Ballot initiatives and information processing, an experimental approach (Pre-Analysis Plan)[*]

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Abstract

Despite representing one of the most common forms of direct democracy around the world, studies of voter persuasion efforts in ballot initiatives remain scarce. These direct democratic efforts have been hypothesized to improve the quality and legitimacy of policy decisions by some, and to lead to poor policy outcomes due to special interest capture and lack of policy knowledge by others. In order to test these hypotheses empirically, we propose a survey experiment measuring voting outcomes on citizen initiated ballot propositions following randomized treatment of information provision and endorsement by special interest groups and policy experts. We seek to test whether citizens acting as policy makers give more credence to high quality policy research versus special interest information. We are hoping this survey experiment can serve as a pilot for a field experiment which we could conduct during the 2020 general election.

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1   Introduction

One of the most direct ways in which citizens can produce policy is through pure democratic channels such as citizens initiatives, popular consultation, and ballot initiatives. Ballot initiatives have been hailed as an efficient direct democracy mechanism, providing citizens with a much-needed voice in policy making and politics. Defenders of ballot initiatives claim that they provide a way for citizens to voice their opinions and produce policies with less interference from parties and special interests (Cronin 1999). Others caution against the excesses of direct democracy and claim that special interest capture also operates in this system (Rosenbluth & Shapiro 2018). Doubts about voters' competency are also central to whether ballot initiatives lead to responsible policy decisions (Achen & Bartels 2017). If voters' competency in general elections is the focus of a huge literature and still very much an open debate, one thing we feel confident arguing is that the bar is even higher in a ballot initiative context: the need for policy knowledge is larger, as fewer heuristic cues are available to voters.

Those doubts about voters' competency interact with interest group's efforts to sway a vote in their favor. Efforts by interest groups to sway votes in legislatures are well-studied. We know that interest groups spend enormous amounts of money on ballot initiatives, and yet we don't know enough about how and whether these efforts work. We seek to determine which interest groups' efforts to sway votes in ballot initiative settings work, and what if any counter-measures such as providing information to voters can be efficient.

We lay out the three main channels through which special interests efforts could be at play: (i) influencing the wording of the initiative on the ballot, (ii) providing information to voters, (iii) spending money to put an initiative on the ballot and then campaign for or against it. We propose a survey experiment to test (i) and (ii), as well as observational data for (iii). Since a large part of the spending goes into influencing what's on the ballot and sending information to voters, we also partially address (iii) in our experiment. We

first conduct a survey experiment as a pilot study. We measure voting outcomes on citizen initiated ballot propositions following randomized treatment of information provision and endorsement by special interest groups and policy experts. We also randomize the wording of the proposal on the ballot. We are hoping to replicate this survey experiment in the field during the 2020 elections.

The paper will also contribute to the growing literature on the effect of campaign outreach on vote choice. Meta-analysis by Kalla and Broockman (2018) suggests that the effect of campaign contact and advertising on voting outcomes is close to zero in general elections. However, effects do seem to appear in non-partisan ballot measure campaigns, but few experiments exist on this topic. Moreover, all existing ballot initiative experiments contacted voters through a single advocacy group with a clear policy position. We know little about whether the quality and/or source of the information provided changes the efficiency of these messages.

2 Theory

2.1 Science, research and democracy

This project speaks to broader theoretical questions on the functioning of our current democracies, and in particular the potential gaps between reality and the ideals of deliberative democracies. We hope to contribute to ongoing discussions within philosophy of science, such as perceptions of research methods and consumption of scientific evidence outside of the academy.

2.2 Ballot initiatives and direct democracy

Direct democracy at the US state-level is extremely prevalent yet understudied. 31 of 50 states permit referendums of some kind, and 24 permit citizen initiatives (LeDuc 2003). Of these 24 states, California engages in more individual exercises of direct democracy than any other. Since 1912 California has voted on 376 ballot initiatives at the statewide level,

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with a sharp rise over the past 20 years, with 139 propositions on the ballot since 1995 (only Oregon comes close to matching these numbers).

Some scholars view this growth of direct democracy as a positive extension of democratic values, providing citizens with a much-needed voice in policy making and politics. Defenders of ballot initiatives claim that they provide a way for citizens to voice their opinions and produce policies with less interference from parties and special interests (Cronin 1999). They allow the collective intelligence of the people in politics (Landemore 2012) to lead to better decisions. Empirically, participation in ballot initiatives is associated with an increased capacity to correctly answer factual questions about politics (Smith 2002). There is also some evidence that voters are responsive to some types of information when casting a vote: a synthesis of a Citizen Assembly held on the topic received in voters' pamphlets can impact people's votes (Carman et al. 2015).

Others contend that the initiative process in the United States has been captured by special interest groups, directly undermining the increased power ballot initiatives are designed to deliver to citizens (Rosenbluth & Shapiro 2018; Stratmann 2004). Doubts about voters' competency are also central to whether ballot initiatives lead to adoption of good policies (Achen & Bartels 2017). If voters' competency in general elections is the focus of a huge literature and still very much an open debate, one thing we feel confident arguing is that the bar is even higher in a ballot initiative context: the need for policy knowledge is larger, as fewer heuristic cues are available to voters.

This implies that the outcomes of direct democracy may be no more democratic than representative democracy, as the same elite manipulation can operate in both domains. This hypothesis is contested, however, with some observational empirical work showing a null or even negative effect of special interest spending on initiative outcomes (Gerber 1999). Our research design allows us to evaluate the special interest capture theory by testing potential channels through which special interests are most likely to impact ballot initiatives outcomes. First, special interest are sometimes accused of having influenced the wording of

the proposal on the ballot, making it confusing to voters (confusion has been shown to make voters tend toward a 'no' vote). Second, groups can provide information to voters before the vote. Third, they spend money to put an initiative on the ballot and to campaign for or against it. Some parts of these spending efforts go into our first two channels, but this is a broader category. We also test what is presented as the most efficient way to counteract those efforts by interest groups: voters are not tied by parties or money, and are therefore more open to new, unbiased information on those questions.

Finally, ballot initiatives have also been identified as a source of policy blunders. One of the most notorious is California's Proposition 13 of 1978, which capped annual increases of real property taxes to 2 percent per year and prohibited governments from increasing property taxes without a further referendum. Proposition 13 has been blamed for underfunding of California public schools, contributing to California's housing crisis, and perpetuating income inequality in the state (Catterall & Brizendine 1985; Wasi & White 2005). Proposition 13 was not without its critics at the time of passage, with critics contending that "Proposition 13 raises serious questions about the feasibility of participatory democracy in a policy area which commonly has been dominated by experts" (McCaffery & Bowman 1978). In this context, our experimental design allows us to test whether voters are receptive to expert policy analysis of ballot referendum policies.

In line with the special interest capture theories, as well as evidence that ballot initiatives often lead to policy blunders, our primary hypotheses are that (i) *voters will be equally swayed by information from special interest groups and policy researchers.*, (ii) that *they will be swayed by wording changes* and that (iii) *information provision will not mitigate the effect of the wording change.*

2.3   Current experimental evidence

A growing body of literature contends that information provision is relatively unsuccessful at changing voting outcomes. The primary conclusion of the Metaketa 1 project—which sought

to determine if politicians were rewarded for positive information and punished for negative information—was that "the overall effect of information [provision] across all studies is quite precisely estimated—and not statistically distinguishable from zero" (Dunning et al. 2018). In the US context, a meta-analysis by Kalla and Broockman (2018) suggests that the effect of campaign contact and advertising on voting outcomes is close to zero in general elections.

However, Kalla and Broockman (2018) find suggestive evidence that campaign contact produces positive effects on vote choice in ballot measure campaigns (see Figure 1). While their meta-analysis includes 23 ballot initiative experiments, these experiments stem from 5 total projects (Arceneaux 2005; Arceneaux & Nickerson 2010; Rogers & Middleton 2015). All of these projects use advocacy groups to contact voters with a pro-initiative message via canvassing, phone, or mail. The existence of positive treatment effects in the ballot initiative environment begs the question of what kinds of informational campaigns are successful. Does high quality policy research persuade voters, or is special interest contact equally effective?
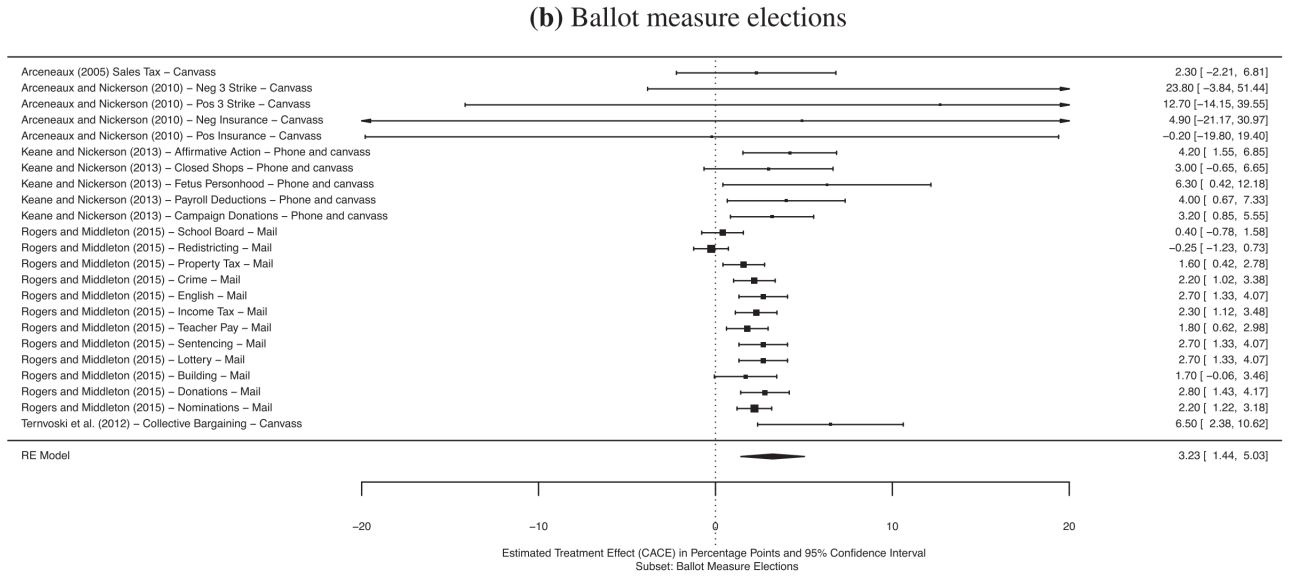
(b) Ballot measure elections

Arceneaux (2005) Sales Tax – Canvass	2.30 [−2.21, 6.81]
Arceneaux and Nickerson (2010) – Neg 3 Strike – Canvass	23.80 [−3.84, 51.44]
Arceneaux and Nickerson (2010) – Pos 3 Strike – Canvass	12.70 [−14.15, 39.55]
Arceneaux and Nickerson (2010) – Neg Insurance – Canvass	4.90 [−21.17, 30.97]
Arceneaux and Nickerson (2010) – Pos Insurance – Canvass	−0.20 [−19.80, 19.40]
Keane and Nickerson (2013) – Affirmative Action – Phone and canvass	4.20 [1.55, 6.85]
Keane and Nickerson (2013) – Closed Shops – Phone and canvass	3.00 [−0.65, 6.65]
Keane and Nickerson (2013) – Fetus Personhood – Phone and canvass	6.30 [0.42, 12.18]
Keane and Nickerson (2013) – Payroll Deductions – Phone and canvass	4.00 [0.67, 7.33]
Keane and Nickerson (2013) – Campaign Donations – Phone and canvass	3.20 [0.85, 5.55]
Rogers and Middleton (2015) – School Board – Mail	0.40 [−0.78, 1.58]
Rogers and Middleton (2015) – Redistricting – Mail	−0.25 [−1.23, 0.73]
Rogers and Middleton (2015) – Property Tax – Mail	1.60 [0.42, 2.78]
Rogers and Middleton (2015) – Crime – Mail	2.20 [1.02, 3.38]
Rogers and Middleton (2015) – English – Mail	2.70 [1.33, 4.07]
Rogers and Middleton (2015) – Income Tax – Mail	2.30 [1.12, 3.48]
Rogers and Middleton (2015) – Teacher Pay – Mail	1.80 [0.62, 2.98]
Rogers and Middleton (2015) – Sentencing – Mail	2.70 [1.33, 4.07]
Rogers and Middleton (2015) – Lottery – Mail	2.70 [1.33, 4.07]
Rogers and Middleton (2015) – Building – Mail	1.70 [−0.06, 3.46]
Rogers and Middleton (2015) – Donations – Mail	2.80 [1.43, 4.17]
Rogers and Middleton (2015) – Nominations – Mail	2.20 [1.22, 3.18]
Ternvoski et al. (2012) – Collective Bargaining – Canvass	6.50 [2.38, 10.62]
RE Model	3.23 [1.44, 5.03]

Estimated Treatment Effect (CACE) in Percentage Points and 95% Confidence Interval
Subset: Ballot Measure Elections

Figure 1: **Meta Analysis of ballot initiative experiments from Kalla and Broock-man (2018)**

3 Survey experiment

3.1 Why conduct a survey experiment?

Conducting a survey experiment in complement of the field experiment will provide at least three benefits: (i) to provide a pilot for the field experiment, (ii) to explore potential mechanisms and different treatments and (ii) to contribute to the growing literature on the replicability of survey experiments results in the field.

Substantively, a survey experiment allows us to add additional treatments and outcome variables. In addition to varying the information we provide to voters, we can add the wording of the proposal as a treatment arm. In addition to asking participants whether they would vote yes or no on the issue, we could ask them basic factual questions on the policy topic. Knobloch, Gastil, Feller, Richards, et al. (2014) study the impact of a Citizen Assembly in Oregon and the presence of a synthesis of the Assembly in the voters pamphlets. A state-wide survey shows that voters seemed to read and appreciate the synthesis, and they conduct a survey experiment to show that the synthesis lead to higher knowledge level on the issue than interest group or government-provided information). However, they do not study vote as the outcome variable of interest, and do not have a field experiment.

Methodologically, a growing literature suggests that survey experimental results often do not replicate in the field. By contrast, we hypothesize that survey experiments should better approximate the context of ballot initiatives as: (1) the vote choices are real ballot propositions rather than hypothetical candidates, and (2) partisan cues are less prevalent in (technical) ballot proposals. The ballot initiative setting would allow us to design the survey so that it exactly replicates the field setting, with the only differences being hypothetical vote choice versus actual.

Whether our hypothesis is correct or incorrect, we will contribute to the growing understanding of the contexts and questions in which survey experiments replicate in the real world. If our hypothesis is correct, we can open the door for more survey experiments with

more varied and ambitious treatments. If our hypothesis is incorrect, we show that survey experiments do not replicate even in a context where they closely approximate the field environment.

3.2 Treatment groups

We will have two treatments. The first one will have two arms: in control, subjects would see a proposal that was accused of being worded in a confusing way as to influence voters. The second treatment will have more treatment arms and will consist in providing subjects with information. Subjects in control would be provided no information. Subjects in different treatment groups would be provided with (i) partisan information, (ii) interest group information, (iii) one-sided scientific information, (iv) balanced scientific information with trade-offs.

3.3 Choice of ballot initiatives

Each subjects will see a series of 5 to 7 ballot proposals, with or without seeing information first. A subject from a given State will never see a proposal that was or will be on the ballot in his or her state.

3.3.1 Complexity treatment

We will exclusively use ballot proposals which were in the ballot on recent elections. To assess the complexity and confusion level of a proposal, we have several options. The first option would be to use existing readability tests to assess how complicated the language on a ballot is. Readability tests offer a systematic way to assess the complexity of a proposal based on the number and length of words and sentences. Such assessments have been conducted on ballot initiatives proposals by Ballotpedia in 2017 and **?** in 2011. Both studies found that the average ballot question requires at least some college-level education - in some states like West Virginia or Texas, a graduate level education is needed to read and comprehend the average ballot measure. **?** also finds that the more complex the proposal, the more likely

voters are to skip the question and not cast a vote on this specific proposal. We would need to find similar proposals on a policy standpoint, one with a low readability score and one with a high score.

A second option would be to use languages that were rejected by the States' supreme courts for different reasons. For the 2018 elections, here is a partial list of ballot initiatives that were rejected by judges: the implications of voting yes or no where not clear enough (Oregon proposition 101), the ballot language would not inform voters as to the amendments' effects and allow voters to form an intelligent opinion (North Carolina's two constitutional amendments), the language was misleading for several reasons, including the fact that the title did not include everything contained within the bill (Florida's amendments 6 and 8). We could use these exemples (and similar exemples from years prior to 2018) and use language after and before the judge's decision.

3.3.2 Information treatment

A solution to the complexity of ballot initiative that has been adopted by several states is to provide voters with explanations about the proposal in plain, simple English. Rhode Island revised its voter information handbook in 2016, adding illustrations and summaries in plain English. Alabama created a Fair Ballot Commission in 2014, staffed with state government officials, private citizens and lawyers and that prints ballot statements online and in printed 2 months before the election. The descriptions must be in "plain, nontechnical language and in a clear and coherent manner using word with common and everyday meaning that are understandable to the average reader'. We are not testing these efforts, as there is little doubt as to their benefits.

We are testing information provision in the form of endorsements. A party or interest group will support or oppose the proposal, providing a couple of arguments. We will find these arguments on campaign materials. Then we would sum up the scientific findings about the topic, either as a one-sided recommendation or as a trade-off.

Because we are hoping to use this survey experiment as a pilot for a field experiment

in 2020, we will add a couple of proposals for 2020 to this survey experiment. For those proposals, we will only manipulate the information provision, not the wording. We will try to find and use proposals on which a clear scientific consensus exists. Early candidates among the approved 2020 state ballot initiatives (the list of which can be found in Table A.1) include a proposition in Oregon to increase taxed on tobacco and e-cigarettes products, or a proposition in California to amend the much-maligned Proposition 13 of 1978 in order to allow commercial and industrial properties to be taxed at market value.

3.4 Outcomes

We would first ask our subjects to vote on a series of ballot proposals. Our outcome measurement will be percentage of "Yes" votes on the ballot initiative. In addition, we will look for the existence of two potential heterogeneous treatment effects: (i) partisanship and (ii) education level. Finally, we will ask a set of basic factual questions on the policy areas people were asked to vote on to assess whether information provision changed the level of understanding.

3.5 Power analysis

4 Field experiment

4.1 Treatment groups

We will provide voters with different randomized pieces of information encouraging either a "Yes" or "No" vote on a 2020 ballot initiative, probably in California or Oregon. We will have three treatment groups in total: (1) placebo, (2) business/special interest group information, and (3) policy research information. Three treatment groups allows us to have one null/zero treatment group (placebo), one positive treatment group, and one negative treatment, facilitating detection of differences between groups.

4.2   Randomization

We intend to use clustered random assignment rather than individual-level randomization (i.e assigning voting precincts to treatment and control rather than individuals nested within them). Arceneaux (2005) argues that clustered random assignment is simpler to implement, less expensive, and allows for direct measurement of voting outcomes without reliance on survey data.

While implementation of clustered random assignment may be simpler and allows for direct measurement of voting outcomes, analysis is more complex. If cluster size covaries with potential outcomes, the classic difference-in-means estimator will be biased. We will therefore calculate the ATE using both: (1) the classic difference-in-means estimator (while examining whether the estimated ATE changes when restricting analysis to large and small clusters), and (2) by measuring the difference in total outcomes. The latter is defined as:

$$\hat{ATE} = \frac{K_C + K_T}{N} \left(\frac{\sum Y_i(1)|d_i = 1}{K_T} - \frac{\sum Y_i(0)|d_i = 0}{K_C} \right)$$

where K_C is the number of clusters assigned to control, K_T is the number of clusters assigned to treatment, N is the number of subjects in the analysis, and $Y_i(1)|d_i = 1$ and $Y_i(0)|d_i = 0$ refer to potential outcomes in treatment and control clusters, respectively. Standard errors will be calculated as:

$$\hat{SE}(\hat{ATE}) = \sqrt{\frac{N\hat{Var}(\bar{Y}_j(0))}{K(N-m)} + \frac{N\hat{Var}(\bar{Y}_j(1))}{Km}}$$

where K refers to the number of clusters, N refers to the total number of subjects, and m refers to the total number of subjects assigned to treatment.

Cluster-random assignment often results in a loss of statistical power. This decrease in precision increases as the difference in potential outcomes between clusters increases. To compensate, we will attempt to increase the number of clusters in our sample to the maximum extent possible while using clusters of similar size. If it becomes impossible to

use clusters of similar size, we will conduct block random assignment by cluster size. We will also adjust for predictive covariates such as past voting behavior, age, gender, party registration, etc. In practice, we will therefore estimate treatment effects and standard errors using OLS regression with cluster robust standard errors and inclusive of predictive covariates, in addition to the baseline measurements described above. [Would there be anything wrong with using the most predictive covariates from the survey experiment for covariate adjustment in the field experiment?]

A potential issue with cluster random assignment at the precinct level in California is the adoption of the Voter's Choice Act in 2016. Under the Voter's Choice Act, voters can go to any polling place in their county and can vote through mail at the county level. 5 counties opted in the system for the 2018 elections and 5 more counties are expected to do so for the 2020 elections (out of a total of 58 counties). For these counties, it means that we will not be able to get precinct-level data. We will therefore need to exclude those 10 counties (which account for 48.8% of the California population) from our analysis. From our initial, quick look, these counties seem to be evenly distributed across partisan lines. If this methodological choice turns out to be hard to defend, we can also give up on California and conduct the experiment in another state with a significant tradition of ballot initiatives (Oregon for example).

4.3   Treatment details

This information provision could be done through mail (a postcard), canvassing, phone calls, text messaging, or emails. Postcards, text messaging, or email would allow for larger sample sizes than canvassing or phone calls. Extant literature on fliers as a persuasive source of information finds null treatment effects (Incerti 2019; Kalla & Broockman 2018), but the evidence in the context of ballot initiatives is more mixed.

Canvassing would provide us with less power, less geographic reach, and would likely be costlier. However, direct contact has been shown to elicit larger treatment effects than other

forms of communication ([Kalla & Broockman 2018](#)).

In both cases (mail or canvassing), we will consider the possibility of partnering with a third party organization. If we send postcards for example, the source of the postcard will become a crucial part of the design. If we do not want our experiment to be about messenger effects (i.e. if we want it to be exclusively about the content, and not the source), then both treatments (interest group and expert messages) should come from the same source. We could also decide that the source of the information is part of the treatment we are interested in studying, in which case we could use different sources.

Moreover, it may be possible to improve upon previous field experiments by incorporating estimates of non-compliance rates into our experimental design, allowing us to estimate complier average causal effects (CACE) in addition to intent-to-treat (ITT) effects. For example, fliers could contain a link to a website with the informational treatment (ideally with some sort of monetary incentive, such as the possibility to enter a lottery). We would then be able to use the percentage of individuals who received fliers that also accessed the website as an estimate of the proportion of compliers. This would allow us to measure the effect of *reading* the informational treatment, rather than the effect of being assigned to the treatment group receiving the information.

4.4 Outcomes

Our outcome measurement is percentage of "Yes" votes on the ballot initiative at the precinct or smaller level. This constitutes a theoretically direct outcome measurement, considering that we are interested in examining how susceptible voters in a direct democracy setting are to information campaigns by different groups.

As our outcome is "Yes" vote and we will provide one pro-initiative treatment and one negative anti-initiative treatment, we should illicit one positive treatment effect and one negative. If the absolute values of the treatment effects are the same, or if the business treatment is higher in absolute value, that supports the capture hypothesis (i.e. people

weigh both sources of information equally). If the business treatment is smaller in absolute value, it disproves the capture hypothesis and suggests that people value research more than potentially biased information.

In addition, we will look for the existence of two potential heterogeneous treatment effects: (i) partisanship of the precinct (as measured by a synthetic variable using results for federal elections over the last 10 years) and (ii) average education or income level of the precinct.

4.5 Election cycle and choice of ballot measure

We seek to launch this experiment during a 2020 statewide general election. An on-year election will give us access to a larger sample of likely voters in total, as well as a greater percentage of voters without strong priors on the specific proposal. Off-year voters tend to be more informed about specific ballot measures and therefore have stronger priors and are less susceptible to informational campaigns.

Our ideal ballot measure would involve a policy with a scientific or policy research consensus on its overall merit or demerit. This would likely represent a technical subject on which people have relatively weak priors. Policy issues such as health care or housing could work very well for our purposes.

4.6 Power analysis

Bloom, Bos, and Lee (1999) and Raudenbush (1997) provided excellent illustrations of power analysis for cluster random assignment. We can also rely on existing field experiments conducted during an election and using precincts as clusters. This power analysis will allow us to evaluate how many clusters (i.e. precincts) we will need to make this experiment worth conducting.

5 Conclusion

Our next steps on this project include finalization of some of the research design decisions that remain open (i.e. choice of ballot initiative, location and creation of informational messages,

possible partnership with organizations to send the messages) and the power analysis. We hope to finalize these details over the summer and to preregister a final Pre-Analysis-Plan with EGAP by the end of 2019. We would then conduct the survey experiment at the end of 2019 and early 2020. Depending on the results of this pilot, we would conduct the field experiment immediately prior to the 2020 state general elections.

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A Appendix

Table A.1: All currently approved 2020 ballot initiatives by state

	State	Title
1	Alaska	Changes to State Board of Education Amendment
2	Alaska	Judicial System Amendment
3	Alaska	Authorize Legislature to Recompile the State Constitution Amendment
4	Alaska	Citizen Requirement for Voting Amendment
5	Alaska	Judicial Vacancies Amendment
6	Arkansas	Transportation Sales Tax Continuation Amendment
7	Arkansas	State Legislative Term Limits Amendment
8	Arkansas	Initiative Process and Legislative Referral Requirements Amendment
9	California	Criminal Sentencing, Parole, and DNA Collection Initiative
10	California	Tax on Commercial and Industrial Properties for Education and Local Government Funding Initiative
11	California	Replace Cash Bail with Risk Assessments Referendum
12	Colorado	Transportation Bond Issue
13	Illinois	Allow for Graduated Income Tax Amendment
14	Iowa	Constitutional Convention Question
15	Louisiana	No Right to Abortion in Constitution Amendment
16	Michigan	Use of State and Local Park Funds Amendment
17	Missouri	State Executive Term Limits Amendment
18	Montana	LR-130, Remove Local Government Authority to Regulate Firearms Measure
19	Montana	C-46, Initiated Amendment Distribution Requirements Measure
20	Montana	C-47, Initiated Statute and Referendum Distribution Requirements Amendment
21	Nebraska	Remove Slavery as Punishment for Crime Amendment
22	Nebraska	Tax Increment Financing Repayment Amendment
23	Nevada	Renewable Energy Standards Initiative
24	Nevada	Marriage Regardless of Gender Amendment
25	Nevada	State Constitutional Rights of Voters Amendment
26	Nevada	Remove Constitutional Status of Board of Regents Amendment
27	Nevada	State Board of Pardons Commissioners Amendment
28	New Mexico	Appointed Public Regulation Commission Amendment
29	North Dakota	Legislature Approval for Initiated Amendments Measure (SCR 4001)
30	North Dakota	Amendment Changing the Membership and Terms of the Board of Higher Education (SCR 4016)
31	Utah	Municipal Water Resources Amendment
32	Utah	Legislator Qualifications Amendment
33	Utah	Removal of Exception to Slavery Prohibition for Criminals Amendment
34	Utah	Gender-Neutral Constitutional Language Amendment
35	Wisonsin	Marsy's Law Amendment
36	Wyoming	Constitutional Amendment A

Table A.2: Potential 2020 California ballot initiatives as of June 2019

Initiative	Topic	Endorsement	Approved	Ideal
Jury trials for child custody	Legal	NA	Yes	No
Rental Affordability Act	Housing	NA	No	Maybe
Risk-based bail	Criminality	NA	No	No
Number of candidates in general elections	Politics	NA	No	No
GMO ban (SI)	Health	NA	No	Maybe
Fluoride ban (SI)	Health	NA	No	Maybe
Remove school vaccine requirement (SI)	Health	NA	No	Maybe
Supermajority for revenue measures	Politics	NA	No	No
Amend three strikes law	Criminality	NA	No	Maybe
Felony for some misdemeaners	Criminality	NA	No	Maybe
Confinement of farm animals	Agriculture	NA	No	No
Estate tax for college aid	Tax & Education	NA	No	Maybe
No changes on approved bond spending	Politics	NA	No	Maybe
Elimination of Open Ended Alimony	Legal	NA	No	No