

# The Ethics of Electoral Experimentation: Design-Based Recommendations

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## Abstract

While experiments on elections represent a popular tool in social science, the possibility that experimental interventions could affect who wins office remains a central ethical concern. I formally characterize electoral experimental designs to derive an upper bound on aggregate electoral impact under different assumptions about interference. I then introduce a decision rule based on comparison of this bound to predicted election outcomes to determine whether an experiment should be implemented. I demonstrate that existing experiments vary substantially in their (*ex-ante*) risk of changing aggregate electoral outcomes. Researchers can mitigate the possibility of affecting aggregate outcomes by reducing the saturation of treatment or focusing experiments in districts where treated voters are unlikely to be pivotal. These conditions identify novel trade-offs between adhering to ethical commitments and the statistical power and external validity of electoral experiments. More broadly, I show that deliberate research design can address some ethical concerns with experiments.

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# 1 Introduction

Experiments on real elections represent a popular tool in studies of elections, political behavior, and political accountability. While the use of experiments on elections dates back nearly a century to Gosnell (1926), the scale, sophistication, and frequency of electoral experiments has increased precipitously since the late 1990s. A central ethical concern in the study of electoral experiments is that by manipulating characteristics of campaigns, candidates, or voter information, researchers may also be changing aggregate election outcomes.

Two notable changes in experimental studies of elections over the past two decades influence these ethical considerations. First, researchers now work in contexts with greater variation in electoral institutions and voting behavior than pre-2000 studies of elections in US college towns.<sup>1</sup> To the extent that subjects and other residents of their districts internalize the consequences of election outcomes, intervening in different types of elections presents different levels of risks to subjects. Second, the scale of electoral interventions, measured in terms of the number of treated voters, has increased precipitously. In addition to academic researchers, campaigns and technology companies now regularly implement massive experimental interventions in elections (see, for example, Pons, 2018; Bond et al., 2012).

I focus on the ethical concern that experimental manipulations may alter aggregate election outcomes. This concern is not new. For example, Dunning et al. (2019) write that the authors of seven coordinated experiments on elections and accountability “elaborated research designs to ensure to the maximum extent possible that our studies would not affect aggregate election outcomes” (52). Recently-adopted American Political Science Association (APSA) “Principles and Guidance for Human Subjects Research” echo this concern writing that interventions are of “minimal social risk if they are not done at a scale liable to alter electoral outcomes” (American Political Science Association, 2020: p. 15). Moreover, the concern is articulated in recent work on experimental ethics

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<sup>1</sup>Early (pre-2000) experiments from Gosnell (1926) to Gerber and Green (2000) occurred in local elections in jurisdictions where researchers worked, namely Chicago, Ann Arbor, and New Haven.

in political science (Desposato, 2016; McDermott and Hatemi, 2020; Phillips, 2021). In an original survey of pre-registrations and pre-analysis plans in the Evidence in Governance and Politics (EGAP) and American Economic Association (AEA) registries, I identify 129 experiments on real elections (see Appendix A2). Just two discuss this potential concern in their written *ex-ante* design considerations (Table A4). This article proposes a formal, design-based approach to the *ex-ante* consideration of how experimental interventions could affect aggregate election outcomes for use in planning electoral experiments.

The ethical considerations related to experimental research on elections are admittedly far more complex than the focus on aggregate electoral outcomes in this paper. Notably, Humphreys (2015), Desposato (2016, 2018), and Teele (2019) raise questions about standards for consent in field experiments, including those on elections. Consent issues represent a distinct ethical consideration from those that I discuss. However, this paper develops other themes articulated in recent literature. Like MacKay (2017), Whitfield (2019) and Asiedu et al. (2021), I emphasize the role of researcher uncertainty over the possible harms and benefits to experimental subjects (and non-subjects) in justifying the adoption of experimental designs. In experimental studies of elections, Carlson's (2020) caution that risks to subjects may outweigh the learning benefits in the presence of general equilibrium effects is salient.

I provide a new justification for efforts to avoid changing electoral outcomes by focusing on researcher uncertainty about the distribution of harms and benefits of different electoral outcomes as both a justification for random assignment of treatment and a rationale for not changing electoral outcomes. I then consider how these ethical considerations change when researchers collaborate with a partner, like an NGO, candidate, or party. In these cases, the random assignment of a partner's intervention in an experiment can change aggregate outcomes relative to the partner's non-experimental treatment allocation (the counterfactual to an experiment). In these collaborations, researchers may study the effects of their partners' interventions intended to change election outcomes. However, these changes in aggregate outcomes should not stem from the re-allocation of treatment necessary to conduct the experimental research.

Minimizing the possibility of changing aggregate electoral outcomes requires two departures from standard practice in the design and analysis of experiments. First, consideration of election outcomes requires aggregation to the level of the *district*. The district is rarely the level at which treatment is assigned or outcomes are analyzed. The frequent omission of information about the relationship between the electoral district and experimental units (of assignment or outcome measurement) makes it difficult to estimate *ex-post* the saturation of an intervention in the relevant electorate in many existing studies.

Second, while experiments are powerful tools for estimating various forms of *average* causal effects, the relevant ethical consideration is whether an electoral experiment changes *any* individual election outcome, defined here in terms of who wins office. Yet, such individual (district-level) effects are unobservable due to the fundamental problem of causal inference. Moreover, any *ex-post* attempt to assess electoral impact must acknowledge that the possible consequences of an electoral intervention are set into motion when the experiment goes into the field. For this reason, I suggest that the relevant course of action is to consider the possible impact of an experimental intervention *ex-ante*. In this sense, I examine how to design experiments that are unlikely to change who wins office via the random assignment of treatment.

In response to these concerns, I propose a framework for bounding the maximum aggregate electoral impact of an electoral experiment *ex-ante*. I focus on the design choices made by researchers, namely the selection of districts (races) in which to implement an intervention and the saturation of an intervention within that electorate. With these design choices, I allow for maximum voter agency in response to an electoral intervention through the invocation of “extreme value bounds” introduced by Manski (2003). Combined with assumptions about interference between voters, this framework allows for the calculation of an experiment’s maximum aggregate electoral impact in a district. The relevant determination of whether an intervention should be attempted rests on how this impact compares to predicted electoral outcomes in a district. I propose a decision rule that can be implemented to determine whether or not to run an experimental intervention. I complement the analysis with an R package for easy calculation of these bounds and the decision

rule.

This analysis identifies a set of experimental design decisions that researchers can make to minimize the possibility of changing election outcomes. They can reduce the saturation of treatment in a district by (1) treating fewer voters or (2) intervening in larger districts. Further, they can avoid manipulating interventions in (3) close or unpredictable contests or (4) PR contests. These design principles suggest trade-offs between ethical considerations and learning from electoral experiments.

While the analysis is agnostic with respect to voter responses to an experimental intervention, I show that some assumption restricting interference between voters is necessary for an experiment to ever pass the proposed decision rule. I derive bounds on the maximum electoral impact under the stable unit treatment value assumption (SUTVA) as well as weaker and stronger assumptions about interference. Because these assumptions must be invoked *ex-ante*, more careful consideration of possible general equilibrium effects is critical for assessment of the ethical implications of an experiment.

This paper makes three contributions. First, it develops tools to guide researchers considering prospective interventions on elections, as well as consumers of this research. I show how these considerations depart from current practices in the design and reporting of electoral experiments. Second, I identify a set of trade-offs inherent to the design of electoral experiments that emerge from considerations of aggregate electoral impact. Characterization of these trade-offs allows for a richer discussion about the merits and limitations of experiments on elections as a research design for learning about political behavior, persuasion, and electoral accountability. Finally, I advance the view that ethical considerations should be paramount when designing experimental interventions. To my knowledge, this is the first general framework to incorporate ethical concerns across a range of experimental designs in a common setting (elections). I outline how this basic structure can be extended to inform the design of field experiments in other domains where changing aggregate social or political outcomes represents a salient ethical concern.

## 2 Defining the Ethical Objective

Intervening in elections presents risks for precisely the reasons that we study elections: because “elections have consequences” for governance, policymaking, and welfare.<sup>2</sup> In principle, these consequences constitute a basis for the set of possible harms and benefits to subjects. In considering these outcomes, the electoral setting is unique among other field experiments because elections generate winners and losers through a fixed (known) aggregation mechanism.

In order to justify both the learning benefits of an experiment and randomization as an ethical means of allocating treatment, researchers rely on two forms of uncertainty. First, researchers must have some uncertainty about treatment effects in order to learn. I define learning as any change in (updating of) beliefs on the basis of evidence that is generated. As such, a necessary condition for learning is some uncertainty over how outcomes respond to manipulation of the treatment.<sup>3</sup> This uncertainty is generally acknowledged or implied when motivating a study or justifying its contribution.

Second and more specific to the experimental context, researchers often have uncertainty over the consequences of the intervention for subjects. By consequences, I refer to potential harms and benefits – measured or unmeasured – to subjects and potentially non-subjects. This uncertainty plays an important role in justifying the ethical allocation of treatment by random assignment. In clinical trials, the principle of *equipoise* holds that investigators must be in a “state of genuine uncertainty ... regarding the comparative therapeutic merits of each arm in a trial” (Freedman, 1987: p. 141). This prevents investigators from assigning subjects to a treatment arm that is known to produce superior or inferior health outcomes. In other words, if researchers knew with certainty that some treatment arm would produce better health outcomes, they could not justify withholding that treatment from subjects.

The application of equipoise to social science and public policy evaluation is a source of recent

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<sup>2</sup>Indeed, downstream analyses of electoral experiments do suggest that who wins office is consequential for later policymaking (Gulzar and Khan, 2018; Bidwell, Casey, and Glennerster, 2020).

<sup>3</sup>By “some uncertainty,” I refer to any non-degenerate prior belief over the estimand of interest.

debate (MacKay, 2017; Whitfield, 2019; Asiedu et al., 2021). By invoking equipoise as a justification for random assignment, researchers rely on uncertainty over the possible harms and benefits to subjects. If we were certain that an intervention would improve welfare, we could not justify withholding the treatment from subjects in control.<sup>4</sup> Whitfield (2019) disputes the application of equipoise in political settings as a justification for random assignment because political scientists lack “broad set of firm moral premises about the ethical value of their treatments and outcome variables” (p. 10). I argue instead that consequences of an intervention that are relevant to welfare are not necessarily captured by measured outcome variables. My conception of the consequences of an intervention is instead rooted in latent individual utilities that represent the possible set of harms and benefits to subjects. I contend that researcher uncertainty over these utilities can be used to justify the random assignment of a treatment even in political settings.

What information would be necessary to resolve uncertainty over the consequences of changing an election outcome? Unlike experiments outside of the electoral context, elections clearly define the population that may be impacted by changes in electoral outcomes: all residents of a district. Note, however, that this population is often much larger than the experimental sample. We would then need to know the utilities for the affected individuals under different electoral outcomes. The harms and benefits realized as a function of changing an election outcome can be expressed as differences in these utilities. While some authors advocate qualifying these utilities (e.g., Zimmerman, 2016), ascertaining cardinal utilities under different outcomes is quite difficult, contributing to uncertainty over the consequences of an intervention. One observation is straightforward, however. In contested elections, changing an outcome will generally harm some individuals (i.e., a losing candidate or her supporters) while benefiting others (i.e., a winning candidate or her supporters) (Gubler and Selway, 2016; Zimmerman, 2016). Finally, invocation of a welfare criterion is necessary to weigh the potential harms and benefits across the population. While there are many proposed welfare criteria, different criteria may lead to different assessments about the aggregate

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<sup>4</sup>Asiedu et al. (2021) argues that if such a treatment was scarce, then randomization may be ethical, a point to which I return in the conclusion.

consequences of an intervention.<sup>5</sup>

This uncertainty over the consequences of an intervention presents a tension. If this uncertainty justifies researchers in granting or denying treatment by random assignment, then researchers cannot simultaneously justify changing an election outcome through the intervention. In contrast, if researchers are certain that a specific election outcome is welfare-improving, withholding an intervention that promotes the desired election result to construct a control group may be unethical.

## 2.1 Collaboration with partners

In conducting electoral experiments, researchers routinely collaborate with partners – parties, NGOs, or candidates – by randomly assigning the partner’s intervention(s). The American Political Science Association (2020) and Hyde and Nickerson (2016) suggest that the ethical considerations with these partnerships depart from those of researcher-initiated and implemented interventions. Like researchers, partners are likely uncertain about the efficacy of their interventions and can also benefit from the learning produced by an experiment. However, unlike researchers, these actors have generally made determinations about the normative consequences of their actions. Candidates and parties view winning elections as a goal, even if it harms other candidates and some voters. Anti-corruption NGOs have determined that eliminating corruption would be good, even if the elimination or exposure of corruption could gravely harm individuals engaged in these transactions. As such, there is a mismatch between the researcher’s and partner’s level of certainty about the consequences of an intervention.

I contend that the relevant consideration in cases of partnerships is how aggregate outcomes may be changed by the *random assignment* of treatment. Thus, in the case of a partnership, the researcher is not “responsible” for the use of an intervention in an election. Indeed partners’ interventions often seek to change electoral outcomes. Researchers are instead responsible for how the random assignment of the intervention – in the service of research – changes the allocation of the treatment. In this sense, when collaborating with partners, researchers may be justified in

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<sup>5</sup>Note that some welfare criteria may not require full specification of cardinal utilities. For example, because changing a contested election creates winners and losers, under the Pareto criterion no alternative outcome is preferred to the status quo.



Case	Actors		Experiment	Counterfactual (absent experiment)	Examples
	Researcher	Partner			
1	✓		<i>Researcher designs, implements experimental intervention.</i> (Note: An partner may participate in or endorse the experiment, but the researcher causes the intervention to occur. Such interventions are often, but not necessarily, funded through the researcher.)	<i>No intervention occurs.</i>	Gerber and Green (2000); Metaketa-I experiments documented in Dunning et al. (2019)
2	✓	✓	<i>Researcher randomizes a partner-funded and implemented intervention.</i>	<i>Partner funds and implements intervention without randomizing allocation of treatment, possibly with less data collection.</i>	Bond et al. (2012), Kendall, Nannicini, and Trebbi (2015), Pons (2018), López-Moctezuma et al. (2021)

Table 1: Classification of experiments and their counterfactuals by the actors involved in experimental design and implementation.

studying treatments that change electoral outcomes, so long as these changes are not induced by the research component.

## 2.2 Experiments and their Counterfactuals

The preceding discussion suggests two possible counterfactuals to electoral experiments, as a function of the involvement of a partner. These cases are summarized in Table 1. Case #1 describes electoral experiments in which a researcher designs and implements an intervention that would not have otherwise occurred. Case #2 considers the change in the allocation of a partner’s intervention to accommodate the random assignment of the intervention. As such, the counterfactual is the partner’s allocation of the intervention as opposed to no intervention.

Because of the aggregation of votes to the district level, changes between an experimental and counterfactual allocation of an intervention take three forms, as depicted in Table 2. First and closest to Case #1, the intervention may be implemented in districts that where it would otherwise not have been implemented in the non-experimental regime. Second and generalizing this point, there may be a change in the proportion of a district that is treated (differential saturation). Finally, it may be the case that different voters in a district are treated under an experimental allocation of treatment. Thus, even holding constant the number of treated voters in a district, the types (and thus voting behavior) of treated voters may change when researchers randomly assign treatment.

Ethical concerns about an experiment changing aggregate electoral outcomes should focus on

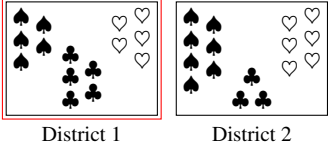
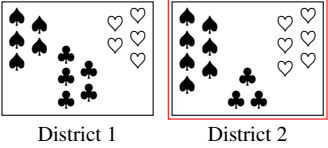
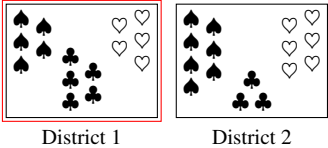
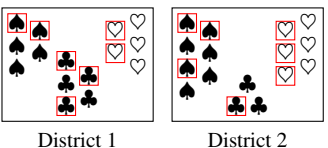
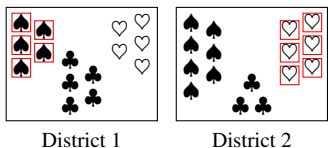
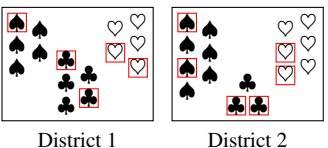
	Counterfactual allocation	Experimental allocation	Description
1	 District 1      District 2	 District 1      District 2	Intervention is implemented in <b>different districts</b> under experimental allocation.
2	 District 1      District 2	 District 1      District 2	Intervention is implemented at <b>different saturation</b> under experimental allocation.
3	 District 1      District 2	 District 1      District 2	Intervention is targeted to <b>different voters</b> under experimental allocation.

Table 2: Each symbol represents a voter. The red boxes indicate treatment assignment.

the difference in treatment allocation between the experiment and its counterfactual. Because of the aggregation of votes at the district level in the context of elections, subjects are not simply “interchangeable” when the allocation of treatment is changed. As such, within this framework, the guideline that “studies of interventions by third parties do not usually invoke [the principle of not impacting political outcomes]” put forth by American Political Science Association (2020: p. 14) is insufficient. Attention to the contrast between experimental and counterfactual allocation of an intervention arguably formalizes Hyde and Nickerson’s (2016) concept of an intermediate level of scrutiny for experiments conducted with partners. In sum, collaboration with a partner does not absolve a researcher from concerns of aggregate electoral impact, though it restricts focus to a more specific set of considerations.

Reporting this counterfactual allocation of treatment is not yet standard practice, making the risks of experiments conducted in partnerships challenging to assess. Two questions are critical. First, how did the experiment change the allocation of the treatment? The 61-million person Facebook mobilization experiment by Bond et al. (2012) included two treatment arms (“control” and “information only”) that Facebook would not have otherwise implemented. Yet only 1% of subjects were assigned to each of these arms, greatly circumscribing the scale of changes in treatment

allocation attributable to the experiment (see A3.3). Second, how similar was the experimental treatment to the intervention that would have been implemented absent the experiment? For example, López-Moctezuma et al. (2021) partnered with two Filipino political parties, but the treatment – deliberative town-hall meetings – were a novel campaign strategy that would not have otherwise occurred (see A3.4). Alternatively, it may be that some specific treatment arms would not be present absent the experiment. In a subset of treatment arms, Bidwell, Casey, and Glennerster (2020) cleverly decompose a candidate debate intervention by exposing voters to specific elements of the bundled intervention. These mechanism-driven treatments were ultimately assigned to a relatively small number of voters, but may have deviated from the broader policy intervention pursued by their NGO partner. When researchers have a role in shaping partners’ interventions, they should justify whether Case #1 or Case #2 better describes the character of the partnership and proceed accordingly.

### **2.3 The Ethical Objective**

I assume researchers’ ethical objective is to avoid changing who ultimately wins office relative to what would have happened absent the experimental allocation of an intervention. As such, researchers would ideally minimize the probability that their interventions change the *ex-post* distribution of seats or offices. In so doing, I assume that the primary electoral consequences on policymaking or governance occur because candidate *A* wins office, not because candidate *A* won office with 60 percent instead of 51 percent of the vote (no mandate effects).

Some literatures suggest that the assumption of no mandate effects may be too stark. Politicians may condition resource allocation on expressed political support, if distributional targets are informed by election outcomes (Lindbeck and Weibull, 1987; Catalinac, Bueno de Mesquita, and Smith, 2020). Alternatively, in uncompetitive electoral autocracies, vote shares – particularly in the form of supermajorities – are argued to signal regime strength (e.g., Simpser, 2013). Changing vote shares in such a context may have consequences for subsequent governance or regime stability.

While this paper abstracts from these types of mandate effects, it also provides the tools to rig-

orously develop such considerations. The calculation of aggregate electoral impact is not affected by the specific ethical objective. The decision rule, however, does depend on how the objective is specified. The exceptions outlined above can be formalized as alternative decision rules in cases where mandate effects are particularly likely or concerning. Thus, this article provides tools that can be developed in the design of experiments in settings where the specific objective – in terms of election outcomes – varies.

There are ethical considerations beyond aggregate electoral impact in all electoral experiments. These include compliance with relevant electoral law (Michelson, 2016), the content of experimental treatments (McDermott and Hatemi, 2020), and issues of consent (Desposato, 2016; Teele, 2019). The focus on aggregate electoral outcomes in this article does not supersede other considerations. Rather, an experiment’s ability to impact electoral outcomes is a function of many experimental design decisions. By developing a framework to organize these decisions, this paper provides guidance for research design to complement guidance on other ethical issues.

The framework starts from the observation that we do not know precisely what an election outcome would be in the absence of an experimental manipulation. This limits our ability to design an experiment to minimize the probability that their interventions change the *ex-post* distribution of office holders or ballot outcomes. As such, this paper advocates the estimation of conservative bounds on the *ex-ante* possible change in vote share. These bounds can be calculated analytically. I then develop a decision rule that compares these bounds to the predicted closeness of an election in order to minimize the risks of altering electoral outcomes. Through these steps, I argue that research can be designed or avoided as to minimize these risks. By reporting these quantities in grant applications, pre-analysis plans, and ultimately research outputs, researchers can transparently justify their design choices.

### **3 Formalizing the Design of Electoral Experiments**

I proceed to construct bounds with three sets of considerations: design decisions made by researchers; researcher assumptions about which voters’ treated potential outcomes are revealed by

the intervention; and a minimal model of voter behavior that is sufficiently general to encompass many types of electoral experiment. Collectively, these considerations allow researchers to calculate a conservative bound on the extent to which an experiment could change election outcomes.

### 3.1 Research Design Decisions

I first consider the components of the research design controlled by the researcher, potentially in collaboration with a partner (as in Case #2). The researcher makes three critical design decisions. First, she controls the set of districts,  $D$ , in which to experimentally manipulate an intervention. Indexing electoral districts by  $d \in D$ , the number of registered voters in each district is denoted  $n_d$ .

Researchers define the clustering of subjects within a district. I assume that voters in district  $d$ , indexed by  $j \in \{1, \dots, n_d\}$  are partitioned into  $C$  exhaustive and mutually exclusive clusters. I index clusters by  $c \in C$  and denote the number of voters in each cluster by  $n_c$ , such that  $\sum_c n_c = n_d$ . In service of generality, there is always a cluster, even when treatments are not cluster-assigned. Individual-level (voter-level) randomization can be accommodated by assuming  $n_c = 1 \forall c$ . Similarly, district-level clustering can be accommodated by assuming  $n_c = n_d$ . In practice, researchers typically assign electoral interventions to individuals or precincts (generally below the district level).

Finally, researchers decide the allocation of treatment within a district. Consider two states of the world,  $E \in \{e, \neg e\}$ , where  $e$  indicates an experiment and  $\neg e$  indicates no experiment. These states represent the counterfactual pairs described in Table 1. Our main potential outcome of interest,  $\pi(E)$  is whether an individual voter is assigned to receive a treatment. In the experiment, allocation occurs via random assignment. Absent an experiment, the treatment could be assigned by any allocation mechanism. This notation allows for characterization of four principal strata, described in Table 3. I use the notation  $S_{11}^{cd}$ ,  $S_{10}^{cd}$ ,  $S_{01}^{cd}$ , and  $S_{00}^{cd}$  to denote the set of voters belonging to each stratum in each cluster and district. The cases defined in Table 1 place assumptions on the relevant strata. Where the counterfactual is no intervention (Case #1), strata where  $\pi(\neg e) = 1$  must be empty.

Stratum		Intervention		Assumptions	
Set	Name	$\pi(e)$	$\pi(\neg e)$	Case 1	Case 2
$S_{11}^{cd}$	Always assigned	1	1	$ S_{11}^{cd}  = 0$	$ S_{11}^{cd}  \geq 0$
$S_{10}^{cd}$	If-experiment assigned	1	0	$ S_{10}^{cd}  > 0$	$ S_{10}^{cd}  \geq 0$
$S_{01}^{cd}$	If non-experiment assigned	0	1	$ S_{01}^{cd}  = 0$	$ S_{01}^{cd}  \geq 0$
$S_{00}^{cd}$	Never assigned	0	0	$ S_{00}^{cd}  > 0$	$ S_{00}^{cd}  \geq 0$

Table 3: Principal strata. Each individual (registered voter) belongs to exactly one stratum. The cases refer to those described in Table 1. The  $|\cdot|$  notation refers to the cardinality of each set, or the number of voters in each stratum in cluster  $c$  in district  $d$ .

With this notation, I proceed by characterizing the proportion of a district’s electorate that is assigned or not assigned to the treatment *because* of the experiment. From Table 3, the relevant strata are  $S_{10}^{cd}$  – individuals exposed to the treatment because it is assigned experimentally – and  $S_{01}^{cd}$  – individuals not exposed to the treatment because is assigned experimentally. The proportion of the electorate in a district that is exposed (resp. not exposed) to an intervention due to the experiment, heretofore the *experimental saturation*,  $\mathcal{S}_d$  can thus be written:

$$\mathcal{S}_d = \frac{\sum_{c \in d} |S_{10}^{cd} \cup S_{01}^{cd}|}{n_d} \quad (1)$$

In the context of electoral interventions that would not occur absent the experiment (Case 1), the interpretation of  $\mathcal{S}_d$  is natural: it represents the proportion of potential (or registered) voters assigned to treatment. For interventions that would occur in the absence of an experiment,  $\mathcal{S}_d$  represents the proportion of potential voters that would (resp. would not) have been exposed to the intervention due to experimental assignment of treatment.

### 3.2 Researcher Assumptions about Interference between Voters

To construct bounds on interference between individuals and clusters, researchers must make some assumptions about the set of voters impacted by an intervention. First, consider the stable unit treatment value assumption (SUTVA), which is typically invoked to justify identification of causal estimands in experimental research. In the setup from the previous section, this means that a

voter’s potential outcomes are independent of the assignment of any other voter outside her cluster, where the cluster represents the unit of assignment as defined above. Denoting a binary treatment,  $Z \in \{0, 1\}$ , SUTVA for electoral outcome  $Y_j(z_{jc})$  is written in Assumption 1.

**Assumption 1.** *SUTVA:*  $Y_{jc}(z_{jc}) = Y_{jc}(z_{jc}, \mathbf{z}_{j,\neg c})$

I add a second *within-cluster* non-interference assumption to the baseline assumption. Note that, in contrast to SUTVA, this assumption is not necessary for identification of the average treatment effect (ATE) in cluster-randomized experiments. This assumption holds that, in the case that treatment is assigned to clusters of more than one voter ( $n_c > 1$ ), a voter’s potential outcomes are independent of the assignment of any other voter inside her cluster, where the cluster represents the unit of assignment to treatment.<sup>6</sup> I express this assumption formally in Assumption 2. In other words, Assumption 2 holds that an intervention could only influence the voting behavior of voters directly allocated to receive the intervention. Analysis of within-cluster “spillover” effects in experiments suggest that this assumption is not always plausible in electoral settings (i.e., Ichino and Schündeln, 2012; Sinclair, McConnell, and Green, 2012; Giné and Mansuri, 2018), so I examine the implications of relaxing this assumption in Section 5.

**Assumption 2.** *No within-cluster interference:*  $Y_{jc}(z_{jc}) = Y_{jc}(z_{jc}, \mathbf{z}_{\neg j,c})$

### 3.3 Voters’ Response to the Treatment

Because the question at hand relates to whether an experimental intervention can change aggregate election outcomes, I focus on voting outcomes. To accommodate the range of interventions in the literature, I assume the potential outcomes framework as a tractable and agnostic model of voting behavior for bounding outcomes. Specifically, given a treatment  $z \in Z$ , I assume that a vote choice potential outcome  $A_{jc}(z) \in \{0, 1\}$  is defined for all  $j, z$ , where 1 corresponds to a vote for the marginal (*ex-ante*) winning candidate and 0 represents any other choice (another candidate, abstention, an invalid ballot, etc.).

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<sup>6</sup>This assumption holds trivially in individually-randomized experiments when  $|n_c| = 1$  or when all registered voters in a cluster are treated.

I bound the plausible treatment effects on vote choice for the marginal “winner” among those whose assignment to treatment is changed by the use of an experiment, i.e. any  $j \in \{S_{10} \cup S_{01}\}$ . Given the binary vote choice outcome, one can bound the possible (unobservable) individual treatment effects, among subjects whose treatment status is changed through the use of an experiment as:  $ITE_{jc} \in \{-a_{jc}(0), 1 - a_{jc}(0)\}$ . If a voter would vote for the winner when untreated ( $a_{jc}(0) = 1$ ), she could be induced to vote for a different candidate  $-a_{jc}(0) = -1$  or continue to support the winner  $1 - a_{jc}(0) = 0$  if treated. Conversely, if a voter would not vote for the winner if untreated ( $a_{jc} = 0$ ), her vote (for any non-winning candidate) could remain unchanged  $-a_{jc}(0) = 0$  or she could be induced to vote for the winner  $1 - a_{jc}(0) = 1$  if treated. Note that these effects serve as the basis for construction of extreme value bounds (EVB) (Manski, 2003).

EVBs can be very wide. Researchers may instead be tempted to use estimates from related studies or less conservative bounding approaches. These approaches may be very misleading. Existing estimates are generally some form of average causal effect (i.e., an  $ATE$ ). The ethical concern is not whether an experimental intervention changes outcomes on average. If  $ATE$  estimates that are small in magnitude mask heterogeneity, bounds based upon existing estimates are non-conservative. Moreover, the outcome measures on vote share for a specific party or incumbent party in the existing literature likely do not uniformly correspond to the relevant *ex-ante* marginal winning candidates. Relative to other bounding approaches, I adopt EVB to avoid incorrect assumptions about the plausible effects of an intervention (i.e., monotonicity). Indeed, Bayesian models of voter updating invoked in information experiments predict non-monotonicity in treatment effects as a function of the location of the signal relative to the prior (i.e. “good” vs. “bad” news).

Voting outcomes are observed at the level at which treatment is assigned, indicated by  $c$ . I assume that in treatment clusters where  $n_c > 0$ , registered voters are randomly sampled to receive the intervention. The expectation of untreated potential outcome  $E[a_c(0)]$  plays an important role in the construction of bounds on aggregate electoral impact. Random sampling ensures that  $E[a_c(0)]$  is equivalent at varying levels of experimental saturation in a treated cluster. This assumption can



be relaxed when it is inappropriate, but the bound on aggregate electoral impact may increase.

## 4 Bounding Effects on Electoral Behavior

### 4.1 Bounding Electoral Impact

Given the design elements characterized by the (experimental) assignment of treatment, researcher assumptions about interference, and the model of voter response to treatment, I proceed to construct an *ex-ante* bound on the largest share of votes that could be changed by an experimental intervention. I term this term, the *maximum aggregate electoral impact* in a district, the  $MAEI_d$ . Under Assumptions 1 and 2, this quantity is defined, by electoral district, as:

**Definition 1.** *Maximal Aggregate Electoral Impact: The ex-ante maximal aggregate electoral impact (MAEI) in district  $d$  is given by:*

$$MAEI_d = \max \left\{ \frac{\sum_{c \in d} [E[a_c(0)] | S_{10}^{cd} \cup S_{01}^{cd}|]}{n_d}, \frac{\sum_{c \in d} [(1 - E[a_c(0)]) | S_{10}^{cd} \cup S_{01}^{cd}|]}{n_d} \right\} \quad (2)$$

Consider the properties of  $MAEI_d$  with respect to untreated levels of support for the winning candidate. Note that  $E[a_c(0)] \in [0, 1]$  for all  $c \in d$ . This has two implications. First, because  $E[a_c(0)]$  is unknown *ex-ante*, a conservative bound can always be achieved by substituting  $E[a_c(0)] = 1$  (equivalently 0). These conservative bounds are useful when the intervention is assigned to a non-random sample of registered voters within a cluster. Second, holding constant the experimental design, the  $MAEI_d$  is minimized where  $E[a_c(0)] = \frac{1}{2}$  for all clusters in a district, with non-empty  $S_{10}^{cd}$  or  $S_{01}^{cd}$ . Thus, going from the least conservative prediction of  $E[a_c(0)] = \frac{1}{2}$  for all  $c$  to the most conservative assumption of  $E[a_c(0)] = 1$  for all  $c$ , the magnitude of  $MAEI_d$  doubles.

Inspection of Definition 1 yields several observations. Most obviously, an identical experiment has less possibility of moving aggregate vote share or turnout in a large district relative to a small district. In other words, the bounds we can place on the electoral impact of the same experimental design are much narrower for a presidential election than for a local school board election. Note

that researchers' desire to work in low-information contexts has directed research focus to legislative or local elections. This result suggests that this decision carries greater risks of changing electoral outcomes, all else equal.

Second, Definition 1 implies that a higher saturation of the experimentally-manipulated treatment increases the potential for effects on vote share, holding constant  $E[a_c(0)]$ . This suggests a trade-off between statistical power and the degree to which an experiment could alter aggregate electoral outcomes. Increasing the saturation of treatment introduces the possibility of changing more votes. For example, changing from an individually-randomized to a cluster-randomized experiment requires many clusters for adequate power to detect treatment effects. When researchers treat large proportions of voters in cluster-randomized experiments, the saturation of treatment increases substantially. Thus, when individual-level outcome data is not available, researchers compensate by cluster-randomizing treatment, often increasing the  $MAEI_d$  substantially.

## 4.2 Assessing the Consequences of Electoral Interventions

The implications of  $E[a_c(0)]$  on  $MAEI_d$  prompt a discussion of the ability of electoral experiments to change electoral outcomes, that is, who wins. While analyses of electoral experiments typically focus on vote share, not probability of victory (or seats won in a proportional representation system), the lever through which elections have consequences is who wins office.

The mapping of votes to an office or (discrete) seats implies the existence of at least one threshold, which, if crossed, yields a different realization of office holding. For example, in a two candidate race without abstention, there exists a threshold at 50 percent. It is useful to denote the “*ex-ante* margin of victory,”  $\psi_d$ , as minimum change in vote share, as a proportion of registered voters, at which a different officeholder would be elected in district  $d$ . In a plurality election for a single seat, the margin of victory terminology is obviously familiar. In a proportional representation (PR) system, there are various interpretations of  $\psi_d$ . Perhaps the most natural interpretation is the smallest change in any party's vote share that would change the distribution of seats.

If  $\psi_d > 2MAEI_d$ , then an experiment could not change the ultimate electoral outcome. In contrast, if  $\psi_d < 2MAEI_d$ , the experiment *could* affect the ultimate electoral outcome. Appendix

A1 shows formally the derivation of this threshold for an  $n$ -candidate race. The intuition behind the result is straightforward:  $n_d\psi_d$  gives the difference in the number of votes between the marginal winning and losing candidates (outcomes). The minimum number of votes that could change the outcome is  $\frac{n_d\psi_d}{2}$  (assuming a fair tie-breaking rule), if all changed votes were transferred from the marginal winner to the marginal loser. Hence, the relevant threshold is  $2MAEI_d$ , not simply  $MAEI_d$ .

Unlike the other parameters of the design,  $E[a_c(0)]$  and  $\psi_d$  are not knowable in advance of an election, when researchers plan and implement an experiment. Imputing the maximum possible value of  $E[a_c(0)] = 1$  allows for construction of the most conservative (widest) bounds on the electoral impact of an experiment under present assumptions, maximizing  $MAEI_d$  while fixing other aspects of the design. However, imputing the minimum value of  $\psi_d = 0$ , the most “conservative” estimate, implies that  $2MAEI_d > \psi_d$  and *any* experiment could change the electoral outcome. Yet, we know empirically that not all elections are close and, in some settings, election outcomes can be predicted with high accuracy. For this reason, bringing pretreatment data to predict these parameters allows researchers to more accurately quantify risk and make design decisions.

To this end, researchers can use available data to predict the parameters  $\psi_d$  and, where relevant,  $E[a_c(0)]$ . Given different election prediction technologies and available information, I remain agnostic as to a general prediction algorithm. Regardless of the method, we are interested in the predictive distribution of  $\psi_d$ ,  $\hat{f}(\psi_d) \sim f(\psi_d|\hat{\theta})$ , where  $\hat{\theta}$  are estimates of the parameters of the predictive model.

### 4.3 Decision Rule: Which (if Any) Experimental Design Should be Implemented?

Ultimately, our assessment of whether an experimental design is *ex-ante* consistent with the ethical standard of not changing aggregate electoral outcomes requires a decision-making rule. I propose the construction of a threshold based on the predictive distribution of  $\psi_d$ . In particular, I suggest that researchers calculate a threshold  $\underline{\psi}_d$ , that satisfies  $\hat{F}^{-1}(0.05) = \underline{\psi}_d$ , where  $\hat{F}^{-1}(\cdot)$  indicates the quantile function of the predictive distribution of  $\psi_d$ . This means that 5% of hypothetical realizations of the election are predicted to be closer than  $\underline{\psi}_d$ . The decision rule then compares

$MAEI_d$  to  $\underline{\psi_d}$ , proceeding with the experimental design only if  $2MAEI_d < \underline{\psi_d}$ .

This decision rule rules out intervention in close elections entirely. It permits experiments with a relatively high experimental saturation of treatment only in predictable “landslide” races. Moreover, basing a decision rule on predictive distribution of  $\psi_d$  as opposed to the point prediction,  $\widehat{\psi_d}$  penalizes uncertainty over the possible distribution of electoral outcomes. Globally, the amount of resources and effort expended on predicting different elections is vastly unequal. As a result, we are able to make relatively more precise predictions in some races in some part of the world than others. Both implications of the decision rule posit implications for the external validity of electoral experiments and the (non)-universal applicability of electoral experiments as a tool, points to which I return in Section 7.

Substantively, this decision rule corresponds to a determination not to change any individual voter’s ability to be pivotal. Variation in pivotality represents one source of political inequality across any electorate. Adherence to the proposed decision rule simply circumscribes researchers’ ability to change (in either direction) the pivotality of a subject or non-subject in an electoral district, limiting researchers’ ability to change the distribution of political power. This connection between pivotality and political equality speaks to concerns enumerated by Beerbohm, Davis, and Kern (2017) about electoral experiments generating various forms of political inequality.

## **5 Relaxing Assumptions of Limited Interference**

Due to the use of extreme value bounds, decisions based on the  $MAEI_d$  are conservative under the assumptions on interference posited Section 3.2. By conservative, I mean that they will induce a researcher to err on the side of not conducting the experiment. Yet, when these assumptions do not obtain, this analysis may justify a non-conservative decision. For this reason, I examine the implications of relaxing these assumptions.

### **5.1 Within-Cluster Interference**

One limitation of the previous analysis, is that an intervention might only change the votes of those that are directly exposed within a cluster (Assumption 2). In this instance, clusters consist

of multiple voters ( $n_c > 1$ ) but not all voters in a treated cluster are treated or untreated due to the experiment. Yet, some “always assigned” (where present) or “never assigned” voters in assigned clusters may change their voting behavior in response to the treatment administered to other voters in their cluster. In electoral context, these spillovers may occur within households (Sinclair, McConnell, and Green, 2012), intra-village geographic clusters (Giné and Mansuri, 2018), or constituencies (Ichino and Schündeln, 2012). In these cases, the maximum aggregate electoral impact with within-cluster interference,  $MAEI_d^w$  can be rewritten as:

$$MAEI_d^w = \max \left\{ \frac{\sum_{c \in d} [E[a_c(0)]n_c I[|S_{10}^{cd} \cup S_{01}^{cd}| > 0]]}{n_d}, \frac{\sum_{c \in d} [(1 - E[a_c(0)])n_c I[|S_{10}^{cd} \cup S_{01}^{cd}| > 0]]}{n_d} \right\} \quad (3)$$

where  $I[\cdot]$  represents an indicator function. Note that the bound  $MAEI_d^w$  maintains SUTVA (Assumption 1).

Two elements change from  $MAEI_d$  to  $MAEI_d^w$ . First, the number of voters whose potential outcomes may be affected by the experimental intervention increases to include all voters in a cluster in which any voter’s assignment status is changed by an experiment. This follows from the fact that  $|S_{10}^{cd} \cup S_{01}^{cd}| \leq n_c$ . Second, the expectation of untreated turnout,  $E[a_c(0)]$  is now evaluated over all registered voters in a cluster (not just subjects). In the context of randomized saturation designs,  $E[a_c(0)]$  does not change because the cluster population is randomly sampled. Random sampling within a cluster is sufficient to ensure that  $MAEI_d^w \geq MAEI_d$ . In other words, within-cluster interference increases the size of the possible electoral impact of an intervention. This analysis implies that if the only form of interference is within-cluster, we can construct a conservative bound on the aggregate impact of an experiment without further assumptions.

## 5.2 Between-Cluster Interference

I now to proceed to relax SUTVA, Assumption 1. Note that SUTVA is typically assumed to justify identification in electoral experiments.<sup>7</sup> In order to account for between-cluster interference, a

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<sup>7</sup>Note that identification of causal estimands is not the concern here. The concern is that some manifestation of the treatment (or response to the treatment) could alter the votes of a growing

violation of SUTVA, I introduce a vector of parameters  $\pi_c \in [0, 1]$ , indexed by  $c$ , to measure researchers' *ex-ante* beliefs about the proportion of voters that could respond to treatment (or some manifestation thereof) in clusters where allocation of the intervention is not changed by the experiment. In experiments in which the intervention would not occur absent the experiment, this term refers to the set of registered voters in control clusters.

$$MAEI_d^{bw} = \max \left\{ \frac{\sum_{c \in d} [E[a_c(0)]n_c I[|S_{10}^{cd} \cup S_{01}^{cd}| > 0] + E[a_c(0)]n_c \pi_c I[|S_{10}^{cd} \cup S_{01}^{cd}| = 0]]}{n_d}, \right. \\ \left. \frac{\sum_{c \in d} [(1 - E[a_c(0)])n_c I[|S_{10}^{cd} \cup S_{01}^{cd}| > 0] + (1 - E[a_c(0)])n_c \pi_c I[|S_{10}^{cd} \cup S_{01}^{cd}| = 0]]}{n_d} \right\} \quad (4)$$

The new term in the numerator of both expressions in Equation 4 reflects the possible changes in turnout in clusters where no subjects' assignment to the intervention is changed due to the experiment. Intuitively, because  $\pi_c \geq 0$ , it must be the case that the aggregate electoral impact of experiments that experience between- and within-cluster interference is greater than those with only within-cluster interference,  $MAEI_d^{bw} \geq MAEI_d^w$ .

Now, consider the implications of conservatively setting  $\pi_c = 1$  for all  $c$ , akin to an assumption that an experiment could affect the potential outcomes of all registered voters in a district. In this case, Equation 4 simplifies to:

$$MAEI_d^{bw} = \max \left\{ \frac{\sum_{c \in d} E[a_c(0)]n_c}{n_d}, \frac{\sum_{c \in d} (1 - E[a_c(0)])n_c}{n_d} \right\} \text{ if } \pi_c = 1 \forall c \quad (5)$$

However, it must always be case that the *ex-ante* margin of victory,  $\psi_d \leq \frac{1}{n_d} \sum_{c \in d} E[a_c(0)]n_c$ , as this represents the case in which the winning candidate wins every vote. It therefore must be the case that if  $\pi_c = 1 \forall c$ ,  $\psi_d \leq 2MAEI_d^{bw}$ . In other words, without circumscribing  $\pi_c$  in some way, we would never satisfy the decision rule proposed in this article in a contested election. As such, a researcher should never run an electoral experiment if she anticipates between-cluster spillover portion of a district.

effects that could reach all voters, even absent problems of identification and inference.

### 5.3 General Equilibrium Effects

The discussion of interference has been agnostic as to the mechanism for between or within-cluster interference. Because of the need to bound  $\pi_c$ , it is useful to consider why more voters may be exposed to some manifestation of the experimental intervention. The causal estimands identified by electoral experiments are generally motivated (explicitly or non-explicitly) as tests of “partial equilibrium” comparative statics in which voters respond to a treatment in isolation. However, other actors – typically candidates, campaigns, or other voters – may also respond to an intervention in attempts to win elections. Such actions change: (1) the treatment bundle received by voters; and (2) the set of voters that receive any part of that bundle. For the researcher designing an experiment, the validity of the present bounding exercise depends on foresight into the set of actors that could respond to treatment and the actions they might take.

Examination of the literature suggests that reactions by other actors can increase or decrease the share of voters exposed to the intervention through the experiment. For example, in an accountability experiment in India, the detention of field staff by acquaintances/affiliates of a candidate and eventually local police curtailed the intervention after less than 10% of the intervention period (Sircar and Chauchard, 2019). In this sense, “general equilibrium” effects ended the intervention, leading to many fewer treated voters than the researchers planned. On the opposite extreme, a postcard intervention insinuating candidate partisanship in a non-partisan Montana judicial election drew the ire of state officials and the attention of national press, plausibly exposing far more than the 14.8% of Montanan registered voters assigned to the intervention to some manifestation thereof.<sup>8</sup>

To the extent that scholars have measured campaign response to voter-level experimental treatments, works like Arias et al. (2019) suggest that incumbents and challengers did choose to amplify or mitigate informational disclosures in an accountability experiment. Importantly, such actions are not precisely targeted to treated voters, suggesting that such responses exposed more voters

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<sup>8</sup>This calculation is based on report of 100,000 fliers in Michelson (2016).

to some manifestation of the intervention than did the researchers. This suggests some  $\pi_c > 0$ , though the plausible range of effects consistent with these measurements is small. Note that if outside actors accurately target general equilibrium responses inside treatment clusters, the bound in Equation 3 is conservative. If, however, such targeting reaches untreated voters outside the cluster, the bound widens. Most challengingly, such a determination must be made before the intervention is fielded.

## 6 Applications: Existing Experiments and Simulation

I now consider how the framework described here can be employed in the planning of an electoral experiment. I first consider the decision rule in light of some specific classes of electoral experiments. I then illustrate several applications of the decision rule in existing experiments. However, relying solely on existing studies directs discussion away from some of the most important design choices in the present framework. Specifically, it does not allow for sufficient discussion of the selection of districts or races in which researchers intervene. These decisions greatly impact the likelihood that an intervention changes aggregate election outcomes. To this end, I simulate a series of experimental research designs using real administrative data that speak to the *ex-ante* application of the decision rule that I advance.

### 6.1 Classes of Electoral Interventions

To this point, the framework developed is silent with regard to the *content* of experimental treatments. To anchor a discussion of different interventions, Figure 1 depicts the distribution of intervention classes within the electoral experiments pre-registered in the AEA and EGAP repositories. I define these classes in Table A2. I consider whether there are specific interventions in which the decision rule is unnecessarily conservative.

First, consider which registered voters' behaviors might be impacted by the intervention. Many of these interventions aim to induce changes in behavior of a subset of the electorate. For example, mobilization (get out the vote) interventions theoretically aim to change the behavior of registered voters who would not otherwise turn out to vote. Persuasion experiments aim to change the votes



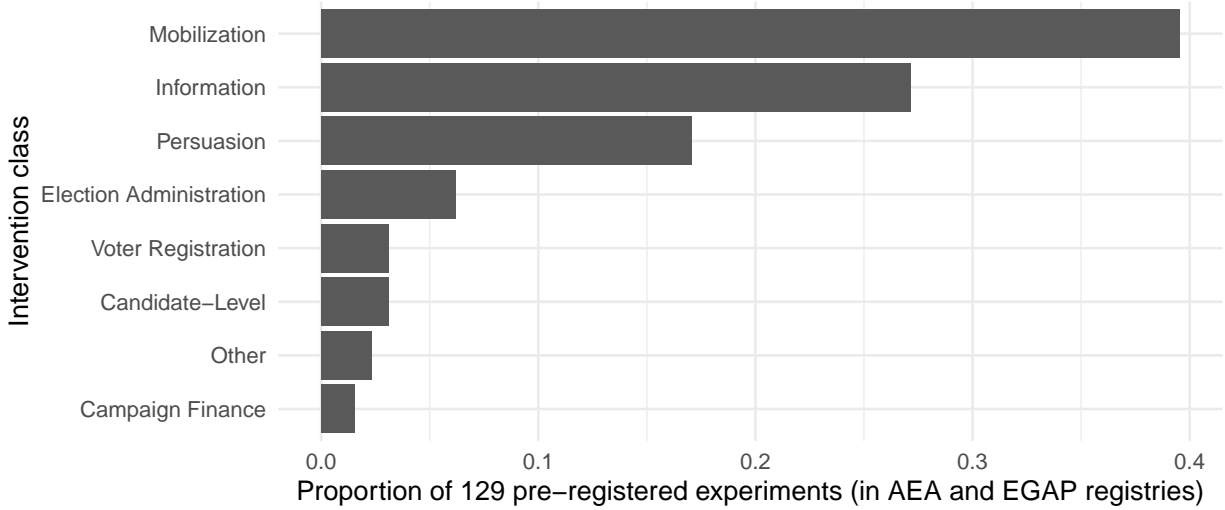


Figure 1: Distribution of intervention classes.

of individuals that intend to vote for a different alternative. Noting these distinct aims, recall that the decision rule,  $2MAEI_d < \underline{\psi}_d$ , allows for the fact that voters may switch their votes between the *ex-ante* marginal winner and loser. If a mobilization intervention only mobilized non-voters to vote (or demobilized voters from voting), a less demanding decision rule of  $MAEI_d < \underline{\psi}_d$  would be appropriate for mobilization interventions. Yet, this less stringent decision rule makes the (potentially strong) assumption that mobilization interventions do not affect the vote choice of subjects who would vote regardless. We have little evidence for or against this assumption. Due to the secret ballot, we cannot measure persuasive effects at the individual level using voter file data, the most common outcome data source in mobilization experiments (Green and Gerber, 2015). In contrast, voter registration experiments (e.g., Shineman, 2020) comprise an emerging intervention class that may productively invoke the less stringent  $MAEI_d < \underline{\psi}_d$  rule. Specifically, if these interventions can be directed exclusively to unregistered individuals, we mechanically eliminate the possibility of “persuasive effects” among subjects.

Second, can the content of interventions be changed to reduce the possibility of large aggregate effects, holding constant sampling and treatment assignment? Particularly in the context of information and persuasion experiments, it may be attractive to pursue “substantive” strategies to temper possible aggregate effects. For example, relative to providing information on an incumbent,

might showing voters debates with all candidates allow effects to “cancel out” in the aggregate, permitting narrower bounds on electoral impact? This line of argument requires an strong assumptions about how voters respond to treatments. Typically, empirical researchers aim to measure voters’ possible responses to an intervention as a finding, not an assumption. In the specific case of debate experiments, Platas and Raffler (2019) and Bidwell, Casey, and Glennerster (2020) both find (some) asymmetric treatment effects across different types of candidates, providing evidence against this assumption. This serves as just one example of the potential limitations of relying on assumptions about how voters respond to treatments to circumscribe the extent of electoral effects.

## 6.2 Relation to Existing Electoral Experiments

I examine the application of this framework to existing experiments in two ways. In Appendix 3, I use replication datasets, administrative, and archival data to apply the framework to four published experiments that comprise mobilization, information, and persuasion interventions. Below, I show that closely related interventions vary substantially in their ability to affect aggregate outcomes. I focus on back-of-the-envelope calculation of the  $MAEI_d$  on 14 studies of information and accountability that are classified by Enríquez et al. (2019). To compare these experiments, I use information reported in papers and appendices without consulting replication or ancillary data. These calculations provide a survey of whether the information necessary to consider aggregate impact is reported and what barriers to these calculations exist in the literature. I report the studies, their relationship to the proposed framework, and the calculations executed in Table A8. I lack any *ex-ante* information about how to predict these races, so I focus only on the calculation of  $MAEI_d$  under Assumptions 1 and 2.

Thirteen of the 14 studies intervene in multiple races (districts). I focus on calculating the *average*  $MAEI_d$  across districts. The *average*  $MAEI_d$  is an abstraction from the decision rule described in this paper. However, for the purposes of examining the literature, it does serve as a measure of the variation across studies on this metric. I am only able to estimate the  $MAEI_d$  in six of 14 studies, varying  $E[a_c(0)]$  from its minimum of 0.5 (for all  $c$ ) to its maximum of 1 (for all  $c$ ). I present these estimates in Figure 2. The graph suggests that the degree to which

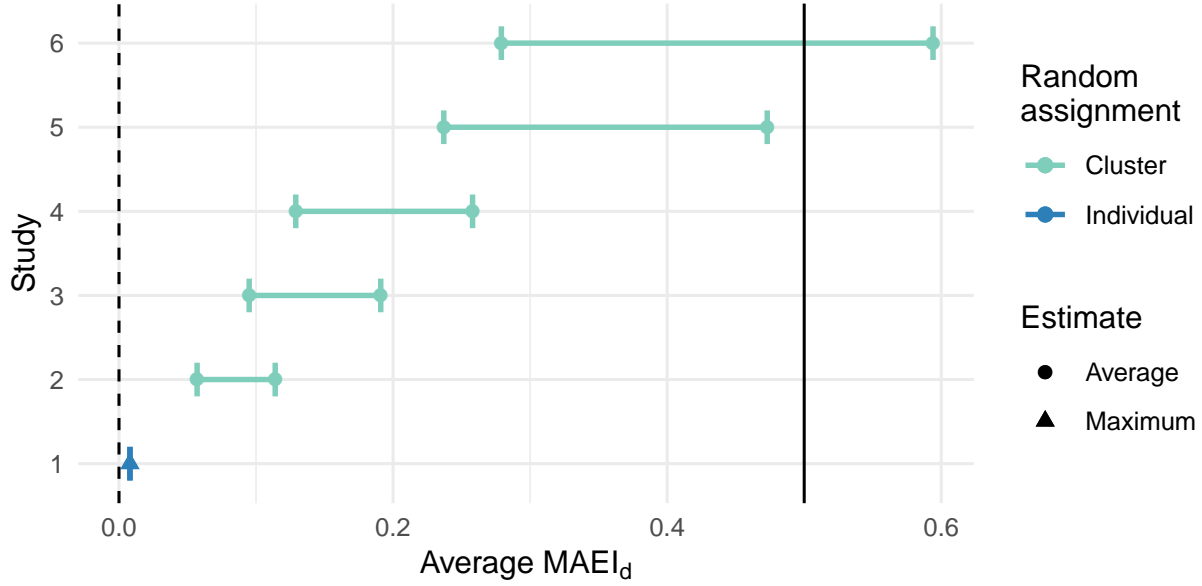


Figure 2: Estimated maximum or average  $MAEI_d$  for six electoral experiments on electoral accountability. The interval estimates in the cluster-randomized experiments indicate the range of  $MAEI_d$  estimates for any  $E[a_c(0)] \in [0.5, 1]$ .

existing information experiments could have moved electoral outcomes varies widely. Recall that these estimates in isolation cannot assess whether an intervention was consistent with the decision rule advocated here because I lack data on the predicted margin of victory. Nevertheless, any  $MAEI_d > 0.5$  will not pass the decision rule, regardless of the predicted *ex-ante* margin of victory. One immediate observation from Figure A8 is that cluster-assigned information treatments appear to be assigned at a very high density within districts.

The barriers to estimation of the  $MAEI_d$  in the remaining eight studies are informative for how we think of electoral impact. In general, these studies do not provide information on how the experimental units relate (quantitatively) to the electorate as a whole. This occurs either because: units (voters or clusters) were not randomly sampled from the district (4 studies) or because there is insufficient information about constituency size,  $n_d$  (4 studies). Note that the non-random sampling is generally well-justified from a design perspective and the constituency size is not necessary for the estimation of causal effects. However, to map these research designs into the present framework or onto electoral outcomes generally, we would need information relating the experimental sample

to the district(s) in which they are conducted. The takeaway from this survey of 14 studies is simply that considerations of aggregate electoral impact require analyses that are not (yet) standard practice. The variation in Figure 2 further suggests that research designs vary substantially on this dimension and justify the considerations I forward.

### **6.3 Simulations Using Electoral Data**

Simulating the design of experiments under the decision rule complements insights from existing studies. This simulation demonstrates how researchers can use sampling of districts and the allocation of treatments within and between districts to design electoral experiments. The simulation uses electoral data from the US state of Colorado. It relies upon real voter registration data, precinct-to-district mappings, and election predictions. Because US elections are administered at the state level, the simulations are greatly simplified by focusing on a single state in one election: the 2018 midterms. All races in 2018 were at the state level or below.

In the simulations, I assume that an experimental intervention would not occur absent the researcher (Case #1 above). In the case of Colorado, there are many forecasts available for the 2018 US House elections. I use the forecast by Morris (2018). I do not know of forecasts for State House seats. Therefore, I predict outcomes from (limited) available data, namely partisan voter registration data and lagged voting outcomes. I fit a basic predictive model on electoral data from 2012-2016 (three elections) and then predict outcomes for 2018 (see A5.3 for details).

Examining only the predictive intervals, Figure 3 depicts the 90% predictive intervals for Colorado's 65 State House and 7 US House seats in 2018. The 90% predictive intervals provide a useful visualization because when they bound 0 (gray intervals in the Figure), no experiment can pass the decision rule proposed in this paper. In sum, 33/65 State House races and 2/7 US House races bound 0. More precise prediction algorithms, particularly in the State House races, may alleviate concerns in some cases. On the other hand, these (effectively) two-candidate races are relatively predictable given the comparative salience of partisanship in the US and vast amount of effort devoted to predicting and understanding US elections. As such, this analysis represents roughly a "best case" scenario for experimental design.

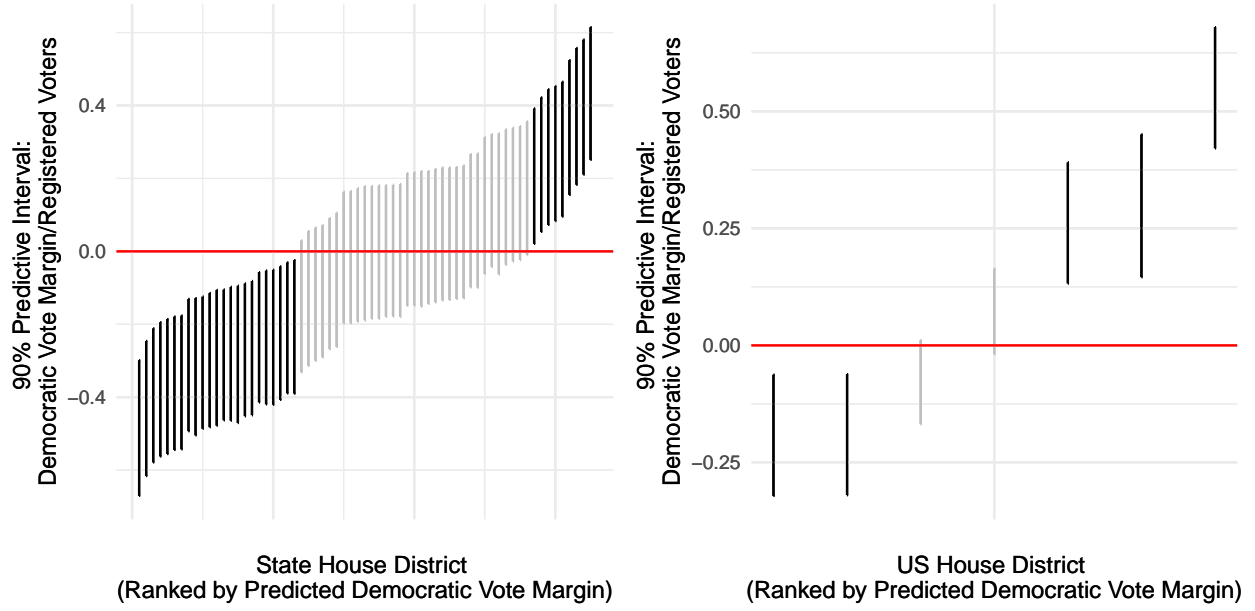


Figure 3: Predictive intervals for 65 State House seats and 7 US House seats. Grey lines represent grounds for declining to conduct an experiment in a district under the decision rule proposed here.

I consider two research designs, each invoking SUTVA and, by design, satisfying Assumption 2.<sup>9</sup> I first consider experiments that assign individual voters (not clusters) to treatment. I show calculations based on three types sampling of individuals into the experimental sample that vary the calculation of  $E[a_c(0)]$  and thus  $MAEI_d$ . A best case scenario sets  $E[a_c(0)] = \frac{1}{2}$  and represents the case in which participants were pre-screened to evenly fall on both sides of the ideological spectrum. A worst case scenario sets  $E[a_c(0)] = 0$  (resp. 1) and could represent the case in which all experimental subjects would vote in the same way absent treatment, as would be the case for an experiment on likely Republican (resp. Democratic) voters. The intermediate case represented by “random sampling” predicts  $E[a_c(0)]$  from 2016 district vote totals.

Figure 4 depicts the theoretical maximum number of individuals that could be assigned to *treatment* in State House and US House elections, by district and race. The shading represents the three sampling assumptions described above. Several features are worth note. First, the experimental allocation of treatment can only pass the decision rule in sufficiently extreme (thus predictable)

<sup>9</sup>I assume all voters in cluster-randomized precincts are assigned to treatment if they belong to a treated cluster.

electorates. Ranking districts from the most Republican to most Democratic (in terms of predicted vote margin) on the  $x$ -axis, the maximum number of individuals assigned to treatment is 0 in competitive races. The more lopsided the race, the more subjects can be assigned to treatment under the decision rule. Second, the type of experimental sample conditions the permissible treatment group size. However, going from worst to best case can double the number of subjects, as implied by Equation 2. Third, comparing the top and bottom plots in the left column, in larger districts, the maximum number of registered voters that could be assigned to treatment grows proportionately to district size (see Table A9 for summary statistics). Finally, when describing the maximum number of treated subjects as a proportion of the electorate, only sparse treatments are permissible under the decision rule. Nevertheless, it implies that one could allocate an individually-randomized treatment in a way such to power an experiment within the ethical constraints proposed by this article.

Moving to a cluster-randomized treatment at the *precinct* level, Figure 5 examines the expected number of precincts that could be assigned to treatment in each type of race. These graphs assume that all registered voters in a treated precinct are treated. The graph suggests that in State House races, the number of “treatable” precincts is quite small. Given that cluster-randomized electoral treatments are most popular in low-level races in developing settings, this suggests that researchers should be much more cautious about treating large segments of a district, even in “best case” scenarios when elections are relatively predictable. In general, if researchers must treat at the cluster level, they should minimize the number of clusters treated in any given district.

## **7 Implications for Research Design and Learning**

The parameters used to characterize the possibility for electoral interventions to change elections reflect features of both electoral systems, context, and data availability. I argue that best practices for electoral experiments are more likely to be tenable on the ethical grounds.

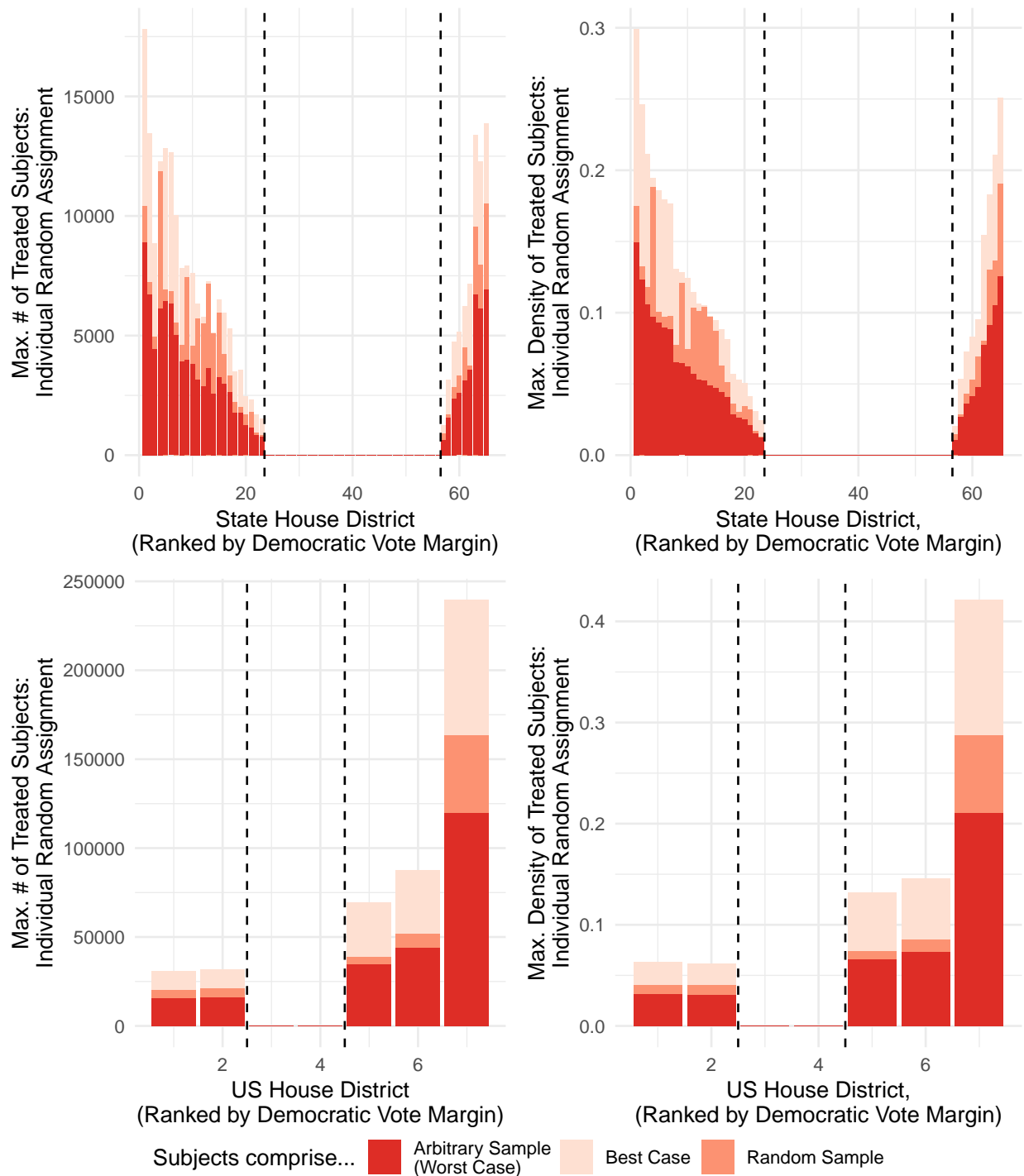


Figure 4: Maximum number of individuals (left) or and individuals as a proportion of registered voters (right) that can be assigned to treatment under decision rule.

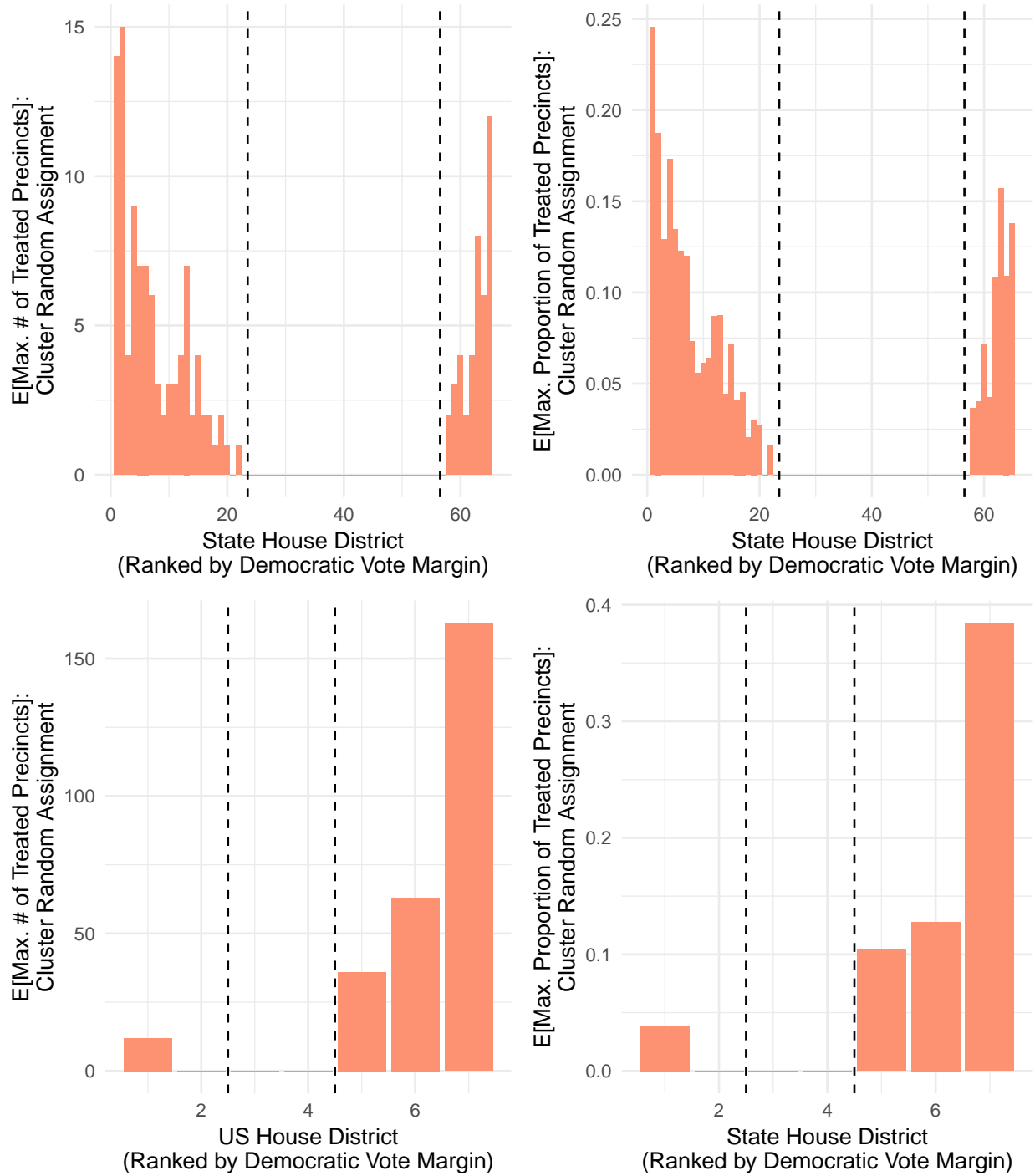


Figure 5: Maximum number of precincts (left) or and precincts as a proportion of all precincts (right) that can be assigned to treatment under decision rule. The expectation is evaluated over simulated assignments of treatment to precincts with heterogeneous numbers of registered voters.



## 7.1 Electoral Systems, Rules

Electoral systems specify the mapping between votes and seats, influencing the plausible range of  $\psi_d$ , the *ex-ante* margin of victory. Consider the distinction between first past the post (FPTP) majoritarian and closed list proportional representation (PR) elections. While either type of race can, in principle, be arbitrarily close, the maximum value of  $\psi_d$  is given by the reciprocal of district magnitude. In a FPTP election with one office at stake, the theoretical upper bound on  $\psi_d$  is 1. In a PR race with standard Hare or D'Hondt seat allocation formulas, the upper bound on  $\psi_d$  clearly falls quickly as district magnitude increases. Increases in proportionality under PR thus limit the possibility of “landslide” elections where high-density treatments would be unable to move outcomes.

More subtle variants of majoritarian and PR electoral systems also exaggerate or limit the degree of variation in  $\psi_d$ . For example, two-round systems or runoff elections create complications in prediction of  $\psi_d$ , at least in advance of a first round. Moreover, if changing electoral outcomes also includes changing which politicians win office (as opposed to which parties win seats), open list PR systems imply that  $\psi_d$  can be interpreted in terms of the last seat allocated *or* the allocation of seats within a list. This can reduce  $\psi_d$ , reducing the admissible  $MAEI_d$ .

Beyond considerations of the plausible range of  $\psi_d$ , there remain questions about our ability to predict this quantity. In FPTP elections, particularly in the context of two parties,  $\psi_d$  is easily interpretable as the margin of victory (as a ratio of registered voters). Yet, in other types of elections,  $\psi_d$  is a much more abstract and less studied quantity. The extent to which we can predict  $\hat{f}(\psi_d)$  across contexts remains an open empirical question that is quite important for the considerations described here.

Other features of the electoral system may also condition the suitability of a race for experimentation. Consider the role of concurrent elections. Concurrent elections often include races with different electorate sizes, captured by the  $n_d$  parameter in the model. (See A3.2 for an worked example.) An intervention on a set of voters may represent a much larger proportion of the electorate in one race than in a concurrent race. Note that concurrent elections do not represent any

form of experimental design violation in standard experimental analyses. In general, concerns of concurrent elections are reduced if the experimental manipulation happens in the “smallest” race: the race with the smallest  $n_d$ . However, if the number of voters in the smallest race is greater than  $\frac{1}{2}$  the number of voters for a larger race, then variation in competitiveness could compensate for variation in saturation. As this discussion outlines, concurrent elections can lead to profound differences in assessments of the risk of electoral experiments.

## 7.2 Contextual Features of Elections

We observe substantial variation in our ability to predict elections across contexts. Most predictive models, including those described here leverage some combination of past electoral returns and contemporaneous indicators (polls, incumbency, and macroeconomic indicators). It is possible that in other contexts, predictive models could exploit ethnicity or other identity-based characteristics that are highly prognostic of voter behavior. Yet, there exist three kinds of variation in our ability to precisely estimate the predictive distribution,  $\hat{f}(\psi_d)$ , across contexts. First, the levels of effort invested in prediction of elections vary by country and office. At the national level executive and congressional elections in the U.S. and other OECD democracies, there is substantially less effort devoted to develop prediction methods for lower level offices or elections in developing countries. To the extent that limited effort stymies the precision of prediction efforts, a researcher would be *less* likely to experiment in elections for which predictive models are underdeveloped.

Second, the degree to which voter characteristics or past voter behavior in election  $t - 1$  is predictive of their behavior in election  $t$  varies substantially. When electoral volatility is high or parties are weakly institutionalized, our ability to identify “safe” districts on the basis of observables is apt to be curtailed.

Finally, the amount of information collected to predict voter behavior varies across contexts. In particular, the availability of polls varies drastically. Absent contemporaneous information, identifying large aggregate shifts in the electorate becomes more difficult.

The availability of administrative election data (as opposed to surveys) often conditions considerations about the unit of clustering. Where we have accurate measures of relevant individual-level

outcomes, randomization at the individual level is often preferred to maximize power. However, in most elections, the ballot is secret. While turnout may be observed in administrative records in some countries, where data equivalent to voter files are unavailable or turnout is not the central outcome of interest, researchers are often forced to treat precincts (or the lowest level of administrative electoral return aggregation). Yet, in order to detect any treatment effect on aggregate vote shares, researchers tend to treat a larger share of the precinct, increasing  $S_d$ . Thus, by informing the level of clustering, the availability of administrative data also conditions the risk presented by electoral experiments.

### 7.3 Trade-offs and Implications for Knowledge Cumulation

This discussion suggests a number of contextual features that may circumscribe the use of electoral experiments. Indeed, the framework suggests a need for selection on intervention content and features of elections, yielding five recommendations for design of these experiments:

1. Select treatments to improve the plausibility of assumptions of restricted interference.
2. Experiment in FPTP races.
3. Implement interventions in larger electoral districts.
4. Avoid implementing experimental interventions in close or unpredictable races.
5. When intervening without a partner, reduce the number of subjects assigned to treatment in each district,  $d$ . When intervening with a partner, reduce the number of voters assigned to treatment or control because of the random assignment in each district,  $d$ .

Some of these recommendations are new and others echo existing sentiment. Most obviously, reducing sample size (related to #5) is often advocated as a strategy to reduce aggregate impacts (Desposato, 2016; Phillips, 2021). While these recommendations broadly concur, the framework developed here provides additional guidance. Desposato (2016) advocates a power calculation to aid in minimizing the sample size in service of ethical concerns. The decision rule in this article

complements this guidance by indicating whether the level of treatment saturation implied by a power analysis remains too high to implement. A subset of experiments at least since Wantchekon (2003) considers the predicted margin of victory when justifying intervention. The framework and decision rule provides allows researchers to understand how to vary multiple design parameters simultaneously. Most obviously when intervening in multiple districts, researchers can use Strategies #4 and #5 in consort. Reallocating the intervention across districts may improve statistical power while reducing the ethical concerns described in this paper.

These design strategies posit trade-offs in terms of learning from electoral experiments. I focus on implications for generalization (external validity) and statistical power. Strategy #1 circumscribes the set of treatments that researchers develop and administer experimentally. In particular, this paper suggests that treatments that vary saturation of treatment assignment to study social dynamics or network effects of voting behavior are unlikely to pass the decision rule. Certainly, some social dynamics like coordination might be examined experimentally within small subsets of the electorate (i.e., within the household), but the current emphasis on these dynamics within large subsets of the electorate yields designs that are less likely to pass the decision rule forwarded here. More attention to when these types of interference are most likely can inform the choice of interventions examined in electoral experiments.

Strategies #2 and #3 constrain the types of races in which intervention consistent with the ethical guidelines in this article is feasible. These strategies rule out electoral experimentation in some countries or offices as a function of the electoral system or institutions. If voters, candidates, and parties behave differently under different electoral systems, it may be hard to generate evidence relevant to different types of elections. To the extent that framework focuses on the ratio of treated voters to registered voters, the guidance to experiment in larger districts is similar to the guidance to reduce the number of voters assigned to treatment. However, it also speaks to the motivation of the interventions attempted. Many mobilization interventions focus on lower-turnout elections (i.e., primaries, state, or midterm elections) and many information interventions focus on low-information elections (often local races). These approaches contrast with the guidance of Strategy

#3. Note that both approaches – the current emphasis on low-turnout or low-information elections and the recommendations to focus on races in larger districts – speak to potential limitations of generalization from electoral experiments. While critiques of the lack of external validity of experiments are widespread, the idea that ethical considerations may lead to a less sample that is less “representative” is new to my knowledge.

Strategy #4 – avoiding close or unpredictable races – posits concerns for both generalization and statistical power. This strategy excludes some polities with high volatility or minimal investment in prediction. A discussion of limited external validity in the context of experiments implies a concern about treatment effect heterogeneity. Indeed, in electoral contexts, we may expect voters (or politicians) to act differently in places where a voter is more or less likely to be pivotal. If treatment effects vary in the characteristics used to target an experimental intervention, there exists a trade-off between these recommendations and the generalizability of insights about behavior. A focus on landslide races may also impact statistical power, though the direction is ambiguous. If there are fewer persuadable (“swing”) voters in landslide districts than in marginal districts, a lack of persuadable voters may imply a lower ceiling for treatment effects, limiting power. However, power also depends on the distribution of the outcome variable. For a binary (voter-level) outcome, power to detect a (fixed) effect size is higher in very imbalanced electorates. The net implications for power are therefore ambiguous.

Finally, Strategy #5 points to a familiar trade-off between statistical power and concerns about impacting aggregate electoral outcomes. I show that this trade-off is particularly salient in experiments seeking to analyze aggregate electoral outcomes at the cluster (i.e., polling station or precinct) level. At the same time, the framework provides novel guidance for the allocation of treatment across electoral districts. Specifically, it suggests that some power concerns may be reduced through higher levels of treatment saturation in districts where elections are highly predictable and not close and lower levels of treatment saturation in districts where elections are predicted to be somewhat more competitive. As such, the framework provides new ways to improve statistical power given this known trade-off.

Does the circumscription of electoral experiments to certain electoral contexts and treatments undermine the utility of electoral experiments as a tool? Here, an analogy to electoral regression discontinuity designs (RDDs) proves instructive (Lee, 2008). Electoral RDDs estimate some form of local average treatment effect (*LATE*) at the threshold where elections are decided. The method is disproportionately used in low-level (i.e. municipal) FPTP contests, in search of statistical power and questions about how to conceptualize the running variable in PR contests (but see, e.g., Folke, 2014). If limitations on the application of electoral experiments discussed here are to be seen as damning to electoral experiments but not electoral RDDs, there seemingly exists a question of whether the study of landslide races are less interesting – or of less political importance – than close contests. Theoretically, there are reasons why close contests may reveal distinct strategic dynamics that are not evident in predictable landslides, but this claim seems non-obvious. As such, this article simply advocates for a more careful application of electoral experiments with broader recognition of their limitations, not a wholesale abandonment of the tool.

## 8 Conclusion

This paper advances two broad insights for future research. First, I show that the formalization of an ethical objective can guide researchers to design research consistent with these standards (or avoid research inconsistent with these standards). I find that adherence to ethical goals may come with tradeoffs for learning. Specifically, I show that in electoral experiments, designing experiments with a minimal possibility of changing electoral outcomes can come at a cost to statistical power and external validity.

Similar formalizations of ethical objectives can guide the design of experiments beyond those in elections. Specifically, an application of the framework developed in this article to other experimental applications would consist of: (1) a clear mechanism linking the experimental intervention to relevant aggregate/societal outcomes; (2) a maximally agnostic model of how actors' responses to the intervention generate those outcomes; and (3) a set of assumptions restricting the set of actors that might respond to the treatment (the interference assumptions). Within this formulation,

elections serve as an “easy” case because they represent a fixed, known mechanism for generating aggregate outcomes. In particular, knowledge of this mechanism increases our understanding of what aggregate impacts are possible and how these impacts are generated. On the other hand, in elections, aggregate outcomes are particularly concerning because the set of impacted actors (residents of a district) is often very large relative to the set of experimental subjects. Whether aggregate effects of other interventions are likely to be as wide-reaching as those in elections depends on the potential mechanisms linking the intervention to aggregate outcomes. Development of similar frameworks for community-targeted development projects and audit/correspondence experiments seem like potentially important applications that are widespread in the literature (McDermott and Hatemi, 2020).

Second, I have focused on developing experiments in which the randomization of an intervention poses a sufficiently limited threat of changing aggregate electoral outcomes. I emphasize researchers’ uncertainty about the distribution of harms and benefits under different electoral outcomes as a justification for randomization. This uncertainty creates an imperative not to change outcomes through the random assignment of an intervention. If researchers, like partners, were certain about the aggregate benefits of a different electoral outcome, a different justification for randomization would likely be necessary. Asiedu et al. (2021) suggest a scarcity of such treatments may justify randomization.<sup>10</sup> Such arguments should be considered for candidate-level interventions like those that vary candidate selection processes (Casey, Kamara, and Meriggi, 2021; Gulzar and Khan, 2018). These interventions cannot pass the proposed decision rule because they impact the entire district, often by changing the identities of candidates on the ballot. Yet, these experiments may provide insights about representation and political selection. To this end, I echo guidance from American Political Science Association (2020) that exceptions to a principle of minimizing the risk of changing outcomes “should describe plausible impacts at the individual and/or societal level” when justifying intervention.

In sum, I show that careful research design can allow researchers to continue to draw some

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<sup>10</sup>However, random assignment may not be the optimal allocation of a scarce treatment.

insights from the experimental study of elections while providing more protections to the communities that we study. This paper advocates a widescale incorporation of ethical considerations as a more prominent guide to research design than is current practice.



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