

# 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 technical knowledge by others. In order to test these hypotheses empirically, we propose a field experiment examining voting outcomes on citizen initiated ballot propositions following randomized treatment of proposition endorsement or opposition by special interest groups and policy experts during the 2020 California state elections. We seek to test whether citizens acting as policy makers give more credence to high quality policy research versus special interest information. In addition, we will replicate our field experiment with a survey experiment using identical subjects, treatments, and outcome measures.

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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 adoption of good policies (Achen & Bartels 2017). The need for technical knowledge may be larger on a ballot initiative than in a traditional partisan election, as fewer heuristic cues are available to voters.

We seek to determine how voters use information when asked to vote directly on a supposedly non-partisan, technical policy proposal placed on a ballot initiative. Are voters able to distinguish between the different levels of information quality and bias that stem from varying sources of information? Relatedly, do ballot initiatives actually lead to less interference from parties and special interests, or are voters equally swayed by special interests cues and objective policy research?

We propose a field experiment examining voting outcomes on a citizen initiated ballot proposition following randomized treatment of proposition endorsement or opposition by special interest groups and policy experts during the 2020 California elections. We seek to test whether citizens acting as policy makers give more credence to high quality policy research versus overtly partisan or biased information (i.e. special interests). In addition, we will replicate our field experiment with a survey experiment using identical subjects and treatments.

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.

A growing literature also suggests that survey experimental results often do not replicate in the field. For example, the zero effect of campaign contact in [Kalla and Broockman \(2018\)](#) does not replicate in survey experiments, and a recent meta analysis of the effects of providing voters with information about the corrupt actions of politicians shows an approximately zero effect in field experiments versus large negative effects in survey environments ([Incerti 2019](#)). By contrast, we hypothesize that survey experiments should better approximate the context of real-world 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. Whether our hypothesis is correct or incorrect, we will contribute to the growing understanding of the contexts and questions in which survey experiments replicate or do not replicate in the real world. Conducting a survey experiment will also allow us to get into potential mechanisms behind our primary outcome (voting decisions), as we will also ask the respondents factual questions on the policy topic on which they were asked to make a decision.

## 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. Over the past two decades California has voted on X ballot initiatives at the statewide level [compare to other states], and the number of ballot initiatives per election has increased rapidly over the same period.

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 (synthesis of a Citizen Assembly held on the topic received in their voters pamphlets) ([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](#)). The need for technical knowledge may be larger on a ballot initiative than in a traditional partisan election as fewer heuristic cues are available.

This implies that the outcomes of direct democracy are 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 comparing the efficacy of special interest messages to those of independent policy analysts.

Ballot initiatives have also been identified as a source of major policy blunders. One of the most notorious of these blunders 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.

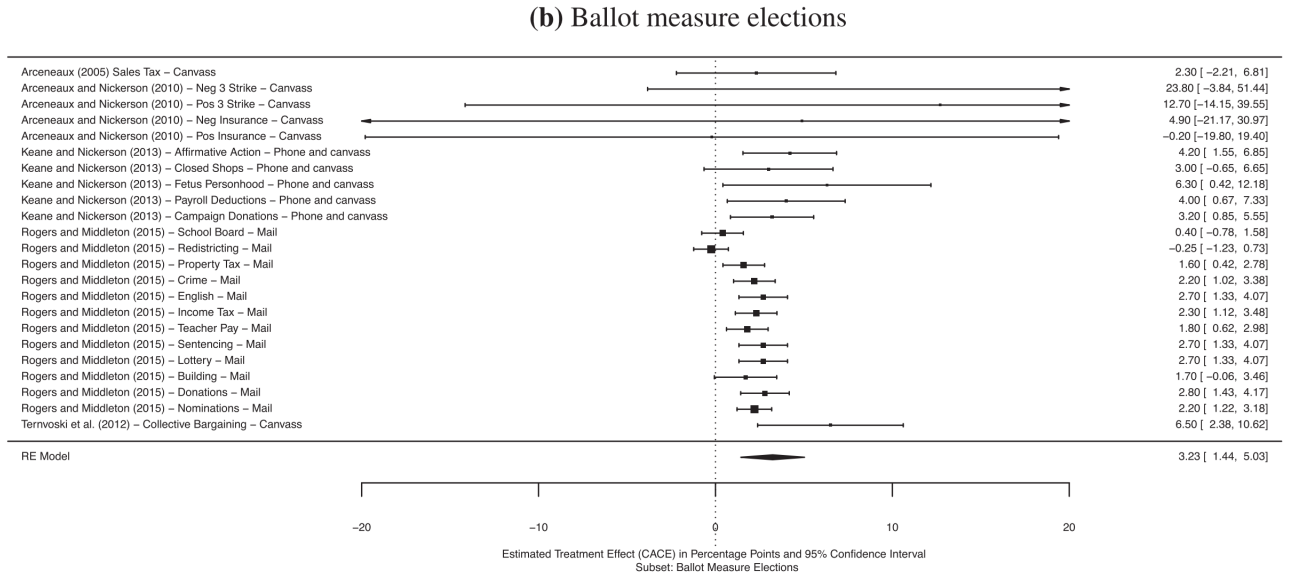
In line with the partisan and special interest capture theories, as well as evidence that ballot initiatives often lead to policy blunders, our primary hypothesis is that *voters will be equally swayed by information from special interest groups and policy researchers*. More formally, we expect the absolute value of special interest and policy research treatment effects to be statistically indistinguishable from one another.

### 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). Incerti (2019) draws a similar conclusion in a meta-analysis of corruption information field experiments, finding no voter punishment of allegedly corrupt politicians across field experiments. 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?



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

## 3 Field experiment

### 3.1 Treatment groups

We will provide voters with different randomized pieces of information encouraging either a “Yes” or “No” vote on a 2020 California ballot initiative. 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.

### 3.2 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:

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

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 total clusters, 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:

$$\hat{SE}(A\hat{T}E) = \sqrt{\frac{NV\hat{ar}(\bar{Y}_j(0))}{K(N-m)} + \frac{NV\hat{ar}(\bar{Y}_j(1))}{Km}}$$

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?]

### 3.3 *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.

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.

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.

### 3.4 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.

### *3.5 Election cycle and choice of ballot measure*

We seek to launch this experiment during the 2020 California 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. A list of all approved 2020 state ballot initiatives can be found in [Table A.1](#), and a list of approved and potential 2020 California ballot measures is provided in [Table A.2](#).

### *3.6 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.

## **4 Survey experiment**

### *4.1 Why conduct a survey experiment?*

Conducting a survey experiment in complement of the field experiment will provide at least two benefits: (i) substantially, explore some potential mechanisms and (ii) methods wise, contribute to the growing literature on the replicability of survey experiments results in the

field.

Substantially, a survey experiment allows us to add a set of outcome variables: 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. If we find differences in our primary outcome (voting decision), adding this second set of outcomes could help us test whether this difference in voting can be partly explained by a difference in the understanding of the issue. [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 other sources of information (interest group, neutral informational by government). But they do not study vote as the outcome variable of interest, and do not have a field experiment.

A second reason to do a survey experiment is to contribute to a methods question. 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 or do not 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.

## 4.2 *Design*

The ballot initiative setting would allow us to create a survey experiment in which the subjects, treatment, and outcome variable are identical to those in the field setting. In other words, we could sample from the same population, use the same informational treatments that would be sent to voters in the field setting, and measure the same outcome—vote choice—with the exception that the outcome is real in the field setting and hypothetical in the survey setting. We would then simply add a set of basic factual questions as a second set of outcomes.

## 5 Natural experiment

We are keeping the possibility of conducting a natural experiment on this topic open at this point, even though we have not found a specific workable design yet. The idea would be to use a real-life variation of information supply (such as data on billboards or a report sent to some precincts). The information provision will very likely not be random in this case, but we could rely on methods such as differences and differences to measure the effect of information provision on an outcome measure to be defined (vote on other measures, in previous elections, early voting, opinion polls).

We could also potentially leverage the signature requirement thresholds to put a ballot initiative on the ballot as a discontinuity (arguing that proposals just below the threshold are substantially the same as proposals just above the threshold, and measuring the causal effect of putting a proposal to a vote through a RD design. This would probably be another project, however, since we do not see how we can use information provision as a treatment in that case.

## 6 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 experiment immediately prior to the 2020 California 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	Wisconsin	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 misdemeanors	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