers in a Randomized Field Experiment in Ghana

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This article studies the effect of domestic observers deployed to reduce irregularities in voter registration in a new democracy, and in particular, the response of political parties' agents to these observers. Because political parties operate over large areas and party agents may relocate away from observed registration centers, observers may displace rather than deter irregularities. We design and implement a large-scale two-level randomized field experiment in Ghana in 2008 taking into account these spillovers and find evidence for substantial irregularities: the registration increase is smaller in constituencies with observers; within these constituencies with observers, the increase is about one-sixth smaller on average in electoral areas with observers than in those without; but some of the deterred registrations appear to be displaced to nearby electoral areas. The finding of positive spillovers has implications for the measurement of electoral irregularities or analysis of data collected by observers.

Following the third and fourth waves of democratization of the late twentieth and early twenty-first centuries, an overwhelming number of countries in the world today elects its leaders. In these new democracies, popular elections are frequently marked by fraud and irregularities (Simpser 2010), which affect public confidence in democracy and regime legitimacy (Birch 2008; Elklit and Reynolds 2002; Rose and Mischler 2009), political participation (McCann and Domínguez 1998), and protest and political violence (Eisenstadt 2002). For both historical and contemporary cases, the question of how informal practices of ballot stuffing, registration fraud, and other electoral malpractices are eliminated is now central to the study of democratization (Ziblatt 2006), which had earlier focused on changes to formal rules like the extension of the suffrage or the development of responsible and limited government. An emerging body of scholarship on democratization and new democracies argues that the extent of electoral fraud is affected by political competition (Lehoucq 2003; Simpser 2010), electoral rules (Birch 2007; Lehoucq and Molina 2002), socioeconomic inequality (Ziblatt 2009), the quality of the electoral management body that organizes and conducts the elections (Elklit 1999;

Elklit and Reynolds 2002; Hartlyn, McCoy, and Mustillo 2008; Pastor 1999), and scrutiny by international election monitors (Hyde 2007; Kelley 2012).

The fundamental difficulty with the study of election fraud is its measurement—it may take many forms and those involved typically wish to hide these illicit activities. Scholars generally rely upon assessments by election observers to measure electoral fraud for quantitative cross-national studies and use media reports and petitions filed by aggrieved parties for single-country studies (Lehoucq 2003). But these measures are generated by participants with different interests, expectations, and standards across elections (Kelley 2012), which raises concerns about consistency and bias. Moreover, politicians may respond to the possibility of detection by observers or media by engaging in fraud at alternative locations or earlier stages of the electoral process that are less likely to be detected (Bjornlund 2004; Carothers 1997), much as police and other crime-fighting measures have spillover and displacement effects on criminal activity (Bronars and Lott 1998; Di Tella and Schargrodsky 2004). Consequently, current measures used in cross-national studies may underestimate the extent of electoral fraud in new democracies, including those that appear to have fairly clean elections. This underlies

the need for substantial caution about the robustness of empirical findings in this emerging large-*n* literature (Birch 2007).

This article studies empirically the strategic response of political parties to civil society actors' efforts to detect and deter misconduct *ahead* of a closely contested election in a new democracy. More specifically, we design and implement a randomized field experiment to examine the causal effect of domestic election observers on the extent of irregularities in voter registration conducted over a 13-day registration period in advance of the 2008 Ghanaian general elections. We directly address the aforementioned measurement problem and the violation of the stable unit treatment value assumption (SUTVA, Rubin 1978) implied by the possible displacement of irregularities using a two-level randomization design. This design enables us to detect, and indeed we find, localized and general spillover effects that are consistent with evasive responses by the political parties to the observation effort of the Coalition of Domestic Election Observers (CODEO) in Ghana. Rather than deterring irregularities entirely, observers *displace* a substantial portion of irregularities to nearby unobserved registration centers in a pattern consistent with communication among political party agents. The effects of these observers and the estimated extent of irregularities are substantial, buttressing Birch's (2007) concerns.

We focus on voter registration for two reasons. First, problems with voter registration are quite common in transition elections and new democracies, and this can create significant doubts about electoral outcome and the legitimacy of the new government. The 1991 general elections in Nepal, the first free elections in that country in over 30 years, were held using a register in which the number of registered voters exceeded census estimates by about 10–15% (Gaige and Scholz 1991, 1056). Similarly, the Philippine general elections in 1998 were run by an electoral commission that refused to “reorganize old voter lists or issue identification cards, thus leaving the door open for so-called “flying voters” (i.e., those who vote more than once)” (Case 1999, 474). Substantial problems with the voter registration process and the resulting voters register also marred the 1993 elections in Senegal. There were numerous claims of discrimination against opposition supporters in the provision of voter cards and an estimated 30–50% of voter cards had factual errors which could prevent people from voting. Moreover, “public perception” that the incumbent government had abused the system of documents that allowed people whose

names did not appear on the register to vote “was undoubtedly the single most harmful issue in terms of eroding public confidence in the integrity of the elections” (Villalon 1994, 178).

In more serious cases, voter registration problems may cause elections to be delayed and possibly not held at all. Accusations of fraud and violence around voter registration in 2008, for example, eventually led to the postponement of parliamentary elections in Yemen from 2009 to 2011 (AFP 2009; *Yemen Times* 2009). Similarly, the election crisis in Côte d'Ivoire in late 2010 was presaged by the inability of the government and opposition to agree to procedures for voter registration. This led to the dismissal of both the Electoral Commission and the government, a delay of the highly anticipated first election since the end of the civil war, and deadly demonstrations (RFI 2010; Zamblé 2010).

Second, political parties have strong incentives to inflate the voters register with their own supporters, even where they are unable to fabricate elections results outright or to widely intimidate voters and opponents, which is precisely where the election would likely be characterized as “free and fair.” Political parties can skew the results in their favor on election day by enabling multiple-voting or adding premarked ballot papers to ballot boxes, without obviously pressuring a voter or restricting his access to a polling station. The risk to this strategy, however, is that if the number of votes cast as a proportion of registered voters appears suspiciously high, public scrutiny and legal and political challenges will follow. To benefit from the extra votes then, a party must ensure that extra names appear on the voters register. Consequently, incentives to rig elections become incentives to inflate the voters register, and moreover, to evade the scrutiny of observers while doing so. Organized political parties should then have party agents avoid the registration centers where observers are located and instead try to inflate the register at nearby unobserved registration centers.

Our study is sited in Ghana in sub-Saharan Africa because it shares with many other partial democracies political and institutional characteristics that are expected to affect politicians' incentives to engage in electoral malpractices (Birch 2007; Hartlyn, McCoy, and Mustillo 2008).¹ It has a majoritarian electoral system and an electoral commission that is officially independent but underresourced. Like many

¹What we label partial democracies are sometimes called hybrid regimes, semidemocracies, and anocracies, among other terms (Collier and Levitsky 1997; Epstein et al. 2006; Schedler 2002).

partial democracies, Ghana is a rapidly urbanizing developing country with a large, poor, rural population, where resources are concentrated in the state—conditions that are frequently associated with vote buying and other electoral malpractices (Kitschelt and Wilkinson 2007; Stokes 2005).

Furthermore, Ghana has significant experience with both international and domestic election observers who, as in other countries, have historically focused on election day activities and may have pushed malpractices by political parties to the preelection stage. Indeed, there is *prima facie* evidence of inflation of the voters register. In 2008, the Electoral Commission expected to register 800,000 voters, the estimated number of citizens who had attained voting age since the previous registration. However, the Electoral Commission registered nearly 2 million new voters, a figure significantly greater than the vote margin in the previous presidential election and for a provisional total number of registered voters greater than the estimated adult population of Ghana. Some but not all of these unexpected 1.2 million registrations were people mistakenly re-registering instead of requesting a replacement for a lost voter ID card. However, in several constituencies, the two main political parties traded accusations of transporting supporters to have them illegally register to vote (Boateng 2008).

Our contribution is threefold. First and most immediately, we shift the focus in this literature on electoral misconduct to the *preelection* stage. We demonstrate and quantify sizable irregularities in voter registration in a country that is considered a “model” new democracy 16 years removed from its transition from autocratic rule. To our knowledge, our work is the first large-scale experimental study to examine preelection irregularities and to work with domestic election observers. It complements related work by Hyde (2007) with election day international monitors and the extensive qualitative reporting on election fraud by domestic and international election observers supported by organizations such as the Carter Center, National Democratic Institute, and the European Union.² Our work also adds to the literature on statistical methods to detect problems with election results (Mebane 2006; Myagkov, Ordeshook, and Shakin 2009).

²The National Democratic Institute has monitored more than 270 elections (<http://www.ndi.org/content/elections>), and the Carter Center and EU have both observed more than 60 elections (<http://cartercenter.org/peace/democracy/observed.html>; http://ec.europa.eu/external_relations/human_rights/election_observation/index_en.htm).

Second, we consider explicitly how political parties are organizations that cover a wide geographical area and create connections across political units, and we study the implication that interventions on illicit political activities will have spillover effects. We find evidence for such interference across spatial units, but also conclude that the spillovers are limited by geographical distance. Our work adds to the handful of recent studies in political science that have explicitly considered spillovers, such as Nickerson’s (2008) experimental study of peer effects on turnout in an American election.

Third, this research contributes to a growing body of scholarship that uses randomized field experiments to study how democracy works in practice in specific developing countries and clarify debates and generalizations from observational studies (Collier and Vicente 2010; Humphreys, Masters, and Sandhu 2006; Olken 2010; Wantchekon 2003). Our experimental study reinforces Birch (2007)’s concerns about the robustness of findings from large-*n* empirical studies that measure fraud using observer reports. These include most obviously studies on the causes and consequences of electoral fraud, but also those on the relationship between election quality and democratic development (Lindberg 2006). Our work also suggests greater circumspection about the efficacy of observers in reducing electoral irregularities (Hyde 2007; Kelley 2012) and points to future research on how fraud is organized and on the relationship between preelection fraud, election day problems, and postelection rigging.

We proceed by first presenting our hypotheses on observers, party agents, and voter registration. We describe the voter registration process and the 2008 general elections in Ghana, then present the experimental design, data, and analysis.

Observers and Party Agents

What is the effect of domestic observers on the behavior of political party agents and the extent of irregularities in voter registration? The basic premise is that political parties want to win the election and want to appear to do so cleanly. If the parties intend to inflate the vote total in their favor on election day, they may wish to inflate the register with their supporters and at the same time do not want their agents to be caught doing so. Therefore, observers should, in fact, *deter* party agents from organizing logistics for fraudulent registrations at the registration centers that the

observers visit. We call this causal effect of observers on registration at the registration centers to which they are deployed the *primary* effect. This logic implies the following hypothesis: *A registration center with an observer should have a lower increase in registration than registration centers without observers (negative primary effect).*

However, two additional effects are possible. Political parties are concerned with voting and voter registration over a wide area composed of many registration centers and political party agents often transport their supporters in vans and buses. Party agents can often communicate with one another and travel fairly easily to avoid a particular registration center with an observer. However, time and resource constraints imply that not all deterred registrants would be relocated to alternative registration centers and whether there is a registration center nearby would affect the extent of this relocation. Consequently, some portion of the extra registration deterred by an observer may simply be *displaced* to nearby registration centers, which we call a *localized spillover* effect. This implies a second hypothesis: *A registration center with a nearby observed registration center should have a larger increase in registration than registration centers far away from observed registration centers (positive localized spillover effect).*

The second possible effect is that observers may deter extra registrations in the constituency overall, not just at the registration centers to which they are deployed. The presence of these registration observers may become widely known and give the impression that the constituency overall is being observed, so that observers may also have a negative *general spillover* effect.

We interpret the *primary* effect as a *lower bound* on the extent of irregularities in voter registration enabled by political party agents, since observers likely do not deter all problems at the locations they visit. However, observers might affect registration through alternative mechanisms that would complicate this interpretation. First, citizens who know that an observer is present at their local registration center may feel less intimidated by possible trouble and become more likely to register to vote. This would attenuate the primary effect of an observer on the visited registration center, and consequently, also implies that a registration center with a nearby observed registration center may not experience a lower additional increase in voter registration. Second, electoral officials might also become either more efficient in carrying out their duties or more diligent and slow down the registration process. If the

effect of observers comes through the influence on electoral officials, the expected primary effect of observers is unclear, and as in the case of influence on citizens, a registration center with a nearby observed registration center should have a lower additional increase in voter registration. However, electoral officials who see registration observers at a registration center could report this up the chain of command, affecting the behavior of their counterparts at other registration centers, so that there may be a general spillover effect. We check these alternative mechanisms following our main analysis.

Voter Registration and the National Election in Ghana 2008

Ghana is a former British colony and is an ethnically and religiously diverse country of approximately 23 million on the Gulf of Guinea in West Africa, bordered by Côte d'Ivoire, Togo, and Burkina Faso. The Akans, who are concentrated in the more prosperous southern parts of the country, are the largest ethnic group in Ghana, but they are composed of many distinct subgroups that together do not comprise a majority of the population.

Ghana has a history of cycles of coups d'état and military rule, but it has held regular, competitive elections every four years since its transition to democracy in 1992. Direct elections are held concurrently for president and a unicameral national parliament, which is composed of 230 members elected by plurality from single-member districts. The winning candidate for the presidency must win a majority of votes cast, with a run-off election between the top two vote getters if no candidate wins a majority in the first round. The then incumbent military ruler, Flt. Lt. Jerry Rawlings, was elected the first president of the Fourth Republic in 1992 and then reelected in 1996 on the platform of the National Democratic Convention (NDC). Rawlings left office in 2000 after the constitutional limit of two terms, and his party's presidential candidate was defeated by John Kufuor of the New Patriotic Party (NPP) in a very close election. Kufuor was reelected in 2004, and there was no question that he would leave office in 2008 following his two terms. Since 1992, the NDC and NPP have been the two major parties in Ghana and both parties compete in local-level and national-level elections throughout the country. The NDC and NPP consider themselves left-leaning and right-leaning, respectively, and each is strongly identified

with regional and ethnic bases (Lindberg and Morrison 2005; Nugent 2001).

In order to vote in Ghana, a voter must go to the particular polling station associated with his residence and present his voter ID card which should have a photograph taken at the time of registration if a camera was available; no other form of identification is required. The electoral official compares this card with the information and photograph printed on the voters register before allowing the person to vote. While international borders are closed around election day to prevent Togolese, Burkinabé, and other foreigners from entering the country to vote, internal roads are left open, and someone who wishes to vote in multiple locations on election day could easily do so as long as he is registered at those multiple locations.

Citizens of Ghana may register to vote only during designated registration periods, only in person, and only at particular registration centers associated with the polling station and electoral area for their residence. Someone who wishes to register to vote must declare his or her name, address, parents' names, and home area, and the electoral official will fill out this information on a registration form. The registrant is photographed if a camera is available, and the photograph is attached to the form, covered by a sticky plastic sleeve and becomes the official voter ID card.³ Like the United States and several other former British colonies, Ghana does not have a national ID card system and electoral officials have no means to check a registrant's identity, so that it is fairly easy to declare false information. Electoral officials may remind the person registering that the penalty for giving false information or registering multiple times is up to a year in prison, but almost no one is ever prosecuted for false registration.⁴

Voter registration was delayed several times due to a controversy around a summary of the 2006 voters register, as well as due to supposed delays in procuring equipment, release of funds from the government, and hiring of qualified temporary staff.⁵ Voter registration finally began on 31 July 2008, with only one day's advance notice. Although each of the approximately 4,800 electoral areas in the country

was expected to have a registration workstation, only about 2,500 workstations were available. The regional Electoral Commissioners distributed equipment and registration materials at each regional headquarters to district-level Electoral Commission officials, who transported them and distributed them at the district offices to temporary staff hired by the district office. The district-level Electoral Commission officials then drew up the plans for which electoral areas would have registration centers on which days. Consequently, no advance information was available centrally on where the mobile registration workstations would be located on which dates. In at least one region, the distribution of equipment and materials among the numerous districts was haphazard and did not follow any formula that considered the size of the districts.⁶ As in previous voter registration exercises, the political parties actively ferried people to registration centers.⁷

On the last day of the scheduled 11-day registration period, the Electoral Commission extended registration by two days due to widespread reports of shortages of materials and equipment. The Electoral Commission of Ghana then processed all the registration forms at its headquarters in Accra and produced a provisional voters register. By law, this provisional register must be made available during an exhibition period, during which an official from the Electoral Commission sits with the provisional voters register at particular locations (usually one central location in each electoral area) so that voters can check for their names. Objections to any names on the register may be lodged with the Electoral Commission at this time. Approximately 0.4% of new registrations were challenged in 2008, which is 10 times the rate of challenges against new registrations in 2004, and this provisional list was cut down to a final list of approximately 12.5 million voters. In Ghana, the voters register is vetted for deceased voters or others who should not be on the register only during this period between production of the provisional and final voters registers.

³The Electoral Commission describes this process as "lamination" but the plastic sleeve is not heated or melted; it is only self-adhesive.

⁴A prominent exception is the case of Pius Opoku Boateng, who came under heightened scrutiny as the NDC parliamentary candidate for Kwabre West constituency and was sentenced to 12 months in prison for double registration (Alhassan 2008).

⁵Meeting with Deputy Electoral Commissioner David Kangah, in Accra, Ghana, July 2008.

⁶Observation by research assistant at regional Electoral Commission headquarters, 30 July 2008.

⁷An NDC agent and a taxi driver independently reported to a domestic registration observer in Trobu-Amasaman constituency in Greater Accra Region that, prior to the observer's arrival, NPP pick-up trucks conveyed people from nearby villages to the registration center. Similarly in Ningo-Prampram constituency in Greater Accra Region, a domestic registration observer reported that both NDC and NPP were bussing people to registration centers.

The general elections took place as scheduled on 7 December 2008, but no candidate for president won a majority of votes cast. The presidential run-off election took place as scheduled on 28 December 2008 in all areas except opposition NDC-leaning Tain constituency, where there was a shortage of ballot materials. The incumbent NPP initially sought a court injunction to stop this last election in Tain, but withdrew the challenge and boycotted the election on 5 January 2009. The opposition NDC won the presidency, with a final official vote margin of less than 50,000 votes.

Research Design

In consultation with the Coalition of Domestic Election Observers (CODEO), we selected four of the 10 regions of Ghana for this study: Ashanti, Brong Ahafo, Greater Accra, and Northern Regions. The leadership of CODEO, an umbrella group of 34 major and many smaller civil society organizations coordinated by the Ghana Center for Democratic Development (CDD-Ghana), was willing to adjust some of their plans in order to learn about the effectiveness of their activities. CODEO did not place any restrictions on our choice of regions, and we selected these four regions in order to cover a wide range of constituencies within our resource constraints, including several incumbent NPP strongholds, one in which the 2004 parliamentary contest was won by the NPP candidate by a 69-point margin; competitive constituencies; and several opposition NDC strongholds, one in which the 2004 parliamentary contest was won by the NDC candidate by a 50-point margin. Approximately 54% of the Ghanaian population lived in these four regions as of the last census in 2000, and they contain 116 of the 230 constituencies and 2,204 of the approximately 4,800 electoral areas (ELAs, which are sub-units of constituencies).

Randomization

Because observers tend to go to locations that are more accessible, are conveniently located to the last observed location, and are likely to have problems with voter registration, it is difficult to determine what portion of any observed difference in voter registration outcomes should be attributed to the presence of observers and what portion to differences in underlying characteristics, even in the absence of

spillover effects. We substantially reduce concerns about confounding by adopting an experimental approach and randomizing which electoral areas should be observed.

In order to examine spillover effects, we used a two-stage randomized design with blocking in the first stage in a design similar to Miguel and Kremer (2004). As noted earlier, these spillovers are forms of interference across units, a violation of the stable unit treatment value assumption (SUTVA) (Rubin 1978). A simple comparison of means of registration between treatment and control electoral areas will therefore be a biased estimate of the primary effect of a registration observer, even if the assignment of observers to registration centers is randomized. Consequently, we design our experiment to explicitly take into account the possible strategic response of political parties in a way that allows us to detect both localized and general spillover effects.

First, within each region, we divided constituencies into blocks according to the difference in vote share won by the NPP candidate and the NDC candidate in the 2004 parliamentary elections. Parliamentary constituencies are political units which are not the same as administrative districts for which government data is made public, and at the time of the experiment, population and other data were not available at the constituency level. Consequently, we blocked only on the 2004 elections results in order to improve the efficiency of our estimates. Within each block, one constituency was randomly assigned to be a treatment constituency and two others to be control constituencies, so that there are competitive constituencies as well as stronghold constituencies for each party among both our treatment constituencies and our control constituencies. Although all regions were available for randomization, as CODEO's mission is to organize observers to improve the quality of Ghanaian elections, a small number of constituencies in some regions were not available for randomization and exposure to the two-thirds probability that they would not be observed. Those constituencies designated neither treatment nor control were made available for visits by other CODEO observers not participating in the experiment.

In the second stage, we randomly selected approximately 25% of the electoral areas in each of the treatment constituencies to be visited by registration observers, which generates random variation in the number of treatment electoral areas in the neighborhood of an electoral area, conditional on the total number of electoral areas in the neighborhood. Although we were aware that there would be fewer

registration workstations than electoral areas and that some electoral areas would share workstations, we conducted our randomization over the list of electoral areas from the 2006 election because the location of the registration workstations were to be determined by local Electoral Commission officials and unavailable ahead of time.

Our randomization procedure classifies electoral areas into one of three groups: control electoral areas in 26 control constituencies, control electoral areas in 13 treatment constituencies, and treatment electoral areas in those 13 treatment constituencies. In the estimation we take into account this design through the inclusion of the full set of block fixed effects and correction of standard errors for clustering at the constituency level (Bruhn and McKenzie 2009; Duflo, Glennerster, and Kremer 2007).

Registration observers were recruited from CODEO member organizations that would ordinarily field registration observers, were trained together by one of the authors and CODEO leaders, accredited by the Electoral Commission as regular observers, and deployed at the same time. The registration observers were instructed to go to their assigned constituencies, find out where and when the registration centers would be open, and then visit unannounced only registration workstations in the electoral areas on their list. They were instructed to visit all the electoral areas on their list at the beginning of the registration period before revisiting the registration centers once (mostly in rural areas) or twice (mostly in urban areas) more during the registration period.

Registration observers stayed at each registration center for about 1–2 hours on each first visit and up to a full day in the later visits. In Ghana, observers are not permitted to assist or interfere with the proceedings, although they may interact with the party agents and electoral officials at the registration center. The registration observers were asked to fill out a one-page checklist with questions such as whether the registration center was open upon the observer's arrival, whether it had a workstation, whether it was well-marked and easy to reach, whether there had been any violence, whether the registration center had been forced to close at any time, whether any people the observer thought were ineligible (e.g., underage) had been registered, and whether any people the observer thought were eligible had not been permitted to register. Registration observers were directed to fax back these checklists to the CODEO secretariat every couple of days or whenever they were in an urban area and had access to

telephones/fax lines.⁸ CODEO officials read these reports and released one general press statement during the registration period and one at its conclusion.⁹

Data

We use a combination of data from our experiment and official sources for our analysis. We gained access to the official number of registered voters at the polling station level in 2004 and 2008 and compiled these into electoral area-level figures. We use whether an observer filed a report for the registration center for a particular electoral area as our measure of whether that electoral area was visited by a registration observer.¹⁰

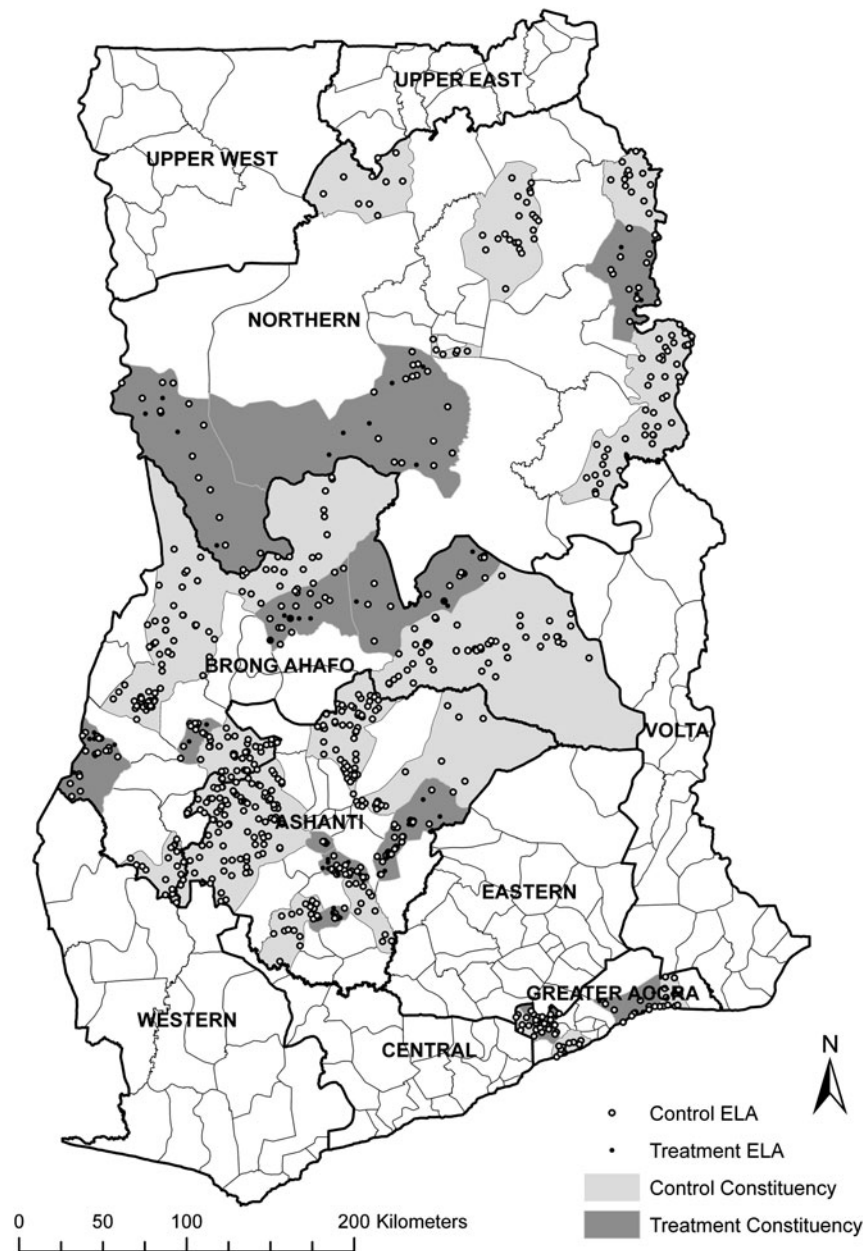
We digitized the Electoral Commission's map of constituency boundaries. We also geocoded the 868 electoral areas in our experiment as points by comparing the names of the polling stations located within those electoral areas with publicly available printed and digital maps, data from the 2000 population and housing census, coordinates from GPS we deployed with some of the registration observers, and on occasion, information given by local electoral officials (Figure 1). We used ArcGIS to calculate the distance between all pairs of electoral areas and construct variables that indicate the number of electoral areas in a 5 km radius in the same constituency, number of electoral areas in a 10 km radius in the same constituency, distance to nearest electoral area in the same constituency (km), number of electoral areas assigned registration observers in a 5 km radius in the same constituency, number of electoral areas assigned registration observers in a 10 km radius in the same constituency, and distance to nearest electoral area assigned a registration observer (km). These variables are summarized in Table 1. We also used these geocoded electoral areas to make small adjustments to the constituency boundaries so that electoral areas identified and located by matching polling station names with other sources actually fell within the boundaries. Neither population figures nor previous elections results were available at this level of disaggregation.

⁸For previous elections, CODEO trained their registration observers to address these issues in a one-to-two-page written report rather than on a preprinted checklist with space for descriptions of any incidents. These observers were free to select which electoral areas to visit.

⁹We cannot use CODEO data to investigate effects of the treatment, because these have no information for the control electoral areas.

¹⁰Replication data are available at: <http://dvn.iq.harvard.edu/dvn/dv/nichino>.

FIGURE 1 Ghana, with Treatment and Control Constituencies and Electoral Areas



Balance Statistics

We check our randomization procedure with difference in means tests for the baseline covariates across our three levels of assigned treatment (Table 1). We find that the density of electoral areas in the neighborhood and the baseline number of registered voters in the electoral area in 2004 are similar across the three levels of assigned treatment. We also regress these pretreatment variables on the constituency-level and electoral area-level treatment indicators and the full set of block fixed effects (Table 2). We find that the coefficients on the treatment

variables are not statistically significantly different from zero.¹¹

In three of our four regions, the baseline number of registered voters in 2004 does not differ significantly between treatment and control, nor with electoral areas in constituencies not selected for the

¹¹We also formally compare the distribution of the constituency-level vote shares for the two major parties in the previous presidential election with a Kolmogorov-Smirnov test. We cannot reject that the distributions of NPP and NDC vote shares are the same for the whole sample ($p = 0.745$), among treatment constituencies ($p = 0.879$), or among control constituencies ($p = 0.918$).

TABLE 1 Means of Variables by Treatment Assignment Status

	Assignment Status			Difference	
	$T^c = 1$ $T = 1$ (a)	$T^c = 1$ $T = 0$ (b)	$T^c = 0$ $T = 0$ (c)	(a)–(b)	(b)–(c)
<u>Pretreatment Variables</u>					
# Registered voters in electoral area in 2004	1899	2189	1799	-290 (375)	390 (252)
# Electoral areas in 5 km radius in same constituency	2.94	3.32	2.79	-0.38 (0.45)	0.53 (0.29)
# Electoral areas in 10 km radius in same constituency	7.53	7.84	7.22	-0.31 (0.78)	0.62 (0.53)
Distance to nearest electoral area in same constituency (km)	3.79	4.25	4.31	-0.46 (0.82)	-0.06 (0.87)
<u>Spillover Variables</u>					
# Electoral areas in 5 km radius assigned registration observer	0.75	0.84	0	-0.091 (0.137)	0.844 (0.041)
# Electoral areas in 10 km radius assigned registration observer	1.95	2.16	0	-0.213 (0.230)	2.16 (0.00)
Distance to nearest electoral area assigned a registration observer (km)	8.34	6.89	41.34	1.45 (1.00)	-34.45 (1.53)

Standard errors in parentheses. N=868 electoral areas.

experiment (not shown). However, Trobu-Amasaman constituency in Greater Accra Region, which was assigned to treatment, had approximately 82,000 registered voters in 17 electoral areas, while the two control constituencies in that block had approximately 110,000 and 140,000 registered voters distributed over five and eight electoral areas, respectively.

Analysis

We examine the effect of registration observers on voter registration using the percentage increase at the electoral area level between 2004 and 2008 as the outcome, to take account of the different baseline numbers of registered voters in different locations.

The percentage change in registration from 2004 to 2008 has a mean of 0.257 with a standard deviation of 0.115. Registration data is also available for 2006, but we do not use this data because of problems with voter registration numbers for 2006 that were found in 2008 and addressed by the Electoral Commission's Kangah Commission Report. This commission found that in several government (NPP) stronghold constituencies the number of registered voters had doubled between 2004 and 2006. This only became clear after our randomization, and five of the 12 constituencies in Ashanti Region selected for our experiment were among those that had nearly double the number of registered voters in 2006 as in 2004. Therefore, we focus on the change between 2004 and 2008.

TABLE 2 Regression of Pretreatment Variables on Treatment Assignment

	(1)	(2)	(3)
Dependent Variable:	# Registered Voters in 2004	# Electoral Areas in 5 km	# Electoral Areas in 10 km
Treatment constituency (T^c)	-45 (569)	0.472 (0.435)	0.619 (1.119)
Electoral area assigned registration observer (T)	-102 (236)	-0.071 (0.377)	0.204 (0.483)
Block Fixed Effects	Yes	Yes	Yes
R^2	0.333	0.335	0.358
N	868	868	868

OLS. Disturbances clustered at the constituency level; robust standard errors in parentheses. Thirty-nine clusters.

Model

To investigate the full model, including the localized spillover effects of observers, we estimate the following:

$$\begin{aligned}
 Y_{ij} = & \beta_0 + \beta_1 T_{ij} + \beta_2 T_i^c + \sum_d (\beta_{3d} \cdot t_{dij}) \\
 & + \sum_d (\beta_{4d} \cdot T_{ij} t_{dij}) + \sum_d (\beta_{5d} \cdot n_{dij}) \quad (1) \\
 & + \sum_d (\beta_{6d} \cdot T_{ij} n_{dij}) + \mu_b + \epsilon_{ij}
 \end{aligned}$$

Y_{ij} is the percentage change in the number of registered voters from 2004 to 2008 in electoral area j in constituency i , T_{ij} is an indicator for whether an observer was randomly assigned to electoral area j in constituency i during the registration period in August 2008, T_i^c is an indicator for whether an observer was assigned to any of the electoral areas in constituency i during registration, and μ_b indicates block fixed effects. Our variable for capturing localized spillovers is t_{dij} , the number of electoral areas in constituency i assigned to treatment within distance d of electoral area j in constituency i . n_{dij} is the total number of electoral areas within distance d of electoral area j in constituency i .

As noted earlier, the stable unit treatment value assumption (SUTVA) is violated in a world with spillovers, and ignoring these spillovers will result in biased estimates of the primary effect. Since an electoral area that has neighboring ELAs that are treated is also always in a treatment constituency, the neighborhood treatment variables (t_{dij}) are positively correlated with the indicator variable for the treatment constituency (T_i^c). Therefore if the effect of the neighborhood treatment variables (t_{dij}) is nonzero and they are omitted from the model, the variable T_i^c is endogenous. If we assume that there is no interaction effect between T_i^c and the neighborhood treatment variables t_{dij} so that the coefficients on $T_i^c \times t_{dij}$ are zero, then we can determine the direction of the bias resulting from the omission. Since the correlation between the neighborhood treatment variables t_{dij} and T_i^c is positive, the sign will be determined by the true coefficients of the neighborhood treatment variables. Under our hypothesis of positive localized spillover effects, there will be an upward bias, and if the true primary effect is negative, a specification that excludes the neighborhood treatment variables (t_{dij}) will lead to a coefficient that is closer to zero (and potentially insignificant) because of this upward bias. In the more complex world in which there is feedback

among treatment ELAs, so that the coefficients on $T_i^c \times t_{dij}$ are not zero, the neighborhood treatment variables are still correlated with the treatment ELA indicator T even after the effect of the constituency-level treatment T^c has been partialled out. Establishing the direction of the bias in this case is more difficult as it would require knowledge of the covariances of the variables with each other. In either case, excluding the neighborhood variables from the model specification will lead to biased estimates because of the SUTVA violation.

While our randomization procedure guarantees that all electoral areas within a constituency have an equal probability of being selected for treatment, electoral areas do not all have the same probabilities *ex ante* of being assigned a particular number of treated neighbors because some electoral areas are more centrally located than others. In practice, treatment and control electoral areas within treatment constituencies have the same number of treated electoral areas in the neighborhood on average (Table 1), but the randomization procedure does not guarantee that the density of treatment in the neighborhood of an electoral area will be uncorrelated with other characteristics of electoral areas, such as population density, distance to roads, and other local characteristics, that may affect voter registration. Therefore, our model also includes n_{dij} , the total number of electoral areas in constituency i within distance d of electoral area j in constituency i , which will capture all these aspects that are unrelated to treatment, for which t_{dij} might proxy. As noted earlier, electoral areas are geographically coded as points, and both t and n are computed as counts of points that fall within a particular distance d of the given point.

If registration observers deter registration in the electoral areas they visit but these deterred registrations are displaced to nearby electoral areas, then $\beta_1 < 0$ and $\beta_{3d} > 0$. We use $d = 0005$ to denote electoral areas within a 5 km radius and $d = 0510$ to denote electoral areas located between 5 km and 10 km from a particular electoral area. We also add interaction terms between the treatment indicator T_{ij} and t_{dij} and between T_{ij} and n_{dij} in some specifications.

Table 3 presents our results, with Column 3 reporting results for the main OLS specification. Allowing the effect of a registration observer in a nearby electoral area to vary with treatment status and defining “nearby” as a 5 km radius of the electoral area, we find that within treatment constituencies, electoral areas with registration observers have an

TABLE 3 Effect of Registration Observers on Percentage Change in Registration from 2004 to 2008, 5km/10km

Dependent Variable: Percentage change in number of registered voters from 2004 to 2008						
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	IV	IV	IV
Treatment constituency (T^c)	-0.006 (0.016)	-0.042 ⁺ (0.022)	-0.041 ⁺ (0.024)	-0.003 (0.017)	-0.036 (0.022)	-0.036 (0.024)
Electoral area assigned registration observer (T)	-0.016 (0.011)	-0.030* (0.014)	-0.035* (0.017)			
ELA visited by registration observer (V)				-0.022 (0.015)	-0.044* (0.016)	-0.042 ⁺ (0.022)
# Electoral areas in 5 km assigned registration observer		0.028* (0.008)	0.027** (0.008)			
# Electoral areas in 5–10 km assigned registration observer		0.010 (0.006)	0.011 (0.007)			
T^* # Electoral areas in 5 km assigned registration observer		-0.007 (0.012)	-0.010 (0.017)			
T^* # Electoral areas in 5–10 km assigned registration observer		0.019*** (0.004)	0.013 (0.009)			
# Electoral areas in 5 km visited by registration observer					0.023* (0.009)	0.023* (0.010)
# Electoral areas in 5–10 km visited by registration observer					0.005 (0.006)	0.005 (0.007)
V^* # Electoral areas in 5 km visited by registration observer					-0.004 (0.013)	-0.001 (0.019)
V^* # Electoral areas in 5–10 km visited by registration observer					0.019*** (0.004)	0.021 (0.013)
# Electoral areas in 5 km			0.001 (0.001)			0.001 (0.001)
# Electoral areas in 5–10 km			-0.001 (0.002)			-0.001 (0.002)
T^* # Electoral areas in 5 km			0.003 (0.007)			
T^* # Electoral areas in 5–10 km			0.002 (0.005)			
V^* # Electoral areas in 5 km						-0.003 (0.009)
V^* # Electoral areas in 5–10 km						-0.001 (0.008)
Block fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.166	0.193	0.193	0.159	0.193	0.192
N	868	868	868	868	868	868

Disturbances clustered at the constituency level; robust standard errors in parentheses. Thirty-nine clusters. ⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

approximately 3.5 percentage point *smaller* increase in registration than electoral areas without registration observers. At the same time, a registration observer visiting a nearby electoral area results on average in an approximately 2.7 percentage point greater increase in registration. This is consistent with the *displacement* of some of the registration *deterred* in the visited electoral

areas to nearby electoral areas which experience a greater registration increase than otherwise. Note that we do not detect the primary effect of observers (T) or a general spillover effect (T^c) in the model without the neighborhood variables (Column 1).

In addition to this displacement or localized spillover effect, we detect a general spillover effect

of these registration observers at the constituency level. The coefficients on the constituency-level and electoral area-level treatment indicators have the same sign, and the estimate of the former ($\widehat{\beta}_2$) is 0.041, with a p -value of 0.086 in the OLS specifications. These results imply that an electoral area assigned a registration observer, but with no electoral areas assigned registration observers in a 5 km radius ($T^c = 1, T = 1, t_{0005} = 0$; henceforth we omit the indices i and j for simplicity), has on average an approximately 7.6 percentage point smaller increase in registration than an electoral area in a control constituency ($T^c = 0, T = 0, t_{0005} = 0$). This average difference shrinks to 5.9 percentage points if an electoral area in the 5 km neighborhood is assigned a registration observer ($T^c = 1, T = 1, t_{0005} = 1$) and to 1.4 percentage points for an electoral area without a registration observer but with an electoral area in the 5 km neighborhood assigned a registration observer ($T^c = 1, T = 0, t_{0005} = 1$). For the specification in Column 3, we reject the null hypothesis of no treatment effect, $H_0: \beta_1 = \beta_2 = \beta_{3d} = \beta_{4d} = \beta_{6d} = 0$, in a two-tailed test with $F(8,38) = 8.20$ and a p -value of less than 0.0001.¹² We also reject the null hypothesis of no primary or general spillover effect, $H_0: \beta_1 = \beta_2 = 0$, in a two-tailed test with $F(2,38) = 4.82$ and a p -value of 0.014.

We use our randomized assignment to treatment as an instrument for whether an electoral area was visited by a registration observer (V_{ij}) in Columns 4–6 of Table 3. Early August is the end of the rainy season in southern Ghana and still the middle of the rainy season in northern Ghana, and the registration observers noted difficulty traveling on many rural roads and crossing rivers. Moreover, there was some confusion surrounding the schedule of which registration centers would be open on a given date. However, compliance was generally very good (Table 4) and consequently there are only small differences between our OLS and IV estimates.

We check for robustness by estimating equation (1) at other distances (4 and 8 km, 6 and 12 km), including the log number of registered voters in 2004 as a control and using the log number of registered voters in 2008 as the outcome with the 2004 figures as a control, and the results remain substantively the same. In general, the

¹²We also use randomization inference (Fisher 1935; Rosenbaum 2002) to test the exact null of no treatment effect, $H_0: \beta_{1i} = \beta_{2i} = \beta_{3di} = \beta_{4di} = \beta_{6di} = 0$, for the intent-to-treat analysis using radii of 5 and 10 km. Using the F -statistic from the actual experiment and the randomization procedure from the experiment to generate the null distribution based on 10,000 randomizations, we reject the null in a two-tailed test with an exact p -value of 0.03.

estimated primary ITT effect of registration observers ($\widehat{\beta}_1$) is about -4% and the estimated localized spillover effect ($\widehat{\beta}_{3d}$) is greater at shorter radii, which is consistent with a displacement of potential registrants away from an observed registration center to closer alternative registration centers. The results also do not change substantively when we include an additional “ring” ($d = 1020$, for example) to consider the effect of registration observers in electoral areas further away. We also estimate equation (1) without T^c but with constituency fixed effects instead of block fixed effects, and the results for the primary and spillover effects of observers remain substantively the same (not shown).

As an additional robustness check, we use the distance to the nearest neighboring treatment electoral area instead of the number of treatment electoral areas in a certain radius. We regress the percentage change in registration from 2004 to 2008 on the constituency and electoral area level treatment indicators, the inverse of the distance to the nearest neighboring treatment electoral area ($(d_{ij}^T)^{-1}$), its interaction with the constituency-level treatment indicator variable, and the full set of block fixed effects:

$$Y_{ij} = \beta_0 + \beta_1 T_{ij} + \beta_2 T_i^c + \beta_3 (d_{ij}^T)^{-1} + \beta_4 T_i^c (d_{ij}^T)^{-1} + \mu_b + \varepsilon_{ij} \tag{2}$$

We expect the effect to be larger the closer the nearest treatment electoral area, since distance raises the cost of visiting another registration center. We use this transformation of the distance measure to allow for this effect to diminish more rapidly at closer distances than at greater distances. We also restrict the sample to electoral areas whose nearest neighbor treated lies at less than the maximum distance for electoral areas in treatment constituencies (54 km) for this analysis.

With this set up, the estimated coefficient on the inverse of the distance to the nearest neighboring treatment electoral area for electoral areas in treatment constituencies is $\widehat{\beta}_3 + \widehat{\beta}_4 = 0.042$ with a p -value of 0.028, while the coefficient on distance for electoral areas in control constituencies ($\widehat{\beta}_3 = -0.148$) is statistically indistinguishable from zero with a p -value of 0.490. The estimate for β_1 is -0.10 (with a standard error of 0.012) and the coefficient on the constituency-level treatment indicator ($\widehat{\beta}_2$) is estimated to be -0.016 (with a standard error of 0.018). The estimated coefficient on the constituency-level treatment indicator ($\widehat{\beta}_2$) is smaller than our estimates for equation (1) reported in Table 3 because it effectively incorporates

TABLE 4 Compliance Rate by Treatment Assignment Status

Assignment Status	# Electoral Areas	# Visited ($V_{ij} = 1$)	Compliance ($T_{ij} = V_{ij}$)
Treatment constituency, treatment electoral area	77	65	84%
Treatment constituency, control electoral area	199	24	88%
Control constituency, control electoral area	592	1	99%

the difference in average distance to the nearest treatment electoral area between electoral areas in treatment and control constituencies. In another robustness check, we use the negative log of the distance to the nearest neighboring treatment electoral area as an alternative measure, and the substantive findings remain the same (not shown).

To get a sense of the magnitude of the treatment effects that includes all localized and general spillover effects, we perform the following calculation. We set all the treatment variables (T , T^c , and the number of electoral areas assigned registration observers in 0–5 km and 5–10 km (t_d)) and all the interaction terms involving those variables to zero and then predict for each electoral area the percentage change in registration using the results from the baseline regression of Table 3, Column 3. Using this predicted percentage change, we calculate the predicted absolute change in registrations for the electoral areas in our sample. These estimates suggest that there would have been 4,600 more registrations in the 13 treatment constituencies in the absence of our treatment. If we assume that the estimates of the treatment effects apply to the whole country and that these effects remain the same once the experiment is scaled up (i.e., we treat 25% of electoral areas in each of the 230 constituencies in Ghana), then we can also quantify the Ghana-wide effect: because our treatment constituencies contain about 5.3% of all registrants in Ghana in 2004, extrapolating the effect from the 13 constituencies implies an estimate of 87,000 fewer registrations as a consequence of a scaled-up treatment. We must be very careful to note, however, that the constituencies in our experiment were not a representative sample of all Ghanaian constituencies, and also that the above calculation assumes, somewhat unrealistically, that increasing the total number of electoral areas and constituencies treated does not affect the magnitude of the primary or the spillover effects.

Overall, we find robust evidence that registrations are deterred at treatment electoral areas and in treatment constituencies more generally. However, some of this reduction in registration is negated by

displacement to electoral areas that are close to treated electoral areas.

Citizens and Electoral Officials

We revisit two alternative mechanisms by which registration observers might affect registration, which could complicate our interpretation of the effect of a registration observer on the electoral area s/he visited as a lower bound estimate on registration irregularities most likely enabled by party agents.

First, registration observers might affect registration by influencing the behavior of citizens who may feel less intimidated and become more likely to register. We believe that this is unlikely because of the confusion around the schedule and logistics of the registration exercise, as well as the unannounced nature of the registration observers' visits. Interviews with district-level Electoral Commission officers indicated that equipment problems sometimes forced registration centers to be merged and their locations changed with little notice.¹³ This mechanism cannot account for the positive localized spillover effects presented in Table 3. Moreover, this mechanism would lead us to underestimate the extent to which extra registrations are deterred by observers, so that the interpretation of our estimate as a lower bound is still valid.

A second possible mechanism is the influence of registration observers on the behavior of electoral officials. To investigate this mechanism, we conducted a survey of electoral officials posted in the electoral areas during the exhibition of the provisional voters register in October 2008. All electoral areas that were selected for treatment during the registration period and approximately 30% of the remaining electoral areas from both our treatment and control constituencies were randomly assigned to be visited by observers during the voter register exhibition period. None of the registration observers

¹³Telephone interview by research assistant, Greater Accra Region, July 2009.

were exhibition period observers, so that no registration observers were involved in evaluating their own effectiveness.

These exhibition period observers conducted a survey of electoral officers and any party agents present about voter registration in that area and asked for provisional registration numbers. These observers completed surveys of electoral officers in 304 electoral areas (of which six are missing information needed to identify the constituency or electoral area). Unfortunately, the district-level Electoral Commission officers hire a large number of temporary staff for these national-scale exercises, and many of the officials posted during the exhibition period were not the same as those posted during the registration exercise.¹⁴

Although officially a registrant does not need to present ID in order to be registered, approximately 60% of exhibition-period electoral officials who reported that there were observers at the registration center in their electoral area in August asked for ID. A nearly identical proportion (56%) of these electoral officials who responded that no observers visited during registration also responded that identification was requested of registrants. Local electoral officials frequently expounded upon their creative solutions to shortages of registration forms and malfunctioning registration equipment in order to accommodate the unexpectedly large number of people who turned out to register. Registration observers therefore likely had very little effect on the behavior of electoral officials in ways that depressed registration. As with the citizens mechanism, it is also difficult to imagine that officiousness would account for such a large positive localized spillover effect.

Registration observers might alter the behavior of electoral officials in other ways, however. Electoral officials who see registration observers at a registration center could report this up the chain of command, affecting the behavior of their counterparts at other registration centers. This is in accord with the finding of no difference in registration increase between electoral areas with and without registration observers in the model without the spillover variables (Column 1) and with the general spillover effect found in the full specification (Column 3) in Table 3. However, we have no direct evidence to support this contention, and by itself, this cannot account for the finding of positive localized spillovers.

The estimated primary effect of registration observers on registration ($\widehat{\beta}_1$), taking into account localized spillover effects, may then be interpreted as the effect of registration observers through their influence on party agents active during registration. We do not argue that all registration irregularities were deterred where registration observers were present, and we would underestimate the extent of registration inflation if the registration of eligible voters was also suppressed. Hence, this is an estimate of the lower bound on the extent of the registration inflation form of irregularities in Ghana in 2008.

Conclusion

This article extends the empirical scholarship on electoral fraud to the study of misconduct at the preelection stage. It presents findings from a randomized field experiment on the effect of domestic observers on the extent of irregularities in voter registration in Ghana in 2008. Our research design and analysis explicitly take into account the spillovers that may result from the organization of political parties across multiple electoral areas and the capacity of party agents to transport supporters from one electoral area to another.

We find a general spillover or constituency-level effect; the increase in the number of registered voters from 2004 to 2008 was on average 4.1 percentage points smaller for electoral areas in constituencies with some registration observers than electoral areas in constituencies with no registration observers. Furthermore, within constituencies with registration observers, the increase in registration was on average approximately 3.5 percentage points smaller in electoral areas with observers than without (primary effect). However, an electoral area with a registration observer located within 5 km led to, on average, a 2.7 percentage point greater increase in registration. This combination of a positive localized spillover effect from nearby electoral areas with a negative primary effect is strong evidence that deterred extra registrations are being displaced. Based upon the design of the experiment, we attribute this effect to the registration observers' influence on the activity of party agents. Therefore, we interpret the negative primary treatment effect as a lower bound on the extent of irregularities.

This research on irregularities in voter registration has implications for both prodemocracy actors and scholars of democratization and electoral fraud

¹⁴Many school teachers and university students were hired for registration since they are literate and registration took place during the school holidays; they were at school in October.

in partial democracies. As countries like Ghana are designated “consolidating democracies,” international organizations shift their attention and resources elsewhere, and the role of domestic observers in protecting the quality of elections grows in importance.¹⁵ Domestic election observers and other prodemocracy actors may be heartened that a relatively small observer presence had a significant impact in our experiment. But they should allocate their resources more densely where greater harmful spillover effects are expected and more towards registration and other earlier stages of the election process (Bjornlund 2004; Carothers 1997). Otherwise, they risk regularly declaring elections with substantial displaced and hidden fraud as free and fair and diminishing their credibility and their potential roles in the democratization process.

Furthermore, our finding of positive spillovers has implications for researchers who wish to measure electoral irregularities or use data collected by observers. Our findings suggest that even in a model new democracy like Ghana, political parties appear to evade observers, who deter some but displace substantial irregularities in registration. Although the magnitude of the primary and spillover effects are likely to vary for other elections, with the extent of media coverage of observers’ activities, the difficulty of reaching an alternative registration center, and political parties’ resources and past experience, the basic incentive for political parties to inflate the register and to evade observers while doing so should pertain in elections in other new democracies. Further investigation of registration and other displaced preelection irregularities for additional elections and other countries should improve our measures for election quality and advance scholarship on electoral fraud and electoral politics in new democracies.

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¹⁵Interview with USAID official, Accra, Ghana, July 2008.

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