The Ethics of Electoral Experimentation: Design-Based Recommendations

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Abstract

While experiments on elections represent an increasingly popular tool in the social sciences, the possibility that experimental interventions could affect who wins office remains a central ethical concern. I argue that researchers should design electoral experiments to minimize the likelihood of changing such outcomes. This paper develops a formal characterization of electoral experimental designs that generates an upper bound on aggregate electoral impact under different assumptions about interference. I present a decision rule for comparing this bound to predicted election outcomes by which researchers can decide whether an experimental design should be implemented. I show that 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 recommendations identify trade-offs between adhering to ethical commitments and the knowledge generated by some types of electoral experiments, which I demonstrate by simulation on real electoral data. In sum, this paper advances an argument that some ethical concerns with experiments should be addressed through careful research design.

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1 Introduction

Experiments on real elections represent an increasingly 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 elections experiments has increased precipitously since the late 1990s. A central ethical concern in the study of elections is that by manipulating characteristics of campaigns, candidates, or voter information, researchers may also be changing aggregate election outcomes.

Two notable changes since the pioneering experimental studies of elections by Gosnell (1926), Eldersveld (1956), Blydenburgh (1971), and Gerber and Green (1999, 2000) influence these ethical considerations. First, researchers now work in contexts with arguably wider variation in electoral institutions and voting behavior than early studies of local elections in US college towns.[1] To the extent that subjects and other residents of their district 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 since early experiments. 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). Indeed, this consideration appears to be invoked informally, if at all, in most *ex-post* accounts of electoral interventions. This article proposes a formal, design-based approach to the *ex-ante* consideration of how experimental interventions could affect aggregate election outcomes.

[1] Early (pre-2000) experiments occurred in local elections in jurisdictions where researchers worked, namely Chicago, Ann Arbor, and New Haven.

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, Desposato (2018) and Teele (2019) raise questions about standards for consent in field experiments including those on elections. This paper develops themes articulated in recent literature. Specifically, Beerbohm, Davis, and Kern (2017) argue that experimentation in elections may undermine political equality in general. Carlson (2019) notes that researchers must weigh the (possibly overestimated) epistemic benefits of experiments against the potential for harm to subjects. While these points merit a lengthier discussion, the focus of this paper is to consider how experimenters can minimize the risk of changing election outcomes.

I provide a new justification for efforts to avoid changing electoral outcomes by focusing on two features of the experimental context. First, contested elections imply an empty Pareto set: changing who wins office harms some individuals while benefiting others. Second, these harms and benefits are distributed across an electoral district, not simply experimental units, as a consequence of the aggregation of votes to the district level. As such, arguments claiming that interventions are welfare enhancing within the subset of district voters in experimental samples generally cannot be informative about the welfare consequences of interventions across the population of plausibly affected individuals. I thus argue that researchers planning experiments should aim to minimize the possibility of changing aggregate electoral outcomes or who wins office.

Minimizing the possibility of changing aggregate electoral outcomes requires a departure from standard practice in the design and analysis of experiments in two ways. 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. As I show in this paper, the frequent omission of information about the relationship between the electoral district and the experimental units (of assignment or outcome measurement) makes it difficult to estimate *ex-post* the saturation of an intervention in the relevant electorate.

Second, while experiments are powerful tools for estimating various forms of *average* causal effects, the ethical consideration is whether an electoral experiment changes *any* individual elec-

tion outcome, defined 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 to 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 least likely to change who wins office.

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 designing an experiment, 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.

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. Yet, these design principles identify novel trade-offs between ethical considerations and various forms of learning from electoral experiments. By treating fewer voters (all else equal), this ethical consideration admits a trade-off between aggregate electoral impact and statistical power. The concern is particularly acute in cluster-randomized experiments. Avoiding close races implies a trade-off between these ethical principles and external validity–a new source concern about a common experimental critique. Finally, these guidelines characterize electoral experiments as a tool that is

suitable in some contexts and for some interventions but not others.

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*, I suggest that more careful consideration of possible general equilibrium effects is critical for *ex-ante* consideration 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 research describing such interventions. I show how these considerations depart from current practices in the reporting of electoral experiments. Further, I illustrate the utility of these tools on electoral data from the US, simulating admissable experimental designs under the decision rule advocated. Second, I identify a set of trade-offs inherent to the design of electoral experiments that emerge in the consideration of whether experiments change electoral outcomes. Characterization of these trade-offs allows for a richer discussion about the merits and limitations of experiments on elections as 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. While some existing works adopt non-standard experimental designs as a function of ethical considerations (i.e., Slough and Fariss, 2019), to my knowledge, this is the first general framework to incorporate ethical concerns across a range of experimental designs in a common setting (elections). Such a framework may inform the development of other design-based strategies to reduce ethical concerns in social science experiments.

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.[2] In principle, such consequences constitute a basis for the set of possible harms and benefits to subjects. In considering these harms and benefits to subjects, the electoral setting is unique among other field experiments because elections generate winners and losers through a fixed (known) aggregation mechanism.

In contested elections, the set of possible Pareto-improving interventions is generally empty: an intervention will harm some subject such as a candidate made to lose support while accruing benefits to another subject such as a candidate with increased support. Even if we were to restrict the subject designation to voters, so long as preferences vary across the electorate, the possibility of shifting support from one candidate to another ostensibly generates harm and benefit to different groups of voters. In this sense, we know that electoral interventions can generally harm some actors.

The aggregation of votes to determine outcomes presents normative considerations unique to the electoral context. Importantly, it suggests that the set of individuals that realize the consequences of an intervention includes members of a district, which often far surpasses the subset of registered voters considered experimental "subjects." This consideration weakens the merits of some standard efforts to mitigate harm to subjects in experiments. In other (heavy touch) field experiments that could generate harm to subjects, researchers often purport to have randomized an intervention that would happen anyway (i.e., by a government or aid group), in so doing gleaning epistemic benefits without imparting additional "harm" (Teele, 2013). However, differences between non-experimental and experimental allocations of an electoral intervention can lead to very different distributional outcomes (of harm and benefit) as a result of aggregation of votes. Furthermore, claims about welfare among experimental subjects (however operationalized) are generally uninformative about the welfare of the full pool of individuals that realize electoral consequences.

[2]Indeed, downstream analyses of electoral experiments do suggest that who wins office (or how office is won) is consequential for later policymaking (Ofosu, 2019; Gulzar and Khan, 2018).

Some experimental interventions like anti-vote buying campaigns or revelation of corruption/malfeasance to voters serve as hypothesized antidotes to bad governance. In the case of a malfeasant incumbent, there may be a motivation to *change* aggregate electoral outcomes, even if it harms the malfeasant candidate, her cronies, and her clients. This motivation relies on an assumption that welfare would improve if a different candidate were elected as consequence of the intervention. In so doing, it imposes an unspecified welfare criterion in addition to strong assumptions about the features of the election, e.g., that the challenger pool includes non-malfeasant candidates. The strength of these assumptions presents a tension with the use of an electoral experiment. Specifically, if a researcher were well-positioned to make these assumptions, there is presumably less to be learned from an experiment, and possibly even an argument against withholding a presumed welfare-enhancing intervention from control-group voters. I contend that in these assumptions are too strong for most electoral settings. As such, researchers should design experiments to avoid changing aggregate electoral outcomes.

2.1 Experiments and their Counterfactuals

A focus on the effects of experimental interventions on aggregate election outcomes requires consideration of what would happen in the absence of the experiment. The interventions that are randomized as part of an experiment may or may not occur absent the experiment. The relevant consideration is thus: how could the experimental allocation of the intervention change electoral outcomes? Because consequences emerge from the aggregation of votes, the impact of changing (randomizing) the allocation of an intervention depends critically on the mapping between: (i) the unit and density of treatment allocation in the experiment; (ii) the unit and density of treatment allocation in the non-experimental implementation (if one exists); and (iii) the relationship between the treated units and the electoral district. Given these considerations, it is important to specify concretely the counterfactual in the absence of the experiment.

To this end, I consider two processes through which electoral experiments may be designed focusing on relevant actors in the research design process. I consider two types of actors: researchers and potential "partners." Researchers conduct an experiments primarily to learn something about

Cases	Actors		Experiment	Counterfactual (absent experiment)	Example
	Researcher	Partner			
1	✓		*Researcher designs, implements experimental intervention.* (Note: An partner may participate in or endorse the experiment, but experiment is initiated by researcher and intervention is funded through the researcher or externally.)	*No intervention occurs.*	Experiments conducted in Metaketa-I and cited in Dunning et al. (2019)
2	✓	✓	*Researcher randomizes a partner-funded and implemented intervention.*	*NGO or IG funds and implements intervention without randomizing allocation of treatment, possibly with less data collection.*	Pons (2018)

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

the world. A partner is a distinct actor that participates in elections in some capacity. Common partner organizations include NGOs, interest groups, or campaigns. NGOs and interest groups are entities that participate in elections in some capacity, albeit with different objectives. For the purposes of the analysis, the identity of the partner does not matter. I simply assume that partners that operate in an election do so in accordance with local election laws, such that the intervention in the absence of an experiment can be seen as consistent with regular aspects of a campaign/election.

Table 1 identifies two cases of electoral experiments, defined by the set of actors involved in their design and implementation. As is evident from comparison of the experiment and counterfactual columns, the experiment adds the researcher as an actor. Delineating what occurs in the absence of an intervention proves instructive for understanding how the imposition of an experimental (randomized) allocation affects the distribution of a treatment.

First, consider the modal case (Case #1) of electoral experiments in which a researcher designs and implements an intervention that would otherwise not have occurred. A researcher values learning; the most common quantitative operationalization of learning in experimental design is statistical power.[3] Given that a researcher cannot control how subjects will respond to a treatment

[3]If learning consists of Bayesian updating, a measure of difference between prior and posterior may be a better operationalization of learning. To my knowledge, there is not yet consensus on how to specify prior beliefs (or whose priors matter), complicating such analysis. Given that learning consists of both a possible change in the mean and dispersion of beliefs, a more powered designs roughly approximate learning in terms of a reduction in posterior variance.

or know *ex-ante* the precision gains from blocking or covariate adjustment, she typically maximizes power subject to a budget or implementation constraint by increasing the number of subjects in an experiment. Yet, increasing the number of subjects increases the range of possible electoral impacts either directly through treated voters (Section 4) or through general-equilibrium responses (Section 5).

Now consider the case in which some intervention by an NGO or IG is modified to include an experimental component (Case #2). The inclusion of an experiment generally changes allocation of the intervention. Since the relevant unit of aggregation is the electoral district, consider three possible changes in the allocation treatment from the non-experimental case. First and closest to the first case, the intervention may be implemented in districts that where it would otherwise not have been implemented in the non-experimental regime. For example, in a cluster-randomized experiment, in search of statistical power, researchers will generally look to expand the number of clusters (as opposed to individuals per cluster), which may expand the intervention into additional districts. Second and generalizing this point, there may be a change in the proportion of a district that is treated (differential saturation). This may stem from increased implementation costs for delivering a randomly-assigned treatment or simply the need for control units. Finally, it may be the case that different voters in a district are treated under an experimental allocation. For example, if a campaign targets its message all probable swing voters, under an experimental allocation, such voters must be assigned to control with some non-zero probability. Even holding constant the number of treated voters in a district, the types (and thus voting behavior) of treated voters may change when incorporating the experimental allocation of treatment.

Ultimately, I argue that ethical concerns about an experiment changing aggregate electoral outcomes must focus on the difference in treatment allocation between the experiment and its counterfactual. In Case #2, a researcher collaborating with a partner need not be penalized for campaign activities that would target the same voters regardless. At the same time, collaboration with a partner does not absolve a researcher from ethical considerations about how the experiment changes allocation of the treatment within a district. Importantly, analysis of the difference

between the experimental and non-experimental allocation of an electoral intervention cannot be completed without reference to the district or unit of aggregation.

2.2 The Ethical Objective

I assume that researchers' ethical objective is to avoid changing who ultimately wins office, relative to what would have happened absent the experimental allocation of an electoral intervention. In the aggregate, thus, 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).[4]

The approach advocated here considers two types of uncertainty that we have as researchers. First, as elaborated above, we lack the omniscience to determine whether electoral outcome is normatively better than another for constituents or a common welfare criterion upon which such a determination could be made. Indeed, any electoral outcome is apt to produce winners and losers. The approach here simply asserts that researchers should not be determining who wins and who loses in the service of research. Second, we do not know 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 seats or offices. As such, this paper advocates the estimation of conservative bounds on the *ex-ante* possible shift in vote share. These bounds can be calculated analytically and compared to predicted distributions characterizing relevant measures of closeness in elections.

When does an elections experiment become unacceptable on grounds that it is too likely to change election outcomes? In principle, we could eliminate the risk of influencing electoral outcomes entirely by not running these experiments. Yet, we also learn about political behavior,

[4]While the calculations of maximum aggregate electoral impact may also be helpful in races where mandate effects are important, more work is necessary to specify an appropriate objective function in these contexts. While electoral experiments in uncompetitive electoral autocracies are not particularly common, if supermajorities have signalling effects (e.g., Simpser, 2013) defining such an optimization problem may be useful in races where the design principles in this paper are relatively lax.

persuasion, and electoral accountability from these interventions. Some existing experimental interventions are small (or sparse) enough to have a near-negligible effect on electoral outcomes, even by the conservative standards specified in this article. This article provides a systematic way to bound possible effects *ex-ante*. It then suggests ways to compare these bounds to predicted outcomes in order to determine how 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 the 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' potential outcomes are affected 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 NGO or IG. 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 $j \in \{1, ..., n_d\}$ 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. 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 $n_c = 1 for all c$. Similarly, district-level clustering can be accommodated by assuming $n_c = n_d$. In practice, researchers generally

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 counterfactuals described in Table 1. Our main potential outcome of interest, $\pi(E)$ is whether an individual voter is assigned to receive a treatment. Note that $\pi(E)$ is not, in general, independent of E. In the experiment, allocation occurs via random assignment. Absent an experiment, I remain agnostic as to the (generally non-random) allocation mechanism. This notation allows for characterization of four principal strata, described in Table 2. I use the notation S_{11}^{cd}, S_{10}^{cd}, S_{01}^{cd}, and S_{00}^{cd} to denote the set of voters in 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	$\lvert S_{11}^{cd}\rvert = 0$	$\lvert S_{11}^{cd}\rvert \geq 0$
S_{10}^{cd}	If-experiment assigned	1	0	$\lvert S_{10}^{cd}\rvert > 0$	$\lvert S_{10}^{cd}\rvert \geq 0$
S_{01}^{cd}	If non-experiment assigned	0	1	$\lvert S_{01}^{cd}\rvert = 0$	$\lvert S_{01}^{cd}\rvert \geq 0$
S_{00}^{cd}	Never assigned	0	0	$\lvert S_{00}^{cd}\rvert > 0$	$\lvert S_{00}^{cd}\rvert \geq 0$

Table 2: 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 2, 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 exposed (resp. not exposed) to an intervention due to the experiment, heretofore the *experimental saturation*, $\mathcal{S}_d$ can thus be written:

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

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 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:* $Y_{jc}(z_{jc}) = Y_{jc}(z_{jc}, \boldsymbol{z}_{j,\neg c})$

I add a second *within-cluster* non-interference assumption to the baseline calculation. Note that, in contrast to SUTVA, this is not a standard assumption justifying identification 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.[5] 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.

[5]This assumption holds trivially in individually-randomized experiments when $|n_c| = 1$ or when all registered voters in a cluster are treated.

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

3.3 Voter 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 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.).

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: $ITE_{jc} \in \{-a_{jc}(0), 0, 1 - a_{jc}(0)\}$.[6] Specifically, treatment may induce a subject to vote for the winner who would not vote for that candidate absent treatment $(1 - a_{jc}(0))$; have no change on a voter's choice (0); or induce a subject who would have supported the winner to support a different candidate absent treatment $(-a_{jc}(0))$. Note that these bounds are effectively "extreme value" bounds (Manski, 2003). In invoking these bounds, I make no assumption about the plausible effects of an intervention (i.e., monotonicity). Indeed, Bayesian models of voter updating invoked in informational electoral experiments predict non-monotonicity in treatment effects as a function of the location of the signal relative to the prior (i.e. "good news" vs. "bad news").

Voting outcomes are observed at the level at which treatment is assigned, indicated by c. Recall that the cluster could represent an individual voter in this notation. 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

[6] Note that because a_{jc} is binary two elements in this set are equal to 0.

varying levels of experimental saturation in a treated cluster. This assumption can be relaxed when it is not suitable, but the bound on aggregate electoral impact will 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:*

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

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). This is important the intervention is assigned to a non-random sample of registered voters in a cluster. Second, holding constant the experimental design, the $MAEI_d$ is minimized where $E[a_c(0)] = \frac{1}{2}$ for all $c \in d$, 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 posits several immediate implications. 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 suggests that higher saturation of (the experimentally-manipulated) treatment implies greater potential effects on vote share, holding constant $E[a_c(0)]$. This introduces a trade-off between statistical power and the degree to which an experiment could alter aggregate electoral outcomes. Treating more individuals increases the saturation of treatment, possibly moving more votes. Moreover, a move from a individually-randomized to a cluster-randomized experiment requires many clusters for adequate power to detect effects. To the extent that researchers treat large proportions of voters in clusters, the saturation of treatment increases substantially. One implication of lack of individual-level outcome data is that the possibility of experimental interventions altering electoral outcomes increases substantially.

4.2 Assessing the Consequences of Electoral Interventions

The implications of $E[a_c(0)]$ on $MAEI_d$ demand 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 "margin to pivotality," ψ_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, this is the margin of victory. In a PR system, there are various interpretations of ψ_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 A 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 are 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 ψ_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 ψ_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 ψ_d, $\widehat{f}(\psi_d) \sim f(\psi_d|\widehat{\boldsymbol{\theta}})$, where $\widehat{\boldsymbol{\theta}}$ 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 ψ_d. In particular, I suggest that researchers calculate a threshold $\underline{\psi_d}$, that satisfies $\widehat{F}^{-1}(0.05) = \underline{\psi_d}$, where $\widehat{F}^{-1}(\cdot)$ indicates the quantile function of the predictive distribution of ψ_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 very 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 ψ_d as opposed to the point prediction, $\widehat{\psi_d}$ penalizes uncertainty over the posible 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.

5 When is this Analysis Non-Conservative?

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:

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

where $I[\cdot]$ represents an indicator function.

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 given random sampling of the cluster population. This condition 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.[7] In order to account for between-cluster interference, a 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.

[7]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 portion of a district.

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

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, $MAEI_d^{wb} \geq MAEI_d^{w}$.

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:

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

However, it must always be case that the margin to pivotality, $\psi_d \leq \frac{1}{n_d}\sum_{c\in d} E[a_c(0)]n_c$, 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 π_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 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 π_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 (calculation based on report of 100,000 flyers in Willis, 2014).

To the extent that scholars have 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 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 (whether in the same district or otherwise), the bound widens. Most challengingly, such a determination must be made before the intervention is fielded.

6 Illustration: Existing Experiments and Simulation

I now consider how the framework described here can be employed in the planning of an electoral experiment. I first provide an overview of how the framework can be applied to existing studies collected by Enríquez et al. (2019). This exercise reveals substantial variability in the estimated $MAEI_d$'s. It also suggests that the framework is most logically (and productively) applied *ex-ante* (before a study goes to the field) rather than *ex-post* (in the analysis of experimental data). To this end, I simulate a series of experimental research designs using real administrative data that speak to the *ex-ante* application of this framework.

6.1 Relation to Electoral Experiments on Information about Incumbent Performance

I focus on back-of-the-envelope calculation of the $MAEI_d$ given information reported in articles and appendices only. I use back-of-the-envelope calculation as opposed to consulting replication data for two reasons. First, these calculations survey whether information necessary to (begin to) aggregate votes is reported and what the barriers to these calculations exist. Second and more practically, many of these studies are still unpublished, rendering justifiably limited access to replication data. I report the studies, their relationship to the proposed framework, and the calculations executed in Appendix Table A1. 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 either an *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 variability 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 1. The graph suggests that the maximum degree to which existing experiments could have moved electoral outcomes varies widely. Note however, 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

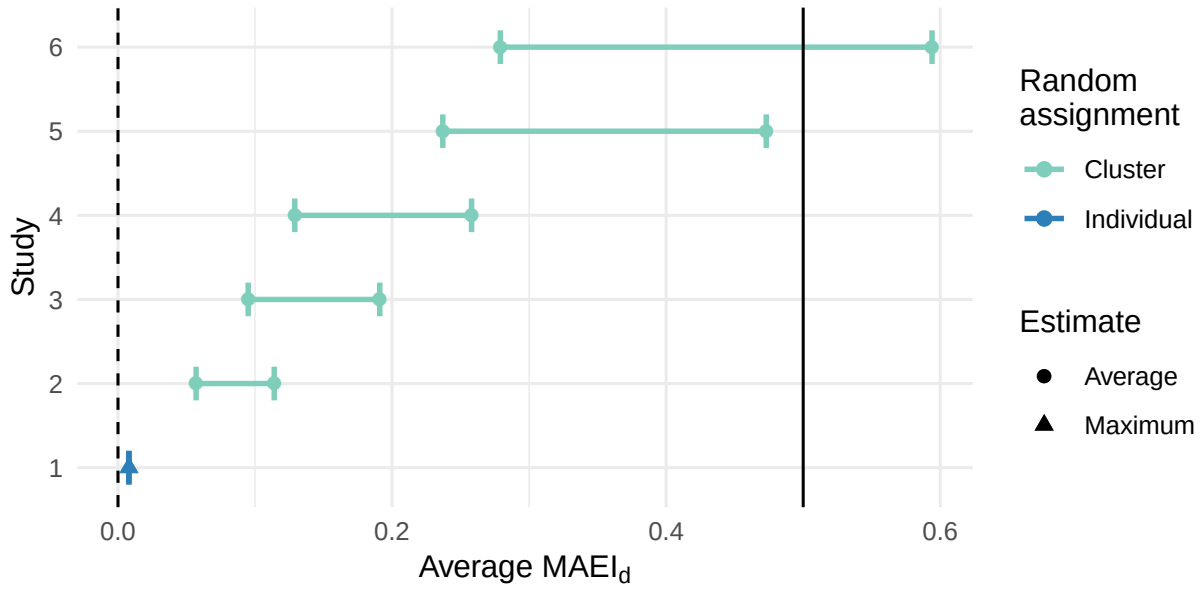

Figure 1: Estimated maximum or average $MAEI_d$ for six electoral experiments on electoral accountability. The interval estimates in the cluster-randomized experiments indicate the range of $MAEI_d$ estimates for any $E[a_c(0)] = [0.5, 1]$. For discussion of these calculations, see Table A1.

$MAEI_d > 0.5$ can never pass the decision rule, regardless of the predicted margin to pivotality. One immediate concern from Figure A1 is that cluster assigned 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. 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 1 suggests that research designs vary substantially on this dimension and justify these considerations.

6.2 Simulations Using Electoral Data

I conduct a simulation of the proposed guidelines for research design with electoral data from the US state of Colorado. The purpose of the simulation is to demonstrate how the framework proposed here can be used in practice and illustrate insights from the model. In the simulation, I rely on real voter registration data, precinct-to-district mappings, and election predictions. I manipulate aspects of the experimental design, particularly the method of assignment to treatment (clustered or individual), the number of units assigned to treatment, and the allocation of treatments across districts.

Specifically, I simulate a hypothetical experiments to be implemented in 2018 elections in Colorado. Because elections are administered at the state level in the United States, the simulations are greatly simplified by focusing on a single state. Moreover, all races in 2018 were at the state level or below. Colorado was randomly selected, though this draw was "lucky" for two reasons. First, Colorado is generally characterized as a swing state, and like much of the US exhibits substantial geographic variation in the concentration of political preferences, allowing examination of these design principles over a heterogenous set of districts. Moreover, Colorado provides partisan voter registration data disaggregated to the precinct level which assists in prediction.

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 do not know of forecasts for State House seats. Therefore, in the case of the State House races, I predict outcomes from (limited) available data, namely partisan voter registration data and lagged voting outcomes. I train a very basic predictive model on electoral data from 2012-2016 (three elections) and then predict outcomes for 2018.[8] I outline my prediction method and the construction of the predictive distribution ($f(\psi_d|\widehat{\boldsymbol{\theta}})$) in Appendix C.3.

Examining only the predictive intervals, Figure 2 depicts the 90% predictive intervals for Colorado's 65 State House and 7 US House seats in 2018. The 90% predictive intervals provide a

[8]One concern is that this model does not incorporate time shocks (in this case, the "blue wave") absent polling data. More sophisticated predictive algorithms can easily be incorporated.

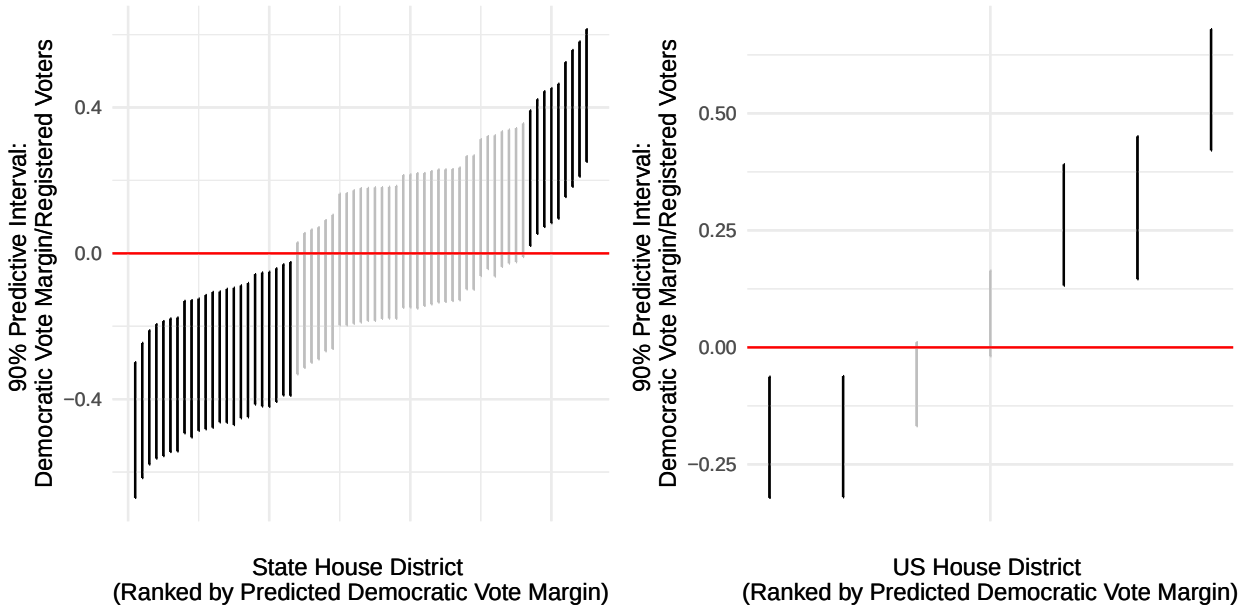

Figure 2: Predictive intervals for 65 State House seats and 7 US House seats in 2018. State House predictions are calculated following the method in Section C.3 while the US House predictions are "off the shelf" from Morris (2018). Grey lines represent grounds for declining to conduct an experiment in a district under the decision rule proposed here.

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-canddate races are relatively predictable given the comparative salience of partisanship in voting decisions the US and a vast amount of effort devoted to predicting and understanding US elections. As such, this analysis represents roughly a "best case" scenario for experimental design.

I consider several variants of research designs, each invoking SUTVA and, by design, satisfying Assumption 2.[9] 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

[9]I assume all voters in cluster-randomized designs are assigned to treatment if they belong to a treated cluster. For individually randomized experiments, Assumption 2 is implied by SUTVA.

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 3 depicts the 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) 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 (on the left and right of sides of the graphs), the more subjects can be assigned to treatment under the decision rule. Second, the type of experimental sample conditions the permissible sample size, though gowing from worst to best case can doubles the number of subjects, as implied by Equation 2. Third, comparing the top to 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 A2 for summary statistics). Finally, when describing the maximum number of treated subjects as a proportion of the electorate, in general, 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 4 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, or equivalently that Assumption 2 (no within-cluster interference) does not hold for some lower level of treatment saturation. 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

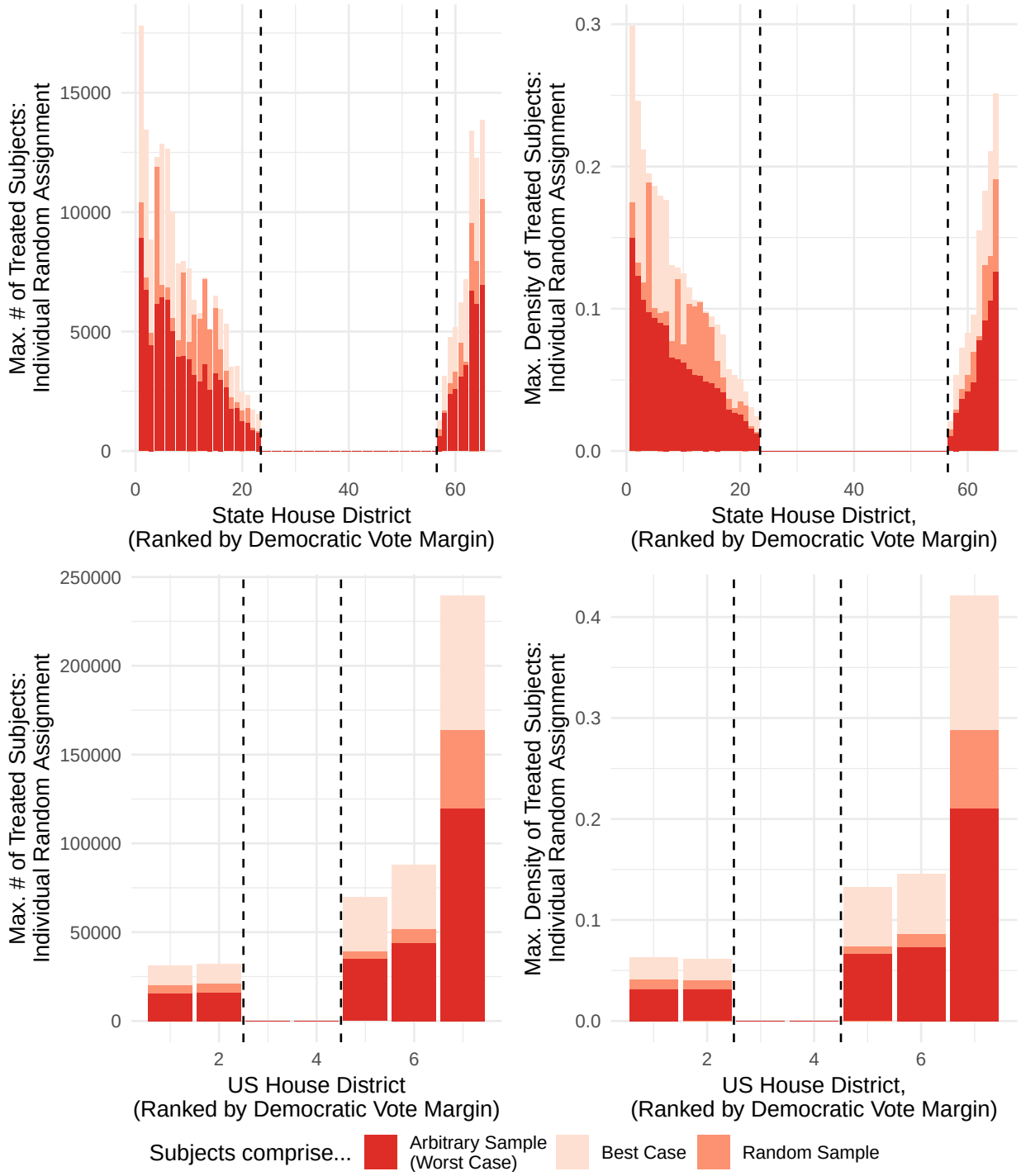

Figure 3: Maximum number of individuals (left) or and individuals as a proportion of registered voters (right) that can be assigned to treatment under decision rule. The black dotted lines denote the districts excluded on the basis of predictions bounding 0.

district, even in "best case" scenarios 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 spelled out in this paper in some contexts than others.

7.1 Electoral Systems, Rules

Electoral systems specify the mapping between votes and seats, influencing the plausible range of ψ_d, the margin to pivotality. Consider the distinction between elections using first past the post (FPTP) majoritarian and a closed list proportional representation (PR). While either type of race can, in principle, be arbitrarily close, the maximum value of ψ_d is given by the reciprocal of district magnitude. In a FPTP election with one office at stake, the upper bound on ψ_d is 1. In a PR race with standard Hare or D'Hondt seat allocation formulas, the upper bound on ψ_d 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 ψ_d. For example, two-round systems or runoff elections create complications in prediction of ψ_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 ψ_d can be interpreted in terms of the last seat allocated *or* the allocation of seats within a list. This can reduce ψ_d, reducing the admissible $MAEI_d$ under the decision rule and complicates prediction of $\widehat{\psi_d}$.

Beyond considerations of the plausible range of ψ_d, there remain questions about our ability to predict this quantity. In FPTP elections, particularly in the context of two parties, ψ_d is easily in-

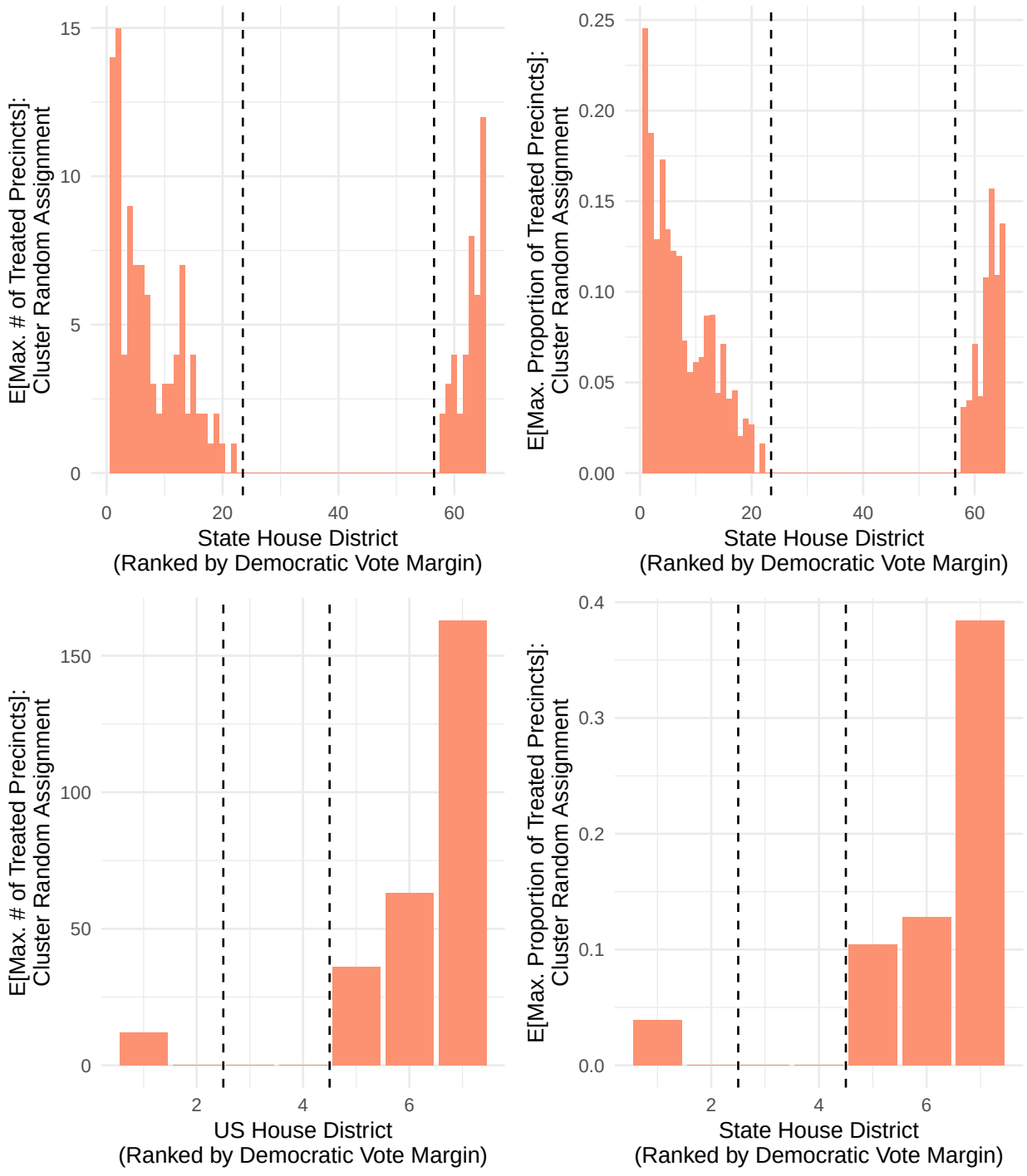

Figure 4: Maximum number of precincts (left) or and precincts as a proportion of all precincts (right) that can be assigned to treatment under decision rule. The expectation is evaluated over simulated assignments of treatment to precincts with heterogeneous numbers of registered voters. Expectations are rounded down to the largest number of full precincts that pass the decision rule. The black dotted lines denote the districts excluded on the basis of predictions bounding 0.

terpretable as the margin of victory (as a ratio of registered voters). Yet, in other types of elections, ψ_d is a much more abstract and less studied quantity. The extent to which we can predict $\widehat{f}(\psi_d)$ 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. 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. Yet, considerations of 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, $\widehat{f}(\psi_d)$, accross 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.

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. Further, the amount of information collected to predict of voter behavior varies

across contexts. In particular, the availability of polls varies drastically. Absent contemporaneous information, identifying large aggregate shifts (i.e, the "Blue wave" in the 2018 US Congressional elections) becomes more difficult.

7.3 Administrative Data Availability

The availability of administrative electoral returns (as opposed to surveys) often conditions considerations about the unit of clustering. Where we have accurate measures of individual-level outcomes, randomization at the individual level is often preferred to maximize power. However, in most elections, the ballot is secret. This implies that 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 $\mathcal{S}_d$.

7.4 Trade-offs and Implications for Knowledge Cumulation

The above discussion posits five main design choices by which researchers can limit the possibility that their experimental interventions change who wins elections:

1. Reduce the number of voters assigned to treatment.
2. Avoid implementing experimental interventions in close or unpredictable races.
3. Implement interventions in larger electoral districts.
4. Experiment in FPTP races.
5. Select treatments to improve the plausibility of assumptions of restricted interference.

Yet, these design strategies posit trade-offs in terms of learning from electoral experiments. First, consider the implications of #1 for statistical power. Constraints on power, at least in terms of the number of observations, N, typically emerge from inability to treat enough individuals or clusters due to budgetary constraints. This paper holds that power may be further constrained by

concerns about minimizing electoral impact when experimenters reduce the density of treatment. This trade-off is particularly salient in experiments seeking to analyze aggregate electoral outcomes at the cluster (i.e. polling station or precinct) level. Thus, I identify a likely tension between the ethical design of electoral experiments and their statistical power.

A further implication of this trade-off between statistical power and the number of voters experimentally assigned to treatment is that researchers should be careful not to "over-power" electoral experiments by including ever-increasing samples of voters. While power is increasing in the number of subjects or clusters (N), as N grows large, the marginal power gains from adding additional subjects is decreasing. Importantly, the possible electoral impact of an intervention increases linearly in the number of treated units,[10] suggesting that above some threshold, the increased risk of impacting elections outweighs precision gains from increasing the sample size. In a time when interventions are becoming cheaper to implement to large swaths of the electorate via SMS or social media, researchers should justify their sample selection carefully to avoid the possibility of changing electoral outcomes.

Strategy #2 – avoiding close or unpredictable races – raises a possible trade-off bentween ethical design of electoral experiments and external validity. In light of these ethical considerations, researchers would ideally maximize the "margin to pivotality," or ψ_d. In such races, the ability of an experiment to change who wins office is lower. 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 in terms of the possible risks to election outcomes and the generalizability of insights about behavior. 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.

Strategies #3 and #4 constrain the types of races in which intervention consistent with the ethi-

[10]This assumes that units are randomly sampled from a larger population, or that $E[a_c]$ remains constant as N increases.

cal guidelines in this article is feasible, with two implications. If we view an experimental estimate as a draw from a distribution of effects across contexts (as in random effects meta-analysis), the qualitative description of that population changes as a function of the places in which experiments are ethical to conduct. Whether a draw from the resultant distribution provides a meaningful answer to questions about voter behavior or elections remains an open question. Finally Strategy #5 circumscribes the set of treatments that we 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.

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 treament effect ($LATE$) at the threshold where elections are decided. The method is disproportionately used in low-level (i.e. municipal) FPTP contests, given a search for statistical power and questions about how to conceptualize the running variable in PR contests (but see, e.g., Folke, 2014; Fiva, Folke, and Sørensen, 2018). If the limitations on the appliation 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 be more interesting or 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 wholescale abandonment of the tool.

8 Conclusions

The ethical considerations and design recommendations in this paper rest on consequentialist considerations. Specifically, if experimental interventions change who wins office, they benefit some district residents and harm others. Moreover, due to the aggregation of votes in elections the set

of individuals benefited or harmed is uniformly larger than the set of experimental subjects. The empty Pareto set and aggregation of votes jointly distinguish electoral experiments from many other interventions.[11]

It is possible construct an ethical argument in favor of the design principles advanced here without reference to the effects of election outcomes. These principles rule out designs that could change any individual voter's ability to be pivotal. Variation in pivotality represents one source of political inequality, and an intervention that could change a voter's ability to be pivotal represents one way in which electoral experiments may change the underlying distributions of political influence across voters. Note that this argument provides a specific measure of how an experiment could undermine political equality. This specific definition of political equality provides one way to design experiments around the concerns enumerated by Beerbohm, Davis, and Kern (2017), if one intends to justify electoral intervention.

I show that careful research design can allow researchers to continue to draw some insights from the experimental study of elections while providing more protections to the communities that they study. While certain aspects of the present discussion are distinct to the electoral context, similar considerations can be undertaken in most field experiments. As such, I advocate the incorporation of ethical considerations as a much more prominent guide to research design than is presently described.

[11] One could attribute similar characteristics to experiments in legislatures, e.g., Zelizer (2018); Malesky et al. (2019).

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Appendices

A Vote Aggregation

Consider the case of an n-candidate (or n-choice) election in which n_d registered voters choose from candidates $i \in \{1, 2, ..., k+1\}$ where abstention is denoted by $k+1$. As above, vote totals absent the intervention are denoted v_d^i where $\sum v_d^i = n_d$. Without loss of generality, assume that:

1. Candidates 1 and 2 are the *ex-ante* marginal candidates.
2. $v_d^1 > v_d^2$ such that candidate 1 would win the last/only seat contested in the absence of intervention.[1]

From the definition of the margin to pivotality, ψ_d, it thus follows that $\psi_d n_d \equiv v_d^1 - v_d^2$.

In response to an intervention, denote the *net* change in votes from party r to party s as Δ_{rs}^d where $r < s$. If $\Delta_{rs} > 0 (< 0)$, candidate r received more (less) votes from candidate s voters than candidate s received from candidate r voters. The post-intervention vote total for party i, $\tilde{v}_d^i$, can thus be calculated:

$$\tilde{v}_d^i = v_d^i + \sum_{r=i} \Delta_d^{rs} - \sum_{s=i} \Delta_d^{rs} \tag{A1}$$

The difference in votes between candidate 1 and candidate i can thus be written:

$$\tilde{v}_d^1 - \tilde{v}_d^i = v_d^1 - v_d^i + \sum_{r=1} \Delta_d^{rs} - \left(\sum_{r=i} \Delta_d^{rs} - \sum_{s=i} \Delta_d^{rs} \right) \tag{A2}$$

If the intervention does not change the election result, candidate 1 must still win the last/only seat. This implies that $\tilde{v}_d^1 > \tilde{v}_d^i$ for all $i \in \{2, ..., n\}$.[2]

$$\tilde{v}_d^1 > \tilde{v}_d^i \Rightarrow v_d^1 - v_d^i > -2\Delta_d^{1i} - \sum_{r=1, s \neq i} \Delta_d^{rs} + \left(\sum_{r=i} \Delta_d^{rs} - \sum_{r \neq 1, s=i} \Delta_d^{rs} \right) \tag{A3}$$

Given an interference assumption, Definition 1 and the definition of Δ_d^{rs} imply that:

$$n_d MAEI_d \geq \sum |\Delta_d^{rs}| \tag{A4}$$

Equation A4 implies that $2n_d MAEI_d \geq 2 \sum |\Delta_d^{rs}|$. It therefore follows that:

$$2n_d MAEI_d \geq 2|\Delta_d^{i1}| + \sum_{r=1, s \neq i} |\Delta_d^{rs}| + \sum_{r=i} |\Delta_d^{rs}| + \sum_{r \neq 1, s=i} |\Delta_d^{rs}| \tag{A5}$$

[1] In a PR election, it may be useful to think of v_d^i as a quotient or remainder on the last seat allocated. The logic follows equivalently.

[2] I assume that there is no minimal participation rule. Thus abstention (option $n+1$) therefore cannot "win," though this does not change the result.

Equations A3 and A5 imply that if $v_d^1 - v_d^2 > 2n_d MAEI_d$, it must be the case that $\tilde{v}_d^1 - \tilde{v}_d^2 > 0$. Substituting $v_d^1 - v_d^2 = \psi_d n_d$, it follows that if $\psi_d > 2MAEI_d$, then the experimental intervention could not change who wins office.

B Existing Experiments

I focus on published experiments on the provision of incumbent performance information to voters before elections, adapting the list of studies from Enríquez et al. (2019). Note that all calculations are back-of-the-envelope. I cannot estimate τ_c in the case of cluster-randomized experiments. For this reason, I show the full range of $MAEI_d$ over the possible domain of $\tau_c \in [0.5, 1]$.

Table A1 describes studies in the framework described in this paper.

Article	Country	Mapping to Framework	Calculation Details	$MAEI_d$ Est.	$\lvert D \rvert$
Adida et al. (2017)	Benin	d : Commune* c: Village (or urban quarters) j: Individual	Treatment (five variants) assigned to 195 of 1498 villages. Density of treatment in a village varies by treatment arm (below 100% in all) and village population is unclear without data on cluster size. Because of distinction in the de-jure vs. de-facto characterization of parliamentary electoral districts, more information needed to clarify n_d.	–	30
Arias et al. (2019)	Mexico	d : Municipality c : Precinct j : Individual	Sampled at most $\frac{1}{3}$ precincts per municipality, albeit non-randomly. Treated 200 households in each of 400 precincts (T1-T4). Precincts had, at most, 1,750 random voters. Non-random sampling of precincts prevents calculation of $MAEI_d$.	–	26
Banerjee et al. (2011)	India	d : State leg. district c : Polling station j : Individual	20 treated polling stations and average of 57.5 control polling stations per district. All households were treated.	$[0.129, 0.258]$	10
Bhandari, Larreguy, and Marshall (2019)	Senegal	d: Department c: Village d: Individual	9 individuals sampled per village. 450 villages are (non-randomly) sampled from the 859 villages in the 5 experimental departments. 375 villages received some treatment (non pure-control). Without further information on the distribution of villages (experimental and non-experimental) and population by district, the $MAEI_d$ cannot be calculated.	–	5
Boas, Hidalgo, and Melo (2019)	Brazil	d : Muicipality c : Individual j Individual	I assume $\frac{2}{3}$ of experimental sample was assigned to treatment (T1 or T2). The most over-sampled municipality had 416 voters in experimental sample and a population (not registered voters) of 45,503. If 70% of population were registered (mandatory in Brazil), upper bound (for any district) is given by $\frac{2}{3}\frac{416}{.7\times 45{,}503}$.	0.008	47
Buntaine et al. (2018)	Uganda	d : District c : Individual j : Individual	Study includes 16,083 subjects (T or placebo) in 111 districts. The subjects per district and registered voters per district are not provided so $MAEI_d$ cannot be calculated.	–	111

Article	Country	Mapping to Framework	Calculation Details	$MAEI_d$ Est.	$\lvert D \rvert$
Chong et al. (2015)	Mexico	d: Municipality c : Precinct j : $Individual$	450 of 2360 precincts were treated (selected randomly). No information on saturation within precinct so I assume all voters were treated.	$[0.095, 0.191]$	12
Cruz et al. (2019)	Philippines	d : Municipality c : Village j: Individual	All households were visited in 104 treatment villages (T1 or T2) across 7 municipalities. Each municipality has "20-25 villages." I assume 25 villages/municipality and that the experimental villages were randomly sampled.	$[0.279, 0.594]$	7
Cruz, Keefer, and Labonne (2018)	Philippines	d : Municipality c : Village j: Individual	All households were visited in 142 treatment villages in 12 municipalities. The average number of villages/municipality not reported. I assume 25 villages/municipality per Cruz et al. (2019) (which is consistent with 284 villages in the experimental sample). Villages were randomly sampled from the municipality.	$[0.237, 0.473]$	12
de Figueiredo, Hidalgo, and Kasahara (2011)	Brazil	d : Municipality c : Precinct j : Individual	$\approx$ All households visited with flyers in 200 treatment (T1 or T2) precincts of 1,759 precincts municipality. The precincts were selected randomly subject to a set of constraints.	$[0.057, 0.114]$	1
George, Gupta, and Neggers (2018)	India	d : Assembly constituency c : Village j : Individual	Intervention treated 500,000 voters (T1-T4) in 1,591 villages. Villages have $\approx 1,200$ registered voters, so saturation rate in treatment villages was averaged 26%. Non-random sampling of villages within constituencies prevents estimation of $MAEI_d$.	–	38
Humphreys and Weinstein (2012)	Uganda	d : Parliamentary constituency c : Polling station j: Individual	2 polling stations in selected constituencies and all households visited with flyers. Number of polling stations/constituency not reported so $MAEI_d$ cannot be calculated. The total number of constituencies where experiment occurred (known to be <147) is not reported.	–	–

Article	Country	Mapping to Framework	Calculation Details	$MAEI_d$ Est.	$\lvert D \rvert$
Lierl and Holmlund (2019)	Burkina Faso	d: Village* c: Individual j: Individual	12 individuals were assigned to treatment (T or placebo) per village. Information about village population (n_d) is not reported.	$\frac{12}{n_d}$	146
Sircar and Chauchard (2019)	India	d : Assembly Constituency c: Polling booth area j : Individual	16 polling booth areas per precinct assigned to treatment (T1 or T2) with $\frac{2}{3}$ of households in each polling booth area assigned to receive flyer. While selection of experimental polling booths is random, the total number of polling booths per constituency is not reported so $MAEI_d$ cannot be calculated	–	25

Table A1: Survey of experiments on information disclosure about incumbent performance. * indicates that there may be distinctions between the *de-jure* electoral system and the *de-facto* vote aggregation rule, indicating some uncertainty about how to determine the electoral district.

		Registerd Voters		Precincts	
District type	Year	Mean	Std. Dev.	Mean	Std. Dev.
State House	2018	55,472	9,489	43.55	15.67
US House	2018	505,812	61,654	404.43	89.46

Table A2: Summary statistics on State and US House districts in terms of registered voters and precincts. Note that past electoral data from 2012, 2014, and 2016 is also collected for use in prediction.

C Supporting Information for Empirical Illustration

C.1 Data and Data Sources

I simulate different research designs on electoral data from the state of Colorado. Because statewide data it is sufficient to simulate all but presidential elections (and the Electoral College renders states the first unit of aggregation in presidential elections), I randomly selected the state of Colorado. As such data comes from:

- Colorado:
  - Precinct-level electoral returns voter registration from Colorado Secretary of State `https://www.sos.state.co.us/pubs/elections/VoterRegNumbers/VoterRegNumbers.html`
  - 2018 House of Representative seat predictions from The Crosstab `https://www.thecrosstab.com/project/2018-midterms-forecast/`

C.2 Mapping the Framework onto Data

To clarify how the data is used, I map the parameters expressed in the paper onto variables in the data/simulation in Table A3.

C.3 Prediction Method

While much has been invested in predicting the results of national elections (in some countries), much less effort has been invested in predicting lower-level (state- and local-level) elections and elections in developing countries. In particular, there is a general lack of public opinion polling in these races. I consider what is possible to ascertain through registration data and past electoral returns alone. I propose the following method for estimating the predictive distribution of each ψ_d:

1. Estimate a model of the form: $y_i = f(\boldsymbol{\beta}\mathbf{X}_i)$, where $\mathbf{X}_i$ is a matrix of predictors. Note that the unit of analysis is the district.
2. Generate many draws from the joint distribution of $\boldsymbol{\beta}$. For each draw:
   (a) Estimate $\widehat{\psi_d}$ from the model (possibly by aggregating over precincts). Then calculate $\widehat{\epsilon} = \psi_d - \widehat{\psi_d}$, the residuals, denote the pdf of residuals by $f_{\widehat{\epsilon}}$.
   (b) Randomly sample $x \sim f_{\widehat{\epsilon}}$ and calculate $\widehat{\psi_d} + x$.
3. These estimates form the empirical distribution $f(\psi_d|\widehat{\boldsymbol{\theta}})$.

Variable	Mapping	Notes
j	Individual voter	
c	Simulation varies for: {Individual, precinct}	Implies n_c
d	Given by the electoral district for a context	Implies n_d
S_{10}	Set of treated voters. Implied by specification of c and assignment of treatment.	
S_{00}	Set of untreated voters. Implied by specification of c and assignment of treatment.	
$E[a_c(0)]$	Bound on possible change in turnout.	Predicted from available data or set to maximum (1) or minimum ($\frac{1}{2}$) possible values for all precincts.
ψ_d	Predicted margin of victory in district d.	Predicted from available data or third-party prediction algorithm (in US Congressional elections only).

Table A3: Mapping of parameters of the model onto variables in the data and simulation. I assume that, as in Case #1, no intervention would happen in the absence of the experiment, i.e. $|S_{11}| = 0$ and $|S_{01}| = 0$.