Ethnicity and Electoral Fraud in New Democracies: Modelling Political Party Agents in Ghana

Nahomi Ichino *   Jake Bowers †   Mark M. Fredrickson ‡

September 8, 2013 (Version: 2163b42)

Abstract

How might ethnicity affect political party strategies for electoral fraud and complicate the consolidation of new democracies? We engage this question by modelling the efforts of political party agents to inflate the voters register ahead of the 2008 general elections in Ghana. Because we cannot directly observe rigging of the voter registration process, we take advantage of a randomized field experiment that placed observers at registration centers to affect the behavior of the party agents. We propose two simple models of party agent behavior, one of which incorporates an additional preference for visiting registration centers in ethnically homogeneous areas. We then use new network data to assess the evidence against these models given the design and data from the field experiment. Our preliminary results indicate that there is little evidence in favor of the party agents each visiting large numbers of registration centers. Although the model with ethnicity cannot be ruled out, it does not appear to improve substantially on our simpler model of agent movement over road networks.

*Associate Professor, Department of Government, Harvard University (`nichino@gov.harvard.edu`). *Acknowledgements:* Thanks to the participants in the Causality in Political Networks conference, the workshop on methodology in the Department of Government at the University of Texas, and the SAMSI Workshop on Social Networks 2013 for helpful advice and the Ghana Statistical Service and the Republic of Ghana Ministry of Roads and Highways for data.

†Associate Professor, Department of Political Science, University of Illinois @ Urbana-Champaign (`jwbowers@illinois.edu`)

‡Ph.D. Candidate, Department of Political Science, University of Illinois @ Urbana-Champaign (`fredric3@illinois.edu`)

The wave of democratic transitions following the end of the Cold War sowed the seed of optimism that fair elections will constrain leaders and improve governance. But many elections in these transitional democracies have been marred by electoral irregularities such as vote-buying, ballot-stuffing, and violence (Schedler, 2002), which can depress political participation (McCann and Dominguez, 1998; Simpser, 2012) and harm public confidence in democracy and regime legitimacy (Birch, 2008; Elklit and Reynolds, 2002; Rose and Mishler, 2009). The prevalence of irregularities and their threat to political accountability and democratic consolidation has motivated domestic and international civil society actors to use various strategies to improve the quality of elections and promote democracy. Research on the effectiveness of these interventions include field or "natural" experimental studies of the effects of election observers who try to prevent election day fraud (Asunka et al., 2013; Brancati, 2012; Enikolopov et al., 2013; Hyde, 2007; Sjoberg, 2012), pre-election observers to prevent inflation of the voters register (Ichino and Schündeln, 2012), and anti-violence or anti-vote-buying campaigns directed towards ordinary citizens (Collier and Vicente, 2011; Driscoll and Hidalgo, 2009; Vicente, Forthcoming). Disappointingly, many of these interventions intended to support democracy have mixed or negative effects (Brancati, 2012; Driscoll and Hidalgo, 2009).

Among these is the field experiment of Ichino and Schündeln (2012), which randomized the allocation of domestic observers during the 13-day voter registration exercise leading up to the 2008 general elections in Ghana. It finds that registration centers that have observers had, on average, a smaller increase in voter registration from 2004 than those without, and that registration centers without observers located near a center assigned observers experienced a larger increase in registrations than would have been expected without the domestic observer intervention. It proposes that this may be explained by observers *displacing*, not just deterring, political party agents who organize false registrations away from registration centers with observers. Ichino and Schündeln (2012) engages the possibility of these spillovers in the design of the field experiment. But lacking direct observation of these party agents' movements and illicit activities, open questions remain about the mechanisms or more specifically how observers sent to a set of registration centers affect election quality in the wider area.

The design and data also have the potential to reflect on broader theoretical questions for new democracies beyond the original focus on the effects of observers on the quality of elections. For example, although ethnicity and ethnic diversity have been central to the scholarship on political party strategies and voter behavior in new democracies, particularly in sub-Saharan Africa, they have been far less prominent in the studies of electoral fraud and other illegal strategies that generally feature the calculation of political parties.[1] But shared ethnicity between voters and politicians

[1] Collier and Vicente (2012) analyze a formal model of electoral competition in which the incumbent and challenger may engage in vote-buying, intimidation, and ballot fraud (vote miscounting). They discuss in their case studies that each of these macro-level) strategies may be influenced by the strong sub-national (ethnic) identities in sub-Saharan Africa, but ethnicity is not formally incorporated into their model.

facilitates vote-buying (Kramon, 2011), and it may affect other illicit electoral strategies including inflating the voters register. Investigating how ethnicity affects some of these other strategies can contribute to our understanding of how political parties in diverse societies might destabilize new democracies, a concern serious enough that constitutions and electoral laws frequently contain bans on ethnic parties (Basedau et al., 2007), but also widely debated.

We build on theories of instrumental ethnic voting (Bates, 1983; Chandra, 2004; Ichino and Nathan, 2013; Posner, 2005) to bring in ordinary citizens into political parties' calculations and strategies. The basic idea is that citizens who strongly prefer a particular party win the election may be tacitly complicit when the party adds false registrants to the voters register. Anticipating this, and using ethnicity as an indicator of who is likely to be supportive of the party, a political party's agents will target registration centers in areas dominated by its associated ethnic group. Furthermore, registration centers are embedded in road networks that connect areas of varying ethnic demographics. So should party agents encounter an observer, they will divert their illicit activities to nearby registration centers located in areas with similarly accommodating ethnic demographics.

This theory is formalized in a model, along with a simpler model in which party agents move to the nearest registration center along a road network without consideration of ethnic demographics. We then characterize support for the model using the Ghana field experiment data and new data on networks in the framework for statistical inferences for causal models proposed by Bowers, Fredrickson and Panagopoulos (2013). As we explain below, our theoretical models and statistical assessments here are useful but unorthodox. We provide neither a likelihood function nor comparative statics, but still use data to learn about a model.

Our preliminary results show that the data do not support the idea that large numbers of party agents added large numbers false registrations at each registration center visited, but a large number of agents having a small effect on any visited center and a small number of agents having a larger effect on any visited center are both not implausible. Although the model with ethnicity cannot be ruled out, it does not appear to improve substantially on our simpler model of agent movement over road networks to the nearest registration center. But our results also indicate that these initial models are not capturing the dynamics of registration inflation very well, and both these models and the ethnicity measures we use can be improved in the future.

The paper proceeds as follows. We first verbally describe our model of how ethnicity might affect the behavior of political party agents. Section 2 provides background information on the 2008 Ghanaian elections and voter registration exercise, details of the field experiment, and the network data. We then formalize the verbal model and elaborate on the framework for assessing models with network treatment effects using the design and data from the field experiment. Analysis and discussion of the models follow.

1 Ethnicity and Registration Rigging

How do registration observers affect inflation of the voters' register? Ichino and Schündeln (2012) focused on the incentives of political parties. For competitive elections where political parties expect that it will be difficult to fabricate favorable results outright or to sufficiently intimidate or buy the support of voters and opponents, it noted that political parties can benefit from inflating the voters register with their own supporters. With a greater number of registered voters, a political party can add pre-marked ballots or have voters vote several times to skew the result in its favor without creating a suspiciously high turnout rate. Because these activities are illegal, and if found could trigger a revision of the register that undoes their work, the party's agents prefer to avoid civil society observers who will call attention to their activities. If they encounter an observer at a registration center, they may simply move their activities to another nearby registration center. Therefore, observers may deter some false registrations at the registration centers at which they are stationed, but divert the illicit activities to nearby registration centers.

This simple model did not feature differences among ordinary voters and their interests, and how these might affect the strategies of the political parties. However, particularly for high stakes elections in new democracies in ethnically divided societies, citizens often prefer to have a co-ethnic in office and may tacitly support false registrations by their own party to help it win the election.

Candidates encourage voters to believe that politicians better represent and reward co-ethnics or supporters in stronghold districts and that it would not be in one's own interest to vote for a politician from a different group in order to deter voters from defecting and supporting other candidates (Bates, 1983; Posner, 2005). Politicians may also stoke fears that having someone else in office can diminish their livelihoods and security, resulting in a "winner-take-all" type politics in which supporting an (incumbent) politician from one's own group is the best option for a voter (Padro i Miquel, 2007). Indeed, many political parties have strong ethnic profiles, and voters use ethnicity as an informational shortcut for what constituency will be served and whose clientelistic promises are more credible (Conroy-Krutz, 2013; Wantchekon, 2003). Political parties encourage and reinforce these divisions (LeBas, 2006; Ferree, 2010), and ethnicity becomes more salient closer to elections (Eifert, Miguel and Posner, 2010).

This narrative and body of scholarship suggest that citizens in ethnically homogeneous areas or "homeland" areas of ethnic groups associated with a particular political party may be more willing than citizens in mixed areas to tacitly enable false registrations. Because many of the benefits provided by government in rural areas are club or local public goods that can be targeted to particular communities (Ichino and Nathan, 2013), citizens in more homogeneous areas have more at stake in the election than their counterparts in areas without many people from the ethnic group(s) affiliated with a major party.

Therefore, anticipating that citizens who want the political party associated with their ethnic group to win will be complicitous with its illicit activities, political parties may target registration centers in more ethnically homogeneous areas for false registrations. And when their agents encounter observers at those registration centers, they redirect their activities to nearby, similarly ethnically homogeneous and accommodating registration centers.

However, ethnicity may play little role if political parties are not concerned with the tacit cooperation of ordinary citizens at the registration centers. The parties may not favor registration centers in more ethnically homogeneous areas, if ordinary citizens would generally do nothing upon seeing a small crowd of unfamiliar people registering to vote. Ordinary citizens may not expect to know other people, particularly at rural registration centers that serve multiple villages in ethnically diverse areas. Even if they thought that the small crowd of registrants was suspicious, they may not want to challenge this crowd and possibly invite trouble with the political parties later on. In this case, the party agents would start out at registration centers that are located near one another. If they encounter an observer, they would move to other nearby registration centers, without regard for the ethnic composition of the areas around those centers.

2 A Field Experiment on Voter Registration in Ghana 2008

2.1 The 2008 General Elections in Ghana

Ghana, an ethnically diverse country of about 23 million in West Africa, has held regular, competitive elections every four years since its transition to democracy in 1992. It holds concurrent, direct elections for president and a unicameral national parliament composed of 230 members elected by plurality from single-member districts. Presidents may serve only two terms and are elected by majority — should a candidate fail to win a majority in the first round, the top two finishers compete in a run-off election. The National Democratic Convention (NDC) and the New Patriotic Party (NPP) are the two major parties in Ghana. They consider themselves left-leaning and right-leaning, respectively, and each is strongly identified with regional and ethnic bases (Lindberg and Morrison, 2005; Nugent, 2001). Jerry Rawlings (NDC), the former military ruler, was president from 1992 to 2000. John Kufuor of the NPP defeated Rawling's successor in 2000, and was re-elected in 2004. Kufuor faced the two-term limit in 2008, and the parties expected the 2008 elections to be extremely competitive.

Election day activities in Ghana are carefully monitored by well-organized domestic and international observer groups. But voter registration is not routinely or broadly monitored, so that both civil society groups and ordinary citizens widely suspect that the voters register is full of false registrations that can be used to manipulate the election outcome. In fact, the political parties accuse each other of inflating the voters register to their own advantage, making it a major point of contention and a threat to the integrity and legitimacy of the election outcome (Ichino and Schündeln, 2012).

Citizens of Ghana may only register to vote during designated registration periods. They must register in person and only at the registration centers associated with the election-day polling station and electoral areas associated with their residence. Each electoral area is composed of several polling stations, and in practice, a voter registration center is located at one of the larger polling stations. Because Ghana does not have a national ID system, it is fairly easy to declare false information. The penalty for giving false information or registering multiple times is up to a year in prison, but very few people are ever prosecuted for false registration. The voter ID card may have a photograph if a camera was available at the registration site. On election day, a voter must go to the particular polling station associated with his residence and present his voter ID card in order to vote. Ichino and Schündeln (2012) describes the voter registration process for 2008 in greater detail. Partly because of the problems associated with the 2008 voter registration exercise, Ghana adopted a new system of biometric voter registration for the 2012 elections.

For a variety of reasons, including shortage of equipment and controversy around suspicious high registration figures in the 2006 voters register, the 2008 voter registration period was delayed several times. It finally began on 31 July 2008, with only a day's notice. Although each electoral area was supposed to have its own registration workstation, only about 2500 workstations were available for approximately 4800 electoral areas. Workstation distribution appeared to be disorganized, and no advance information on which electoral areas would have workstations on what days was centrally available. On the last day of the registration period, the Electoral Commission extended this period from 11 days to 13 days, due to shortages of materials and equipment.

Although the Electoral Commission had estimated that 800,000 people had become newly eligible to vote since the last voter registration period, there were approximately 2 million new registrations on the provisional new register as of the end of the 2008 registration period. As in previous voter registration exercises, registration observers and others saw political parties transport people to registration centers on buses and trucks (Ichino and Schündeln, 2012). After vetting for deceased voters and other processing by the Electoral Commission, the final voters register listed approximately 12.5 million voters. The general elections were held without delay on 7 December 2008. The opposition NDC won the presidency with a final official vote margin of less than 50,000 votes in the runoff election.

2.2 Experimental Design

The field experiment was conducted in cooperation with the Coalition of Domestic Election Observers (CODEO), an umbrella group of civil society organizations that monitors election campaigns and organizes observers. Before randomizing, CODEO and the researchers decided to focus the experiment on four of the ten regions of Ghana: Ashanti, Brong Ahafo, Greater Accra, and Northern Regions. These four regions contain approximately 54% of the Ghanaian population as of the 2000 Census, containing 116 of its 230 parliamentary constituencies and 2,204 of its approximately 4,800 electoral areas (ELAs, which are subunits of constituencies). Selection of regions was not random and reflected both practical concerns about the availability of observers, available budget, and a desire to include as much of the population of Ghana as possible given those constraints.

We organized the constituencies into between two to four blocks in each region by the difference in vote share won by the NPP and NDC candidates in the 2004 parliamentary elections, which could be as great as 69 percentage points. In the first stage of the randomization, from each block, we randomly selected two constituencies to have no observers and one to have observers. In the second stage, in each of the treatment constituencies, we randomly selected approximately 25% of the electoral areas to be visited by registration observers. This resulted in 77 ELAs assigned to the treatment condition and 791 ELAs to the control condition. The outcome is the number of registered voters, which was obtained from the Electoral Commission. To control for variations in the size of the ELA, we also obtained registration figures for the 2004 election and subtracted these from the 2008 registration counts, making the final outcome the difference in registration across these two elections.

The observers were recruited from CODEO member organizations through their usual procedures, trained by CODEO and the research team, and deployed with official accreditation as observers from the Electoral Commission. They were instructed to visit unannounced only the registration centers for the ELAs on their list, to make an initial visit of 1–2 hours and then return for up to a full day in a later visit on an unannounced date. Observers in Ghana may not assist or interfere in voter registration, but they may interact with the officials and party agents present. For further details, see Ichino and Schündeln (2012).

2.3 The Network

Our models endeavor to explain the patterns in which party agents avoid observers across a network of roads and places with different ethnic characteristics. To the original data of Ichino and Schündeln (2012), we added road and ethnicity data. We produced the road network data by digitizing regional maps of roads and villages from the Ghana Ministry of Roads and Highways, Department of Feeder Roads, in Accra. Because most urban roads are not under the purview of this department, we drop Greater Accra Region which contains the capital city Accra from the analysis. This leaves 797 ELAs in 33 constituencies organized into 11 blocks.

These maps and the updated GNS gazetteer of place names were also used to make corrections to some of the geocoding of the electoral areas in the original study.[2] The electoral areas are each composed of several polling stations, and because no maps of the electoral area boundaries are available, we used the polling station in the electoral area with the greatest number of registered voters in 2004 to geocode the electoral areas as points. These electoral areas could then be associated

[2]The GNS data are available from the United States National Geospatial-Intelligence Agency (NGA) at `earth-info.nga.mil/gns/html/`.

with census enumeration areas, which allowed them to be assigned the ethnicity data for those enumeration areas from the 2000 Ghana Population and Housing Census from the Ghana Statistical Service. Unfortunately, ethnicity data is only available at the level of the larger groups and not their subgroups recognized by the Ghanaian government, and this may obscure some important differences across registration centers. In some cases, multiple electoral areas are coded to the same enumeration area, but in many cases the catchment area for the electoral area is larger than just the enumeration area to which the electoral area is coded.

The distance along the road network from one electoral area to another was calculated as the sum of the distance from the origin electoral area to its closest point on the network, the distance from the destination electoral area to its closest point on the network, and the distance along the network between those points. The remaining data on observer assignments and the number of registered voters at the ELA-level come from the original analysis of the field experiment.

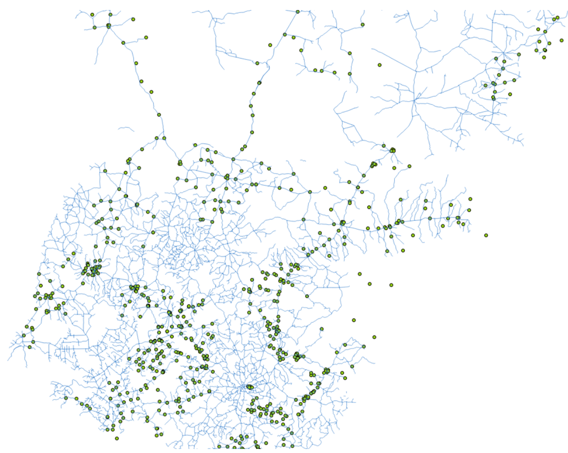

Figure 1: A portion of the road network connecting electoral areas in Ghana.

2.4 Registered voters in treatment and control ELA

The left panel of Figure 2 shows the outcomes from the study centered by constituency and block. On this outcome, ELAs without an observer had a median change in the number of registered voters of 40, while ELAs that were assigned an observer had a median change in the number of registered voters of 103. Among the control ELAs, 40% had more registered voters in 2008 than they did in 2004, as compared with 26% of the treated ELAs. The treated and control ELAs differ in their distribution of the outcomes, as can be seen in the left panel of Figure 2.

This figure represents a puzzle: how much of this difference in distributions can be attributed to the observers? Moreover, if observers were able to prevent party agents from inflating voter roles, how much of that activity was redirected to nearby registration centers? In other words, how much of the increase in the number of registered voters in the control units would we have even in the absence of the field experiment? More importantly, did the presence of a party's ethnic base enhance the effect of observers in certain treated ELAs? If party agents indeed diverted their corrupt activities elsewhere, did they target areas with concentrations of co-ethnics who are likely to be partisan supporters?

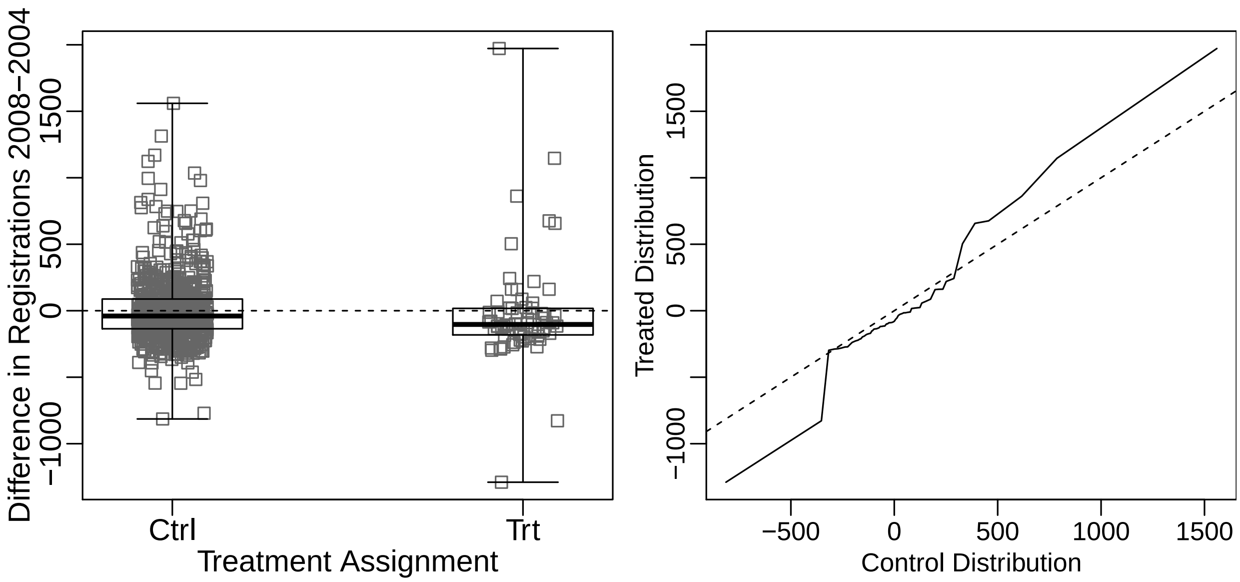

Figure 2: Left: The number of registered voters was lower in 2008 than in 2004 in treated ELAs (centered within experimental block). Right: QQ-plot of change in the number of registered voters in control and treated ELAs. The distributions differ largely at the tails.

In the next section we present models that assess the plausibility of explanations accounting for the patterns observed in Figure 2. We premise our analysis on the maxim, "set a thief to catch a thief." If party agents are subverting the voter registration process, we can only understand these outcomes by understanding the motivations and constraints faced by party agents. Our models start by considering the goals of party agents and how they would achieve those objectives. From these micro-foundations, we derive the overall impact these agents would have on voter registration counts in each ELA. Using these implications, we test hypotheses that capture the size and scope of corrupt activity. In the end, the goal of these models is to remove the patterns we see in the boxplot and QQ-plot of Figure 2. If our models are good approximations to the actual activities of party agents, we can make the two boxplots in Figure 2 (left) would look like two random samples from the same population, rather than looking as different as they do now. In the next section we provide the details of these models and the statistical methods we use to evaluate them.

3 Formalizing and Assessing Models of Network Treatment Effects

A scientific model makes a statement about each observable unit in a study.[3] A causal model makes this statement in counterfactual form. For example, a very simple model might say that, an assigned to treatment ELA i would have registered $\tau = 20$ more people if it had been in the control condition because the buses used to transport party activists between polling stations held approximately 20 people. We can formalize this counterfactual model as $y_{i,Z_i=0,\mathbf{z}_{-i}} = y_{i,Z_i=1,\mathbf{z}_{-i}} + \tau$, where $y_{i,Z_i=0,\mathbf{z}_{-i}}$ is the outcome we would have seen for unit i if it had been assigned to the control group ($Z_i = 0$) and the other units in the study ($-i$) were assigned treatment according to some vector of treatments $\mathbf{z}_{-i}$. We often write the potential outcome for unit i in the control condition as $y_{i,Z_i=0,\mathbf{z}_{-i}} = y_{i,Z_i=0,\mathbf{z}'_{-i}} = y_{i,Z_i=0} \equiv y_{i,0}$ where $\mathbf{z}_{-i} \neq \mathbf{z}'_{-i}$.[4] This means that the outcome that we would have observed for unit i in the control condition would be the same regardless of the treatments assigned to the other units in the study. Of course, we can, and will, relax this equivalence using a model.

This first model is a simple model of constant additive effects based on an observation from the field. Later, we will show how to assess whether there are any values of model parameters, like τ above, that are implausible given our data. While we cannot say for certain that social actors behave according to the same rules or in the same environment as the agents in the model, we will show that sets of rules and environments that *do not* lead to observed social patterns can be discarded. A model that is not substantively incommensurate with a set of data and hence not discarded is a *candidate explanation* for a social phenomenon (Epstein and Axtell, 1996; Epstein, 2006).

In our causal scientific model, the actors are the political party agents who want to add false registrations to the voters register during the pre-election voter registration period to facilitate and cover up ballot stuffing or multiple voting at the general election. For now, we do not differentiate agents of different parties, and all agents share the same behavioral rules and abilities. At the beginning of the registration period we imagine that a total of k agents are placed at voter registration centers (ELAs). The party agents know their own location and the distances to all other ELAs. Agents may then to move from ELA to ELA for a set number of iterations t, which we call "ticks." At each ELA an agent visits, it inflates the voters register by a given amount, τ, if an observer is not present. If an observer is present, we assume that the agent is unable to add registrants at that ELA, but the agent may immediately move to a new ELA. An agent may not visit an ELA that

[3]It also may make a statement about units not in a study, but the relationship between a model and units not observed (in times and places unknown) is beyond the scope of this paper.

[4]The idea that we might conceptualize and formalize causal relations for experiments using counterfactuals stems from Neyman (1923 [1990]) although the development and formalization of the idea of potential outcomes is mostly traced to Rubin (1974). Excellent expositions of these and related ideas for social scientists may be found in Brady (2008); Sekhon (2008).

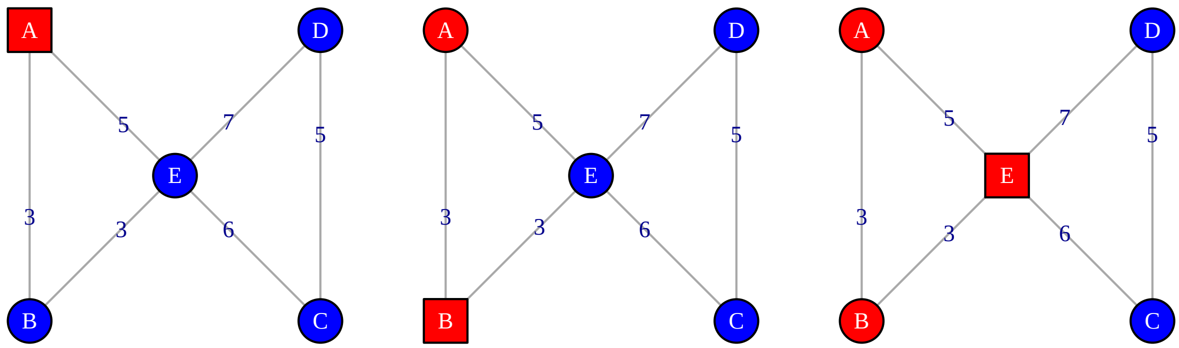

Figure 3: Agent movement rules when no observers are encountered. Squares indicate the agent's current location. Red ELAs are visited. Blue ELAs are not yet visited. From left to right: 1) $t = 0$, the agent starts at A, 2) agent selects B as closest ELA, 3) Agent moves to E in final period.

he has already visited, but multiple agents may visit the same ELA. The observers randomized to ELAs to prevent registration fraud do not move, and their locations correspond to the location of observers in the fielded study. The observers are fixed features of the environment, and the only actors in the model are the agents.

Figure 3 illustrates these rules governing agent movement for five ELAs (A, B, C, D, and E) connected by roads. In this example, there are no observers present. The figures show a party agent starting at ELA A and moving through the network for two ticks. The agent's position is marked with a square. Visited ELAs are red, while unvisited ELAs are marked in blue. The distances between ELAs are marked on the roads between them. Although not marked, the agent could travel from A or B to C or D in a single tick by passing through E without stopping. However, for this simple example, the agent will always prefer the shorter, single edge paths. At time zero, the agent starts at A, and considers its options of distances: $(B = 3, C = 11, D = 12, E = 5)$. Since B has the smallest value, it moves to B in tick 1. The agent then reviews the options: $(C = 9, D = 10, E = 3)$. As E is the minimum distance, the agent moves to E, and the simulation ends.

Figure 4 shows the party agent's movements when observers are placed at ELAs C and E, indicated by large circles. The agent does not know that there are observers at these locations in advance. In the first time period, the agent makes the same choice as before and moves to B. In the second period, the agent again chooses E, but because an observer is located at E, the agent

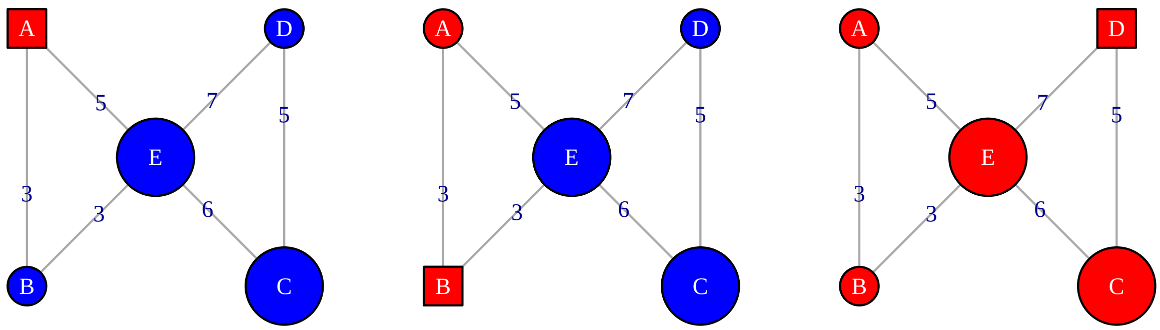

Figure 4: Agent movement rules when observers are present. Squares indicate the agent's current location. Red ELAs are visited. Blue ELAs are not yet visited. The large circles indicate observer ELAs. From left to right: 1) $t = 0$, the agent starts at A, 2) agent selects B as closest ELA, 3) Agent moves to E, but as an observer is present, immediately moves to C, again encounters an observer, and finally stops at D.

immediately moves to a new location. As A and B have already been visited the agent selects between $(C = 6, D = 7)$. It chooses C, but again finds an observer and must go to D. As D does not have an observer, the agent stops there, ending the simulation.

To summarize, this framework has five parameters:

k The total number of agents.

t The total number of "ticks," or time periods, in which agents can visit ELAs.

τ The number of false registrants an agent can add to an unobserved ELA.

Movement rule The rule by which agents move to their next target. If an agent cannot move to another ELA, he becomes inactive for the remainder of the registration period.

Starting location rule For now, we imagine that the agents can rank all of the ELAs according to how easy it would be to inflate the register in those places. If they can do this, then we posit that they will choose the most desirable ELA as their starting positions.

This raises the following question: what makes a registration center (ELA) attractive to party agents? The theoretical discussion above suggests and distinguishes between two different models of party agent behavior – one that explicitly considers the ethnic composition of the areas around a registration center and another that only considers travel distance. We will show that these models imply different patterns of outcomes for the ELAs. Therefore, our observed data will allow us to assess whether and how each of the models may be a candidate explanation for registration rigging, and extend our understanding of how ethnicity affects political party behavior in new democracies.

3.1 Geographical Distance Minimizing Model

In this model, agents only wish to minimize the distance travelled from one ELA to another. Agents must visit t locations, but they try to do so by taking short steps. In this model, agents are not optimizing their entire route. At each tick, the agent considers all neighboring ELAs and selects the closest. If the ELA contains an observer, the agent chooses a new location by the same decision rule. This extra move counts as part of the same round or "tick." This greedy approach may result in longer routes than would be available if the agent considered all possible routes of size t from a given starting location.

Since the party agents wish to minimize distance in one step increments, we select starting locations based on the closeness to other ELAs. That is, the most desirable ELAs are those from which it would easiest to travel to other ELAs. ELAs are ranked by their minimum distance neighbor, and the first k are selected as starting locations for the agents.

3.2 Ethnic Homogeneity Preference Model

Like the distance minimizing model, party agents in this model have an eye towards to keeping their total travel distance small but have the additional desire to visit ethnically homogeneous areas. For each ELA, we compute the ethnic compositions over 8 groups and a residual category. Each ELA is then given an ethnic fractionalization score based on the percentages (p) of each ethnic group:

$$F = 1 - \sum_{i=1}^{9} p_i^2 \tag{1}$$

The value of F is 0 when there is only one ethnic group in an area and 0.89 when all 9 ethnic groups (including the residual category) are equally balanced. In this simulation, agents prefer to visit ELAs that have values of F close to zero. At the beginning of the registration period, the agents rank the ELAs by F rather than by centrality in a dense road network, and k agents go to the k ELAs with the smallest values of F.

At each step of the process, agents look at the value of $F_{current}$ for their current location and only consider moving to an ELA with $F_{move} \leq F_{current} + 0.05$. Among these, an agent picks the closest ELA in terms of road distance. If no ELAs have sufficiently low values of F, the agent is stuck and stops his efforts to add false registrants. As before, if an agent moves to an ELA with an observer, he immediately moves, choosing the closest location with $F_{move+1} \leq F_{move} + 0.05$.

4 Assessing the evidence against a model

Following Bowers, Fredrickson and Panagopoulos (2013), we assess the empirical evidence against our model within a framework of Fisherian hypothesis testing. We first describe the general approach with a simple example, before applying the method to the more complicated model of political party agents.

In Fisher's original framework for hypothesis testing, a hypothesis is a statement about a counter-factual for each unit in the study. The most famous such hypothesis is the sharp null hypothesis of no effects, which we might write in terms of potential outcomes following Rosenbaum (cite) as $H_0 : y_{i,1} = y_{i,0}$. Notice that this hypothesis is very similar to our model of constant additive effects, which only differs because we added a parameter. In general, any model which generates a counter-factual statement for all units in a study can be understood to be, in essence, a sharp hypothesis generator. We can therefore engage such models using very well established procedures for hypothesis testing in a randomized study.[5]

Recall our simple model of constant, additive effects for a unit i: $y_{i,1} = y_{i,0} + \tau$. This model encodes not only a hunch about how the treatment operated (here, via simply adding τ to all the potential outcomes under control), but also a statement that the potential outcomes of unit i do not depend on the potential outcomes of other units. What does this model imply if τ were 100? Figure 5 shows one vision of what this model implies. On the left hand side we see the outcomes in the control and treated groups compared just as they were in Figure 2. The right hand side shows the two outcome distributions after removing $\tau = 100$ from the treated group. If the simple model described the data well, then removing 100 from each and every unit in the treated group would make the treated and control groups look like they were random samples drawn from the same population, the set of ELAs in the study. Note that this model shifted the treated group distribution down, but it did not appreciably change the distribution of the treated group to make it look especially similar to the control group. The graphical evidence here suggests that our observed data (shown at left) would be more surprising from the perspective of hypothesis of $\tau = 100$ given the model $y_{i,1} = y_{i,0} + \tau$ than would be the hypothesis that $\tau = 0$ (i.e., the sharp null hypothesis of no effects). In fact, when we compare our observed data to what would be implied by each hypothesis, $\tau = 0$ and $\tau = 100$, we learn that it would be quite surprising to see our observed data from the perspective of either hypothesis, with $p = 0.014$ for $H_0 : \tau = 0$ and $p = 0$ for $H_0 : \tau = 100$.[6] Notice that the low p-values refer to both the model and the particular parameters of the model here.

[5] See Rosenbaum (2010, Chapter 2) for a clear and modern description of Fisher's approach to statistical inference. Of course, Fisher's own "Lady Tasting Tea" example is also highly recommended (Fisher, 1935, Chapter 2).

[6] We are glossing over the choice of test statistic for this example. Here we use the KS test statistic which gauges the difference between two distributions using the maximum distance between the ECDFs of two distributions. We repeated the sampling process (in blocks, etc..) 1000 times and recorded the proportion of the distribution of the distribution representing the hypothesis greater than or equal than our observed value as the p-value. Thus, we make no large sample assumptions here.

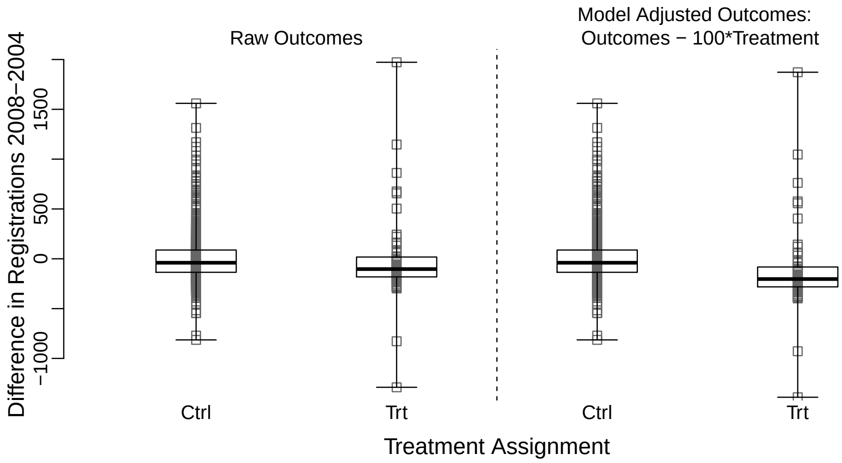

Figure 5: An example of outcomes implied by a model. Left boxplots show original outcomes. Right boxplots show the outcomes implied by $y_{i,1} = y_{i,0} + \tau$, $\tau = 100$.

	Minimum	Maximum
k	1.00	797.00
ticks	1.00	10.00
tau	-25.00	500.00

Table 1: Parameter range investigated for the two models. k is the number of party agents. t is the number of iterations of the simulation or number of ELAs at which registration rigging would have been attempted. τ is the number of false registrations each party agent can add to an ELA he visits.

Now, let us return to our models of party agents engaged in inflating the voters register. Recall that our models are broadly distinguished from each other by movement and starting rules that consider distance only or also consider the ethnic homogeneity of the area, and that for a given selection of τ, k, t, our models generate a specific number of false registrations for each ELA, V_i, which we collect into a vector $\mathbf{V}$. To assess the evidence against our models, we select a range of values for k, t, and τ, as shown in Table 1. Then for each model and set of parameters, we run two simulations. In the first simulation, we model the agents behavior in the observed experiment in which observers are placed at the ELAs assigned to treatment. The agents are placed in their starting locations and proceed to visit ELAs according to the decision rules in the model. If an agent encounters an observer, he immediately moves to another location within the same "tick" period. Once the simulation has worked through the iterations, we have a count of the number of times each ELA was visited by agents for that model and parameter set.

Then we remove all the observers and repeat the simulation. This second simulation corresponds to the world in which the experiment was never run. Agents start at the same location, but because there are no observers, they will stop at one ELA per tick and register false names. At the end of this simulation we again have a list of how many times each ELA was visited for this world with no observers.

Using these two sets of counts, which we call $\mathbf{V_z}$ and $\mathbf{V_0}$, respectively, for the experimental and "uniformity trial" with no observers, we can adjust the observed outcome to the outcome that would have been observed if no experiment had been conducted. If our models were true, or a good approximation to the truth, the treated and control groups would be random samples from the observer-less outcome. We generate this outcome by asking, "what is the difference between the observed (experimental) data and the world in which no experiment had occurred?" In these models, if an observer were at a site during the intervention ($Z_i = 1$), agents are unable to change the number of registrations at that ELA. However, if an agent had not been present, the registration count would have been inflated by τ for each agent, or $\tau V_{\mathbf{0},i}$. Therefore, for any treated unit, its outcome without any observers present would have been $y_{i,\mathbf{0}} = y_{i,\mathbf{z}} + \tau V_{i,\mathbf{0}} \,|\, z_i = 1$.

Conversely, if an observer was not present at ELA i in the experiment, in both worlds all agents were able to register τ false names. Thus, to adjust the observed outcome to get the observer-less outcome, we care about the difference in the number of agents that visited in the experiment as compared to the world in which no observers were present. For each additional agent that visited during the experiment, τ additional false registrations were added, so we can adjust the observed outcome to the unobserved one as $y_{i,\mathbf{0}} = y_{i,\mathbf{z}} - \tau(V_{i,\mathbf{z}} - V_{i,\mathbf{0}}) \,|\, z_i = 0$. We can combine this statement with that for the treated units to achieve the overall model of effects:

$$\mathbf{y_0} = \mathbf{y_z} + \tau\left(\mathbf{z}\mathbf{V_0} - (1-\mathbf{z})(\mathbf{V_z} - \mathbf{V_0})\right) = \mathbf{y_z} + \tau\left(\mathbf{V_z}(\mathbf{z}-1) + \mathbf{V_0}\right) \tag{2}$$

This model of effects can be illustrated by again referring to Figures 3 and 4. In Figure 3, which corresponds to the uniformity trial with no observers, the party agent visited (A, B, E). In the second figure, which corresponds to the experimental trial, the agent visited (A, B, E, C, D). The effect of the observers at C and E is the difference between these two paths. The agent visited both A and B in both worlds, and neither ELA had an observer. Therefore, the outcome $y_{A,\mathbf{0}} = y_{A,\mathbf{z}}$ and $y_{B,\mathbf{0}} = y_{B,\mathbf{z}}$, and no adjustment is necessary. In the uniformity trial (the world without observers), E was visited by a party agent who was able to add false registrations. In the experiment, E was again visited by a party agent, but he encountered an observer and was unable to register voters. Therefore the outcome of this ELA is $y_{E,\mathbf{0}} = y_{E,\mathbf{z}} + \tau$. C was not visited by the agent in the uniformity trial, and although C was visited by the agent in the experiment, the observer prevented the addition of false registrations. Therefore, no adjustment is necessary for C. Finally, the agent did not have time to visit D in the uniformity trial, but because observers deterred activity at E and C, D was visited by the agent in the experiment. Thus, D's outcome is $y_{D,\mathbf{0}} = y_{D,\mathbf{z}} - \tau$. By repeating this logic for all the agents in the model, we can adjust our observed outcome in the experiment to that which would have been observed in the uniformity trial.

We apply the methods of Bowers, Fredrickson and Panagopoulos (2013) to test the hypotheses generated by our models of party agents. In addition to the model of effects in Equation 2, this statistical approach requires that we define how the treatment was randomly allocated and provide a test statistic to score the residual differences between the treated and control groups that remain after adjusting for the hypothesized effects of the agents. The randomization procedure was a hierarchical, blocked randomization, as discussed in Section 2.2. For a test statistic, we elect to look at the KS test-statistic as described above after adjustment.[7] Our model implies a shift in the distribution of treated units in the presence of observers, but with several ELAs with very large numbers of registrations, looking at the means of the two groups may hide the true effect. The results of these statistical tests are discussed in the next section.

5 Analysis

We start by asking, "What is the probability of seeing the observed difference between the treated and control groups, *if the observers had no effect at all?*" This particular hypothesis is frequently referred to as "the sharp null of no effect." If this hypothesis were true (i.e., observers had no effect), any difference in the observed difference in between the two groups is purely due to the chance introduced by random assignment. In fact, if we were to reassign treatment repeatedly using the same blocked design, we could exactly compute the distribution of differences that would occur just due to chance in this study, conditional on observers having no effect. By comparing the observed difference between the treated and control groups to this distribution, we have an answer to our question: if the observers truly had no effect, the probability of seeing a test-statistic between the treated and control groups of 0.2 or greater is 0.019, an event that would happen in fewer than 1 in 52 of the possible ways that treatment could have been randomized in this study.[8]

We now turn our attention to the question: "Which combinations of parameters (for each model) would have been surprising given the observed data?" As with the sharp null, we compare the observed test statistic to the values that the test statistic would have taken on purely due to chance through random assignment, adjusting for the effect implied by each set of parameters as in Equation 2. Figures 6 and 7 display the 95% confidence regions over (τ, k) for different values of t. For the distance-minimizing model, the vector of parameters that maximizes the p-value is $(k = 200, t = 9, \tau = 18)$, with a p-value of $p = 0.999$ — recall that t is number of ELAs visited, τ is the number of registrations inflated, and k is the number of party agents aiming to rig the

[7] We plan to use higher power test-statistics in the next iteration of this paper.

[8] There are several parameter combinations for the our models that generate the same adjustment as the sharp null (i.e., no adjustment at all). Whenever the number of agents is zero ($k = 0$), the number of ELAs visited is zero ($t = 0$) or the effect an agent is zero, ($\tau = 0$), both models imply no adjustment to the data. The models also may imply similar adjustments when these parameters take on other, non-degenerate values, but as the parameter interact with the network structure and ethnic composition of the election registration sites, such similarities are difficult to deduce.

registration process. For the ethnicity/homogeneous-seeking model, the highest p-value is acheived at $(k = 797, t = 1, \tau = 29)$ with a $p = 0.962$.

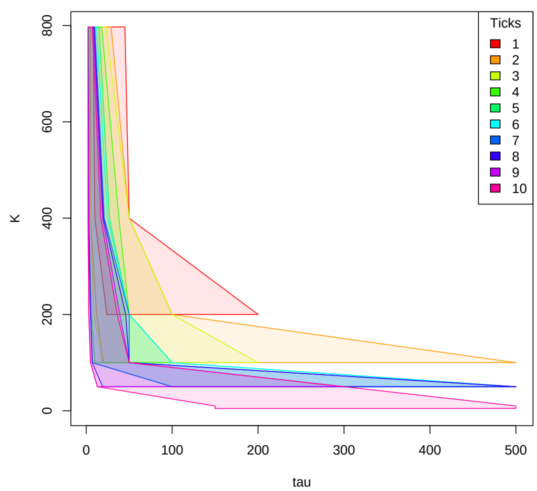

Figure 6: 95% confidence regions for the distance minimizing model for the parameters k and τ. Each colored region represents a different value of t. White space indicates collections of parameter values with p-value below .05.

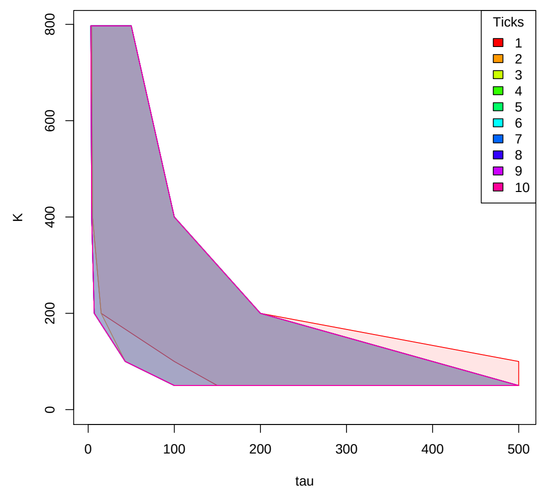

Figure 7: 95% confidence regions for the homogeneous ethnic composition seeking model for the parameters k and τ. Each colored region represents a different value of t. White space indicates collections of parameter values with p-values below .05.

With this test statistic, the idea that many party agents (k) added large numbers of false registrations at each registration center visited (τ) as represented in these models has no support in these data. For example, neither model would be plausible with 800 agents, each inflating the voters register by 250 at each registration center visited. However, the evidence against these models in the data is similar in broad contours. We see, for example, that if many agents were to work to rig the election, then it would be reasonable for each agent to only have a relatively small effect on any given place (no more than $\tau = 100$). Similarly, if parties deployed few agents (say, fewer than 200 for the 33 constituencies in our sample), then we could not exclude the idea that each of the agents had a large effect on each ELA that they visited. Unfortunately, we do not have any direct information about the number of party agents working to inflate the voters registers in 2008. But by excluding implausible sets of parameters represented as white space, our model provides the rough contours of what kinds of patterns might be plausible. One small difference between the models is that the ethnicity models excludes the possibility of zero party agents (the white space on the bottom of the plot) whereas the distance model only excludes that idea when the distance possible to travel, t, is small. If party agents were to travel long distances, then, according to the distance model, it would not be too surprising to see very few party agents involved in registration rigging.

The two models differ only in their sensitivity to the number of ELAs visited by an agent (t). The data and test statistic do not differentiate between the implications of the ethnicity model for different values of t; the confidence regions at different values of t overlap more or less completely. This kind of result — where tests of the model report the same amount of evidence against of the model for a range of parameter values — suggests that our model is not capturing the dynamics of registration rigging well or that our test statistic is particularly low powered against the kinds of alternatives that we see here. Since we see that the test statistic is able to exclude entire ranges of parameters in general, we suspect that this kind of parameter insensitivity is due to the model.

The minimum distance model shows much more sensitivity in general and to the t parameter in particular than the ethnicity model. For example, as Figure 8 shows, if agents were to travel to only one other place, then it would be implausible to see fewer than about 200 agents (see the lower left hand panel in the figure).

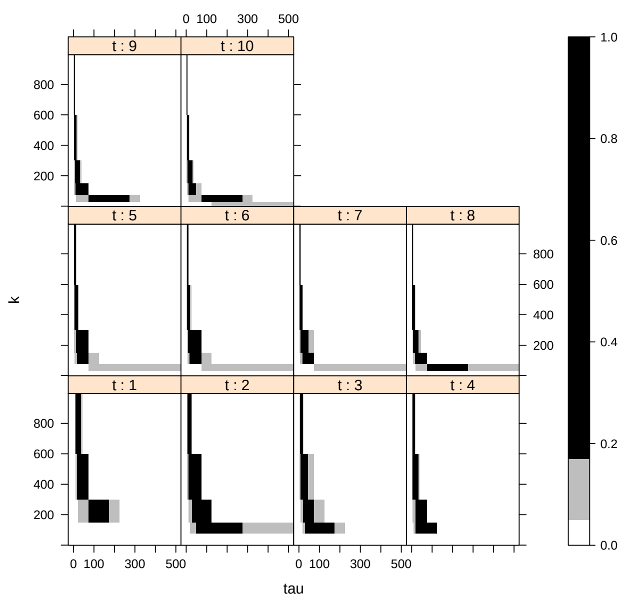

Figure 8: 95% and 66% confidence regions for the minimum distance model with different regions for different "tick" sizes t. Black indicates p-values above .18, gray between .05 and .18, and white indicates p-values less than .05.

6 Discussion and Conclusion

This work in progress combines a new way of writing models with a new way of assessing their relationship to data. The goal is to infer about a model from a randomized experiment, and our tests of the models, as shown in the previous figures, teaches us about the models, not primarily about the observations. These models involve parameters representing the unobserved movement of party agents along road networks in Ghana in response to observers at the registration stations, as well to the features of the network itself like the density of the connections and the ethnic homogeneity of the locations.

Although we need to work to build models with better parameter sensitivity, we can already exclude certain ranges of parameters and their implications from these models: that is, any model implying very large scale fraud with, say, many hundreds of agents who each manage to inflate the election rolls by many hundreds of false registrants would not be supported by the data.

Our next steps here involve both focusing on the model building itself, but also attempting to improve the power of our tests using better test statistics, and perhaps to delve deeper into reports of electoral irregularities surrounding this voter registration exercise in order to fine tune the details of the models.

Finally, it is worth noting that the ethnicity model, while fairly insensitive, both excludes and includes some collections of parameter values. This suggests that models in which ethnicity plays a role cannot be ruled out wholesale even if, for now, they do not appear to improve substantially on our simpler model of displacement over road networks.

References

Asunka, Joseph, Sarah Brierley, Miriam Golden, Eric Kramon and George Ofosu. 2013. "Electoral Violence and Fraud in the 2012 Ghanaian Elections: Polling Station Results." Paper prepared for presentation at a Workshop on Electoral Integrity, Violence, and Vote-Buying, Center on Democracy, Development, and the Rule of Law, Program on Poverty and Governance, Stanford University, April 12-13, 2013.

Basedau, Matthias, Matthijs Bogaards, Christof Hartmann and Peter Niesen. 2007. "Ethnic Party Bans In Africa: A Research Agenda." *German Law Journal* 8(6):617–34.

Bates, Robert H. 1983. Modernization, Ethnic Competition, and the Rationality of Politics in Contemporary Africa. In *State Versus Ethnic Claims: African Policy Dilemmas*, ed. Donald Rothchild and Victor A. Olorunsola. Boulder, CO: Westview Press.

Birch, Sarah. 2008. "Electoral institutions and popular confidence in electoral processes: A cross-national analysis." *Electoral Studies* 27:305–20.

Bowers, Jake, Mark M. Fredrickson and Costas Panagopoulos. 2013. "Reasoning about Interference Between Units: A General Framework." *Political Analysis* 21(1):97–124.

Brady, Henry E. 2008. "Causation and explanation in social science." *Oxford handbook of political methodology* pp. 217–270.

Brancati, Dawn. 2012. "Building Confidence in Elections: The Case of Electoral Monitors in Kosova." Working paper, Washington University in St. Louis.

Chandra, Kanchan. 2004. *Why ethnic parties succeed: patronage and ethnic headcounts in India.* Cambridge: Cambridge University Press.

Collier, Paul and Pedro C. Vicente. 2011. "Votes and Violence: Experimental Evidence from a Nigerian Election." Working Paper, University of Oxford.

Collier, Paul and Pedro C. Vicente. 2012. "Violence, Bribery, and Fraud: The Political Economy of Elections in Sub-Saharan Africa." *Public Choice* 153(1-2):117–47.

Conroy-Krutz, Jeffrey. 2013. "Information and Ethnic Politics in Africa." *British Journal of Political Science* 43:345–373.

Driscoll, Jesse and F. Daniel Hidalgo. 2009. "The Intended and Unintended Effects of Democratization Aid: Experimental Evidence from the Republic of Georgia." Presented at the 2009 Annual Meetings of the American Political Science Association.

Eifert, Benn, Edward Miguel and Daniel N. Posner. 2010. "Political Competition and Ethnic Identification in Africa." *American Journal of Political Science* 54(2):494–510.

Elklit, Jorgen and Andrew Reynolds. 2002. "The Impact of Election Adminsitration on the Legitimacy of Emerging Democracies: A New Comparative Politics Research Agenda." *Commonwealth & Comparative Politics* 40(2):86–119.

Enikolopov, Ruben, Vasily Korovkina, Maria Petrova, Konstantin Sonin and Alexei Zakharov. 2013. "Field experiment estimate of electoral fraud in Russian parliamentary elections." *Proceedings of the National Academy of Sciences* 110(2):448–52.

Epstein, Joshua M., ed. 2006. *Generative Social Science: Studies in Agent-Based Computational Modeling.* Princeton University Press.

Epstein, Joshua M. and Robert Axtell. 1996. *Growing artificial societies: social science from the bottom up.* Washington, DC: Brookings Institution Press.

Ferree, Karen E. 2010. *Framing the Race in South Africa: The Political Origins of Racial Census Elections.* New York: Cambridge University Press.

Fisher, R.A. 1935. *The design of experiments. 1935.* Edinburgh: Oliver and Boyd.

Hyde, Susan D. 2007. "The Observer Effect in International Politics: Evidence from a Natural Experiment." *World Politics* 60:37–63.

Ichino, Nahomi and Matthias Schündeln. 2012. "Deterring or Displacing Electoral Irregularities? Spillover Effects of Observers in a Randomized Field Experiment in Ghana." *Journal of Politics* 74(1):292–307.

Ichino, Nahomi and Noah L. Nathan. 2013. "Crossing the Line: Local Ethnic Geography and Voting in Ghana." *American Political Science Review* 107(2):344–361.

Kramon, Eric. 2011. "Why Do Politicians Buy Votes When The Ballot Is Secret? Theory And Experimental Evidence From Kenya." Working Paper, UCLA.

LeBas, Adrienne. 2006. "Polarization as Craft: Party Formation and State Violence in Zimbabwe." *Comparative Politics* 38(4):419–438.

Lindberg, Staffan I. and Minion K. C. Morrison. 2005. "Exploring voter alignments in Africa: core and swing voters in Ghana." *Journal of Modern African Studies* 43(4):565 – 586.

McCann, James A. and Jorge I. Dominguez. 1998. "Mexicans React to Electoral Fraud and Political Corruption: an Assessment of Public Opinion and Voting Behavior." *Electoral Studies* 17(4):483–503.

Neyman, J. 1923 [1990]. "On the application of probability theory to agricultural experiments. Essay on principles. Section 9 (1923)." *Statistical Science* 5:463–480. reprint. Transl. by Dabrowska and Speed.

Nugent, Paul. 2001. "Ethnicity as an Explanatory Factor in the Ghana 2000 Elections." *African Issues* 29(1/2):2–27.

Padro i Miquel, Gerard. 2007. "The Control of Politicians in Divided Societies: The Politics of Fear." *Review of Economic Studies* 74:1259–74.

Posner, Daniel N. 2005. *Institutions and Ethnic Politics in Africa.* New York: Cambridge University Press.

Rose, Richard and William Mishler. 2009. “How Do Electors Respond to an “Unfair” Election? The Experience of Russians.” *Post-Soviet Affairs* 25(2):118–36.

Rosenbaum, Paul R. 2010. *Design of Observational Studies*. Springer.
URL: *http://www.springer.com/statistics/statistical+theory+and+methods/book/978-1-4419-1212-1*

Rubin, D.B. 1974. “Estimating the Causal Effects of Treatments in Randomized and Nonrandomized Studies.” *J. Educ. Psych.* 66:688–701.

Schedler, Andreas. 2002. “The Menu of Manipulation.” *Journal of Democracy* 13(2):36–50.

Sekhon, J.S. 2008. “The Neyman-Rubin model of causal inference and estimation via matching methods.” *The Oxford handbook of political methodology* pp. 271–299.

Simpser, Alberto. 2012. “Does Electoral Manipulation Discourage Voter Turnout? Evidence from Mexico.” *Journal of Politics* 74(3):782–795.

Sjoberg, Fredrik M. 2012. “Making Voters Count: Evidence form Field Experiments about the Efficacy of Domestic Election Observation.” Harriman Institute Working Paper No 1, Columbia University.

Vicente, Pedro C. Forthcoming. “Is Vote Buying Effective? Evidence from a Field Experiment in West Africa.” *Economic Journal* .

Wantchekon, Leonard. 2003. “Clientelism and Voting Behavior: Evidence from a Field Experiment in Benin.” *World Politics* 55(3):399–422.