

Carattini, Stefano; Chatterjee, Anomitro; Cherry, Todd

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Voting and Information: Evidence from a Field Experiment

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Poschingerstr. 5, 81679 Munich, Germany

Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de

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Voting and Information: Evidence from a Field Experiment

Abstract

Biased beliefs affect real-world decisions, including political solutions to societal challenges. One crucial example is environmental policy: people tend to underestimate the incentive effect of Pigouvian policies. Addressing biased beliefs at scale is then paramount. In the days leading up to a ballot initiative in Washington state, we implemented a large-scale field experiment providing information on carbon taxes to over 285,000 individuals. We complemented it with a survey experiment of about 1,000 individuals, with the same treatments as in the field experiment, shedding light on social desirability bias and mechanisms around belief revision. Using data at the voting precinct level, we show that our intervention increases revealed support for carbon taxes, mainly for a treatment centered around earmarking of tax revenue, which was one of the design features of the ballot initiative. We find the effect to be stronger in precincts relatively opposed to the initiative, and less exposed to media coverage of carbon taxes, and more exposed to coverage challenging their effectiveness.

JEL-Codes: C930, D720, D820, D830, H230, Q540.

Keywords: carbon taxes, voting behaviour, Facebook ads, natural field experiment.

*Stefano Carattini**
Department of Economics
Andrew Young School of Policy Studies
Georgia State University
55 Park Place
USA - Atlanta, GA 30303
scarattini@gsu.edu

Anomitra Chatterjee
Department of Geography and Environment
and Grantham Research Institute on Climate
Change and the Environment
London School of Economics and Political
Science, London / United Kingdom
A.Chatterjee6@lse.ac.uk

Todd Cherry
Department of Economics
University of Wyoming
Laramie / WY / USA
tcherry@uwyo.edu

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1 Introduction

An informed citizenry is one of the main tenets of a well-functioning democracy. According to Bartels (1996), “one of the most striking contributions to the political science of half a century of survey research has been to document how poorly ordinary citizens approximate a classical ideal of informed democratic citizenship.” Deviations from this ideal have been documented in a wide variety of contexts since then (e.g. Kull et al. 2003; Gentzkow 2006; Gerber et al. 2009; Enikolopov et al. 2011; Kendall et al. 2015). In more recent times, political polarization in the United States as well as abroad, and the systematic emergence of “fake news,” have further contributed to creating misinformed citizens (Allcott and Gentzkow 2017; Allcott et al. 2019; Pennycook et al. 2020a,b; Hangartner et al. 2021).

In this paper, we provide evidence from a large-scale field experiment providing information to voters about a ballot initiative aiming to implement a carbon tax in Washington state, Initiative 1631 (I-1631). We complement the field experiment with a survey experiment providing exactly the same information to a representative sample of respondents, aimed at measuring potential social desirability bias, and examine the potential mechanisms underlying the effects on actual voting behavior. In the field experiment, information was provided in the days leading up to the vote through Facebook ads, randomizing treatment assignment at the precinct level. In the survey, information provision was randomized at the individual level. We inform voters in the field experiment and respondents in the survey experiment about (i) the ability of carbon taxes to reduce emissions (i.e. about their effectiveness) in one treatment and (ii) make more salient the environmental earmarking planned under

I-1631 in the other. We refer to the former as the “Effectiveness” treatment and the latter as the “Earmarking” treatment. In a context where biased beliefs have been widely documented, we find evidence of belief revision in the survey for both treatments and of an impact on voting behavior in the field experiment for at least one of them, namely the Earmarking treatment.

In the literature, biased beliefs have been generally corrected through lab and survey experiments, providing randomized information to individuals in controlled settings. Such approaches have been used in a wide variety of contexts, such as to improve the understanding of fiscal policy, immigration policy, or affirmative action (Stantcheva 2021; Alesina et al. 2023; Haaland and Roth 2023), among many others (see Haaland et al. 2023 for a review). However, the use of such approaches in highly controlled settings, where information is provided directly by the researchers, raises important questions of scalability, which is an important dimension to evaluate social interventions (Deaton 2010; Al-Ubaydli et al. 2017, 2021; List 2022). In other settings, it may be hard to have the undivided attention of thousands of voters, who would also likely be exposed to opposing arguments about the issue. There may also be questions of social desirability bias and experimenter demand effects in survey experiments, and potentially also in lab experiments depending on the stakes, as well as questions of persistence in the ability of informational treatments to close informational gaps. While the literature has started assessing the extent of potential experimenter demand effects (De Quidt et al., 2018; Mummolo and Peterson, 2019), here we focus specifically on social desirability bias. In less controlled environments, one would ideally like biased beliefs to be addressed permanently, or at least long

enough for voters to take an informed decision on an important issue.

Climate change is an example of a context with important information asymmetries between experts and the general public, not only concerning the problem, but also its solution, climate policy. Evidence from various sources, including leaked documents, point to intentional efforts by fossil fuel companies to muddy the waters on climate change and potentially climate policy as well (Oreskes and Conway 2011; Farrell 2016; Supran and Oreskes 2017; Brulle et al. 2020; Brulle 2021; Carattini et al. 2023; Supran et al. 2023). While generalized climate skepticism may be confined to a few contexts globally, including the United States, resistance to the use of economic instruments in climate policy is observed worldwide, largely originating from misperceptions, which also apply to other Pigouvian policies (Carattini et al. 2018). The following stylized facts have been identified by the literature and summarized in the review by Carattini et al. (2018), leveraging belief revision following direct experience with Pigouvian policies. Voters often tend to underestimate the benefits of Pigouvian policies, in particular their ability to lead to changes in behavior (perceived ineffectiveness), and overestimate potential drawbacks, including job losses (perceived negative impact on competitiveness). Such pessimism may dissipate once voters get to experience the policy, creating a rationale for gradual implementation, and trial periods for policies whose effects are highly visible (such as congestion charges or pricing garbage by the bag). Voters also tend to have a preference for policies with progressive distributional implications, assuming that they are informed about such progressivity. Given such biased beliefs, a growing literature has aimed at tackling information asymmetries about Pigouvian policies, with the

use of randomized informational treatments in surveys and in the lab, starting with Carattini et al. (2017) and Dal Bó et al. (2018), respectively. Perceived ineffectiveness is an important driver of opposition to Pigouvian policies at large and carbon taxes in particular, leading scholars to inform survey respondents about expected (including simulated) effects of carbon taxes since Carattini et al. (2017), including several studies realized after our field experiment (e.g. Dechezleprêtre et al. 2022; Douenne and Fabre 2022). When respondents underestimate carbon taxes’ ability to reduce emissions through changes in relative prices, they may be more likely to demand revenues to be earmarked for environmental purposes, as they may see it as the only way for carbon taxes to make an impact and not to just be a revenue generator.

Against this backdrop, we implemented a large-scale field experiment, complemented with a survey experiment, ahead of the vote on I-1631, the ballot initiative aiming to implement a carbon tax in Washington state in 2018. Our intervention focused on perceived effectiveness, or ineffectiveness, of carbon taxes through two separate treatments. The first treatment leveraged the implementation of a carbon tax in 2008 in the contiguous Canadian province of British Columbia. We provide potential voters with descriptive evidence suggesting that the carbon tax decreased refined petroleum product use in British Columbia compared to the rest of Canada, in line with the discussion and evidence in Murray and Rivers (2015). Hence, we show voters the potential of carbon taxes in reducing greenhouse gas emissions. In the case of British Columbia, the carbon tax was introduced as part of a revenue-neutral reform, with revenues used to reduce pre-existing distortionary taxes. Reductions

in greenhouse gas emissions in British Columbia have thus been the result of the incentive effect of carbon taxation. In the case of I-1631, however, revenues would have been earmarked for environmental and social purposes. This earmarking would, in principle, contribute to further reductions in greenhouse gas emissions, as well as other social goals. Given that voters may miss, entirely or in part, the incentive effect of carbon taxes, the second treatment leveraged the fact that under I-1631, all revenues from the carbon tax were to be earmarked, with a substantial portion set aside for environmental purposes. Hence, the second treatment made this way of using revenues especially salient. Our intervention was targeted at some 285,000 individuals, so already designed and deployed to inform voters at scale.

We find that information provision tends to increase support for carbon taxes, both in the survey (on voting intentions) and in the field experiment (on actual voting behavior). This finding applies in particular to the treatment leveraging earmarking as a feature of I-1631's design. The survey also allows us to measure potential social desirability bias, by analyzing treatment exposure among voters who self-declare in the survey to have already voted. We find that there is a positive but statistically insignificant effect of our treatments among those who have already voted. While the positive effect could be indicative of social desirability bias, the magnitudes are much smaller than the effects on those who have not yet voted. Thus, the extent of social desirability bias seems insufficient to call into question the use of survey experiments. Overall, our findings tend to be rather consistent with, and complementary to, Mummolo and Peterson (2019), who examine the role of experimenter demand effects, an issue that shares implications with social desirability

bias.

When looking at sources of heterogeneity to which we had committed when pre-registering this field experiment, and in particular at political partisanship, we observe in our survey data that the information treatments do not seem to affect voters with different political affiliations differently. However, we observe heterogeneity in the field experiment between Democrat-leaning and Republican-leaning precincts. We reconcile these two findings by documenting important variation in how the media most consumed in such precincts cover the issue of climate change and carbon taxes. We show that heterogeneity is driven more by the environment in which people live and the information to which they are exposed, directly and indirectly, than by partisanship in individual political attitudes.

As anticipated above, we contribute to several strands of literature. One strand of literature is on informed citizenry (e.g. Bartels 1996; Kull et al. 2003; Gentzkow 2006; Enikolopov et al. 2011), where two studies are close to ours. First, Gerber et al. (2009), where voters are provided at random with a free subscription to either the *Washington Post* or the *Washington Times*, and the information that they contain, ahead of a gubernatorial election in Virginia, with information from both newspapers increasing votes for Democrats. Second, Kendall et al. (2015), who collaborate with the re-election campaign of the mayor of Arezzo, a mid-sized Italian city. Kendall et al. (2015) randomize the content of the political messages that voters receive at home, assigning treatment at the precinct level, and examine their impact on voting behavior, identifying a difference between valence- and ideology-related messages. More recently, a related strand of literature has focused on the role of misinforma-

tion and polarization in politics, to which our paper also speaks (e.g. Allcott and Gentzkow 2017; Allcott et al. 2019; Pennycook et al. 2020a,b; Hangartner et al. 2021; Levy 2021). Then, we contribute to a growing literature examining public support for Pigouvian taxes, with a particular focus on information asymmetries (see Cherry et al. 2014; Carattini et al. 2017, 2018; Dal Bó et al. 2018; Carattini et al. 2019 for some early studies). Further, we add to a body of work using survey experiments to address biased beliefs in many contexts, related or unrelated to the topic of our study (e.g. Stantcheva 2021; Dechezleprêtre et al. 2022; Alesina et al. 2023; Haaland and Roth 2023). In this respect, Dechezleprêtre et al. (2022) confirm the existence of biased beliefs about carbon taxes across countries, as well as the ability of information provision to help address them in a survey experiment. While the findings in Dechezleprêtre et al. (2022) contribute to the external validity of our approach, in our paper we show the potential of information provision in a context that tracks the ultimate behavior of interest, voting. Finally, our paper also speaks to a stream of work on digital ads, as also summarized in Aridor et al. (2024), and including the concurrent work by Enríquez et al. (2021) on corruption in Mexico.

The remainder of the paper proceeds as follows. Section 2 provides some background information about carbon taxation in Washington state and I-1631. Section 3 introduces our research design. Section 4 describes the empirical approach used to obtain our main results, which are discussed in Section 5. Section 6 concludes.

2 Background

In this section, we describe the context of I-1631.¹ In 2016, voters in Washington state were first asked to vote on a carbon tax, namely Initiative 732 (I-732). The proponents of I-732, a group named Carbon Washington, aimed to implement a revenue-neutral carbon tax that, in their mind, would have appealed to both climate-friendly voters and moderate conservatives. The carbon tax under I-732 would have started at \$15 per ton of CO₂ in 2016 and increased gradually thereafter. Fossil fuels would have been taxed upstream. To contribute to ensure revenue neutrality, the state's sales tax would have been reduced from 6.5% to 5.5%. The aim was also to address some of the regressivity of the carbon tax, by reducing another regressive tax. Further, some of the revenues would have been allocated to match the Federal Earning Income Tax Credit at 25%. Additionally, local businesses would have benefitted from the elimination of the state's business and occupation tax for manufacturers. I-732 was supported by groups aligned with carbon taxation, such as the Citizens' Climate Lobby, Audubon Washington, and minor environmental groups, as well as local Democratic party chapters and the renewable industry. However, it was opposed by progressive organizations such as labor and social justice groups and other environmental groups, including the very influential Sierra Club, and the state Democratic party, besides the usual suspects, such as carbon-intensive industries. Voters rejected I-732 at 59% on November 8, 2016.

Following the rejection of I-732, to which the split among environmental groups contributed (Anderson 2017), I-1631 was announced in March 2018, led precisely

¹A similar description is provided in Carattini and Sen (2019).

by those environmental groups that had opposed I-732. The initial tax rate for this new proposal was also set at \$15 per ton of CO₂, to then increase gradually. However, this time the carbon tax was not designed to be revenue neutral, a key difference with respect to I-732. Revenues would have been used for environmental and social purposes, through three funds: a fund for clean air and clean energy; a fund promoting water quality and forest health; and a fund for community-related investments. The policy was labeled “fee” (instead of “tax”), following Washington state’s laws. The Crosscut/Elway opinion poll of October 11, 2018, gave I-1631 a very comfortable margin, with 50% respondents in favor and 36% against.² I-1631 was eventually also rejected, at 57%, on November 6, 2018.

In 2021, after our study, Washington state passed the Climate Commitment Act, which implemented a cap-and-trade scheme (or cap-and-invest as labeled by the legislators) covering about 75% of emissions in the state. Under this scheme, emissions allowances are auctioned and revenues (exceeding three billion dollars) earmarked for environmental purposes. The policy entered into force in 2023. In November 2024, Washington state voters rejected an initiative, Initiative 2117, aimed at repealing the scheme, with 62% of voters choosing to keep it in place, after having experienced it for about a year. In this respect, it is interesting to note how the earmarking of revenues took center stage in the political discourse, emphasized in particular by the

²Source: <https://www.cleantechalliance.org/2018/10/12/elway-poll-nations-first-carbon-fee-leading-among-voters> (last accessed November 27, 2020).

camp opposed to the initiative (and thus in favor of cap-and-invest).³

3 Research design

Our research design combines primary data collected through a survey of Washington state’s residents and a field experiment using administrative data at the precinct level. The two data sources complement each other. The survey allows us to examine the effect of our treatments on voting intentions at the individual level as well as to analyze the underlying mechanisms, in particular in terms of revision of beliefs about carbon taxes. When using survey data, inference is based on stated preferences. However, due to the possibility of early voting, some of the respondents report having already voted when exposed to our treatment, offering an opportunity to consider our treatments as “placebos” for this subsample of voters.

To maximize comparability between survey and field experiment, the treatments were implemented in both cases with the same figures coupled with short text. This choice is driven by the implementation of the field experiment, which relies on Facebook ads. In what follows, we describe our treatments.

3.1 Treatments

We first describe the treatments in words and then show the exact figures used in both the survey and field experiment.

³See the official website of the campaign against Initiative 2117 (<https://no2117.com/>; last accessed December 18, 2024) as well as media coverage such as in the *Washington State Standard* (<https://washingtonstatestandard.com/2024/09/05/foes-of-measure-to-repeal-wa-climate-law-launch-their-first-tv-ad/>; last accessed December 18, 2024) and in the *Seattle Times* (<https://www.seattletimes.com/seattle-news/politics/climate-advocates-finally-won-in-wa-how-by-not-talking-about-climate/>; last accessed December 18, 2024).

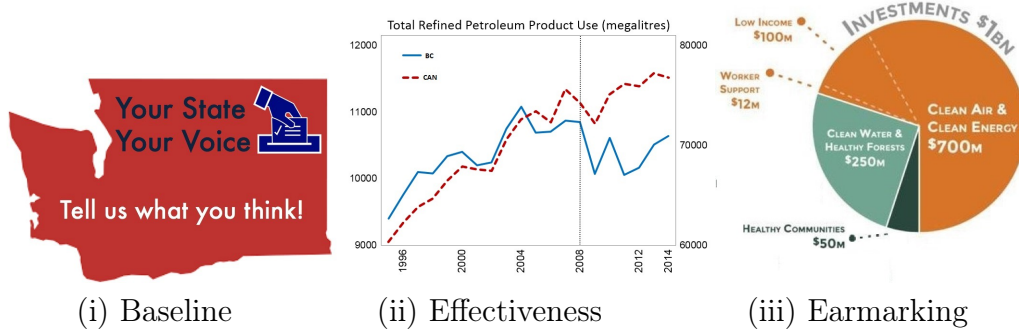
Baseline In the neutrally-framed Baseline treatment, we simply ask respondents to share their opinion regarding carbon taxes. A figure representing the silhouette of Washington state and including two text entries, “Your State Your Voice” and “Tell us what you think!” was coupled with the following short text: “Washington Initiative 1631 is on the ballot this November 6th. What do you think? Your input can help future policymaking.”

Effectiveness The first treatment focuses on the effectiveness of carbon taxes in changing people’s behavior and is henceforth named “Effectiveness.” Like the Baseline treatment, it also combines a figure with short text. The Effectiveness treatment leverages the geographical proximity between Washington state and the Canadian province of British Columbia, which implemented a carbon tax in 2008. Empirical evidence suggests that the British Columbia carbon tax led to substantial reductions in emissions since its implementation, compared to the counterfactual (Murray and Rivers 2015). Our Effectiveness treatment summarizes this evidence, providing the following text: “I-1631 is on the ballot. When British Columbia introduced a carbon tax in 2008, emissions of CO₂ decreased by up to 10%.” The figure illustrates such effect, by showing the trajectory of total refined petroleum product use, in megaliters, between the late ’90s and the mid-2010s, in British Columbia compared to the rest of Canada. The trajectories are generally similar until 2008 when a carbon tax was implemented. The tax was announced in February and implemented in July. After 2008, petroleum product use increased steadily in the rest of Canada while it

declined in British Columbia.⁴

Earmarking The second treatment focuses on the use of revenues from the carbon tax suggested by the committee behind I-1631 and in particular on how it differed from what was proposed under I-732. Recall that I-1631 proposed that revenues from the carbon tax would be earmarked for environmental and social purposes. Hence, we refer to this treatment as “Earmarking.” Like the Baseline and the Effectiveness treatments, the Earmarking treatment also combines short text and a figure. The short text is as follows: “I-1631 is on the ballot. In contrast to I-732, it would invest about \$1 billion in clean air, water, energy, and communities.” Accordingly, the figure that we shared used a pie chart to show how the revenues would have been earmarked across different funds and goals.

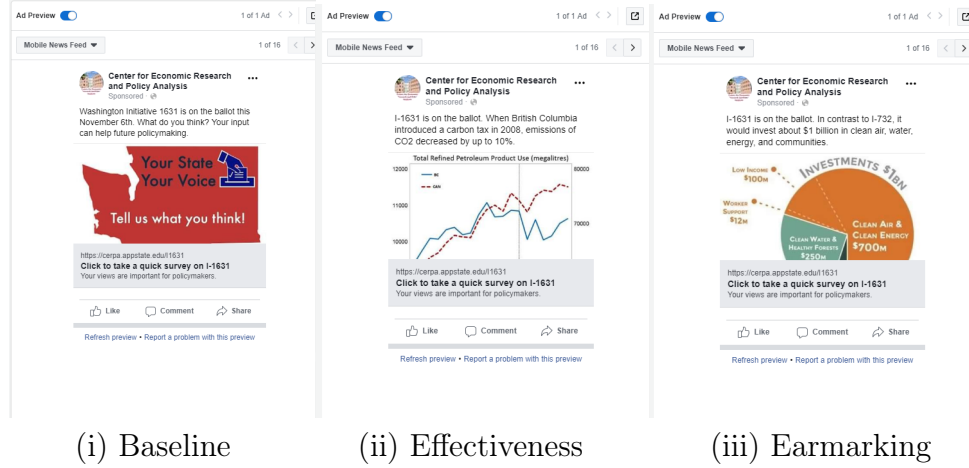
Figure 1: Treatments in the survey



Notes: Figures shown to survey participants as part of each treatment. The treatments were randomized at the individual level.

⁴The figure used in this treatment was sourced from Wikipedia. Time is on the horizontal axis (1996-2014) and total petroleum product use in megaliters on the left axis. The left vertical axis is for British Columbia and the right vertical axis is for the rest of Canada. The figure is similar in spirit to Figure 2 in Murray and Rivers (2015).

Figure 2: Treatments in the field experiment



Notes: Messages shown to voters in Washington state through Facebook. The treatments were randomized at the voting precinct level. All individuals registered on Facebook to be residing in a treated precinct could potentially be exposed to one of the three messages through their Facebook or Instagram feed. If people were inclined to also click on the ads, they could have filled a survey as well, which is however not used to study our intervention, since we rely on simple exposure to the ads.

In what follows, we describe how the treatments were implemented in the survey and in the field experiment.

3.2 Survey

The survey was administered by a private marketing company on a sample of 1,000 adult residents in Washington state. Responses were collected between October 24 and October 30, 2018. Since early voting opened on October 19, 2018, part of our sample had already voted by the time they were surveyed.

The survey was structured in parts. First, a screening section, based on age and state of residence, followed by a standard informed consent protocol. Second, a short introductory text describing I-1631, followed by one of the treatment messages.

Third, questions about I-1631, including voting intentions, beliefs about the proposed policy’s effectiveness, and information on whether the respondent had already voted. Fourth, a battery of questions on climate change beliefs and attitudes. Fifth, a short section with standard socioeconomic questions. Appendix Section E provides our full survey instrument.

3.3 Field experiment

In this part of the study, we are interested in assessing the effect of providing publicly available information about carbon taxes on voting behavior, to infer from revealed preferences and complement the evidence based on stated preferences provided by the survey. Voting behavior is measured through administrative data provided by the Secretary of State in Washington (WA SOS). Administrative data provide information on ‘yes’ votes as well as on turnout. The data are available at different geographical levels, with the precinct being the most disaggregated one.

For the purposes of targeting and statistical power, precincts are the ideal unit of randomization in this experiment (see Appendix B.3 for detail on power calculations). As of November 2018, Washington state had 7,317 voting precincts with an average of 660 registered voters per precinct. Facebook allows automatic bulk targeting of advertisements to different types of geographical areas. The narrowest level for automatic targeting is, however, the zip code. In Washington state, there are 733 zip codes. Each zip code contains on average 9,101 residents.⁵ Hence, zip codes are on average 10 times larger than precincts. Facebook also provides a manual solution,

⁵Data available from <https://www.unitedstateszipcodes.org/zip-code-database/> (last accessed, November 27, 2020).

which we used in this experiment, to target geographical areas that are smaller than zip codes. Specifically, Facebook allows advertisers to select a point on a map by manually dropping a “pin” at a GPS coordinate (latitude-longitude), and selecting the size of a radius around the point. The smallest radius allowed is one mile, with potential increments of one mile, up to 100 miles. Only circles are allowed with this approach, not polygons.

We implemented this solution as follows. We first used the centroids of the polygons representing each precinct as the GPS coordinate at which the pins were dropped. Even applying a one-mile radius around the centroid of randomly selected precincts can cause two issues. First, if the precinct is relatively small, the buffer may extend beyond the precinct’s boundaries, contaminating contiguous precincts. Second, if the precinct is relatively large, the buffer may only cover a small share of the precinct, leading only a small part of it to be treated. We refer to this issue as undertreatment. While the radius can be adjusted so that a sufficient area of the precinct’s territory is covered, by doing so one may exacerbate the aforementioned issue of contamination, especially when the precinct’s shape is substantially different from circular, which is often the case.

To address these issues, we selected a subsample of 980 precincts for inclusion in the experimental sample with the aim of reducing potential contamination as well as undertreatment. Then, we randomly assigned the Baseline, Effectiveness, or Earmarking treatment to each of these selected precincts, with the goal of ensuring balance (a priori) on precinct-level sociodemographic characteristics.⁶ Table B.10

⁶The intervention was preceded by standard communication with Washington state’s Public Disclosure Commission (PDC). Our treatments, reporting information already available in the

provides the balance of covariates across treatment conditions as well as a comparison with the rest of the state’s precincts.⁷ In our empirical analyses, we first compare across treatments within the set of precincts selected into the experimental sample. Then, we move beyond the experimental sample. Further details on how precincts were selected are included in Appendix B.1. The resulting assignment of precincts between Baseline, Effectiveness and Earmarking treatments, and no advertisements is shown in Figure 2b in the Appendix.

4 Empirical approach

4.1 Data and descriptive statistics

In this section, we describe the data used for our main analyses. We use two sources of data: our primary survey and administrative data on voting behavior and other precinct- and census-tract level characteristics. Summary statistics for our survey sample are presented in Appendix Table A.1, along with summary statistics for the underlying population. The survey sample is fairly representative of the underlying population, although it is composed of slightly older and slightly better educated residents. In terms of party affiliation, our survey is similar in composition to the underlying population of Washington state, with 51% declaring to have voted Democrat in the 2018 elections (versus 54% in the population) and 36% to have voted Republican (versus 38% in the population). Further, Table A.2 in the Appendix, shows

public domain, were categorized as purely informational by the PDC.

⁷From the WA SOS office, we were able to source coordinates of precincts that changed boundaries between 2016 and 2018. This allows us to map precinct-level voting outcomes in 2016 to those in 2018.

that our treatments are well balanced across most covariates, with some imbalances for instance on income categories, which are nevertheless included as controls in our econometric estimations.

Turning to the administrative data, we use socioeconomic characteristics provided at the census-tract level by the US Census Bureau, namely American Community Survey (ACS) 5-year estimates for 2012-2016, and voting behavior at the precinct level provided by WA SOS. In 2018, the state had 1,458 census tracts and 7,317 voting precincts. However, voting precinct boundaries are not clearly nested within census-tract boundaries. Therefore, we adapt the strategy of Anderson (2017) to map census-tract level demographic information to precinct-level vote statistics. The process is as follows. First, we calculate the share of population of each precinct within each census tract using the voter registry database of Washington state. Second, we use the share of population within each census tract to weight demographic characteristics and obtain population-share weighted averages of the census-tract level variables mapped to each voting precinct.

Table B.8 in the Appendix documents correlations between the outcomes of interest pertaining to I-1631 and precinct-level characteristics. The sample for this table excludes precincts that are included in the field experiment in order to uncover relationships in the absence of any treatment effects. Note that many of these characteristics (e.g. share of Republican voters and share of population commuting by car) are, as expected, statistically significantly correlated with our main outcomes of interest. Hence, these factors are controlled for in our empirical analyses. We also include precinct-level summary statistics by treatment status in Table B.9 and

test balance of covariates between treated and control precincts in Table B.10 in the Appendix.

4.2 Econometric specifications

In this section, we describe the empirical approaches used to analyze survey data and administrative data. We start with the former. For the analysis of survey data, we focus on two main dimensions. First, we examine reported voting behavior and voting intentions. Recall that given the timing of the survey administration, we observe both individuals who have not yet voted, for whom we can measure voting intentions, and individuals who have already voted, for whom we can measure reported voting behavior. Second, we examine beliefs about carbon taxes. In particular, we have respondents' beliefs about the effectiveness of carbon taxes to reduce greenhouse gas emissions and to reduce local air pollution. In this way, we can measure whether the treatments lead to belief revision. Our main estimation strategy for the survey is the following linear probability model (LPM):

$$y_i = \alpha + \beta_1 * Effectiveness_i + \beta_2 * Earmarking_i + \gamma * X_i + \epsilon_i \quad (1)$$

where y_i is a binary variable indicating whether respondent i will vote (or has voted) 'yes' on I-1631, $Effectiveness_i$ and $Earmarking_i$ are dummy variables that take value 1 if individual i receives the Effectiveness or the Earmarking treatments, respectively, and X_i denotes individual-level covariates, including age, gender, household size, income, education, and political party preferences. Our coefficients of interest are β_1 and β_2 , which capture the effect of each treatment compared to the Baseline

condition. Since our outcome is a binary variable, we also estimate a probit model with the same outcome and independent variables.

Moving to the field experiment and administrative data, we first estimate the effect of our treatments within the sample of 980 precincts that are selected into the experiment. To account for the fact that our treatment is delivered to individuals via Facebook while our outcome of interest is at the precinct-level, we set analytic weights for each precinct equal to the number of registered voters. We then estimate the following linear specification:

$$y_i = \alpha + \beta_1 * Effectiveness_i + \beta_2 * Earmarking_i + \gamma * X_i + \epsilon_i \quad (2)$$

where y_i is the share of ‘yes’ votes or the absolute number of ‘yes’ votes in precinct i , $Effectiveness_i$ and $Earmarking_i$ are dummy variables that take value 1 if precinct i is assigned the Effectiveness or the Earmarking treatment, respectively, and X_i denotes a set of precinct-level political covariates, including the share of ‘yes’ votes for the previous carbon tax ballot measure (*‘Yes’ share for I-732*), the share of votes for Hillary Clinton in the 2016 Democratic party primary (*Primary (2016) Clinton*), the share of Democrat votes in the 2018 senate elections (*Democrat share (Senate 2018)*), the share of Republican votes in the 2016 Presidential election (*Republican share (2016)*), and the number of registered voters (*Registered voters*). Our coefficients of interest are β_1 and β_2 , which indicate the effect of each treatment compared to the outcomes in the Baseline treatment.

Equation 2 is our main specification for the field experiment. However, we also extend our interpretation beyond the experimental sample to all precincts within

the state of Washington. To this end, we proceed by first estimating the following equation:

$$y_i = \alpha + \beta_1 * \textit{Baseline}_i + \beta_2 * \textit{Effectiveness}_i + \beta_3 * \textit{Earmarking}_i + \gamma * X_i + \epsilon_i \quad (3)$$

which adds an indicator, $\textit{Baseline}_i$, for precincts that are assigned the baseline treatment arm. Therefore, β_1 , β_2 , and β_3 now indicate the effect of being exposed to the neutral Baseline treatment, the Effectiveness treatment, and the Earmarking treatment, respectively, compared to precincts that are not exposed to any of the three treatments, since they are outside the experimental sample. In addition to the political covariates mentioned earlier, X_i now includes sociodemographic covariates at the precinct-level, namely percentage of males (*Male (percent)*), white people (*White (percent)*), car commuters (*Commuting by car (percent)*), public transport users (*Commuting by public transport (percent)*), percentage of people with a bachelor’s degree or higher (*Bachelor degree (percent)*), median age, and median income.

We examine heterogeneous effects of our treatments by interacting the treatment variables with precinct-level covariates. We focus on two obvious margins of potential heterogeneous effects, namely (i) the share of Republican voters in the 2016 presidential elections, and (ii) the share of voters who chose Hillary Clinton over Bernie Sanders in the preceding Democratic primary elections. Indeed, as shown in Table B.8 in the Appendix, I-1631 received higher support in Democratic-leaning precincts than Republican-leaning precincts and, among Democratic voters, support was higher in Sanders-leaning precincts than in Clinton-leaning precincts. Recall

that I-1631 aimed at implementing a carbon tax whose revenues would have been used for environmental spending, along the lines of the Green New Deal, as advocated by economist Edward Barbier (Barbier 2010) and later by Representative Alexandra Ocasio-Cortez (Ocasio-Cortez 2019), among others.

We use several robustness tests to corroborate our results from the field experiment. First, to account for the fact that our main dependent variable (share of ‘yes’ votes) is bounded between 0 and 1, we use a non-linear model to complement the ordinary least square estimation. Specifically, we estimate fractional logit models as described in Section B.5.1.

Second, our preferred linear specification estimates intent-to-treat effects, since we cannot observe whether individuals in the precinct actually see our messages on their Facebook feed. We also do not observe a precinct-level estimate of the number of times the ads were displayed on Facebook feeds, only the number of times the ads were shown by treatment status. Moreover, some individuals may not be affected by our ads, because they had voted already. Further, because of our use of buffers, precincts may not be covered by ads in their entirety. We therefore expect our estimates to be lower bounds of the true average treatment effect of receiving the information. However, we can use treatment assignment as an instrument for precinct-level “coverage” of our treatments. We calculate precinct-level coverage using the share of voting population that could plausibly be exposed to our treatments since they are within the buffer area where the messages are delivered. The results from this approach are discussed in Section B.5.2. Importantly, this approach corrects for the fact that exposure was not universal, not for the fact that some in-

dividuals exposed to the ads might have ignored them or been exposed after having voted, so that some degree of conservativeness remains.

Third, we consider the possibility that precincts that are not assigned to our treatments can still be exposed to our messages since the buffers within which the messages are shown can sometimes overlap precinct boundaries. In this way voting precincts may have “extra exposure.” Section B.5.3 contains further details on estimations accounting for potential extra exposure.

Finally, we consider additional covariates as a robustness check in Section B.5.4 of the Appendix.

5 Empirical results

5.1 Effects in the survey

In what follows, we present our main empirical results. We start with the survey data and then extend to the analysis of the field experiment, which includes both average and heterogeneous treatment effects. Table 1 provides the main results from the survey data, estimating model (1) to find the effect of treatment exposure on reported voting behavior and voting intentions. Odd-numbered columns do not include socioeconomic characteristics as control variables, while even-numbered columns do. Table A.3 in the Appendix provides coefficients for all control variables, which generally show plausible signs.

Columns (1) and (2) analyze treatment effects on reported voting behavior. Since people already voted before being exposed to the treatment, columns (1) and (2) are

considered a placebo test. Estimates from columns (1) and (2), which are very similar and statistically indistinguishable, indeed point to no statistically significant effect of the treatments on reported voting behavior. The point estimates of around 6% may still point to some degree of social desirability bias, which our survey design allows to isolate. The extent of social desirability bias is, however, fairly limited, supporting the use of survey experiments. The field experiment infers from revealed preferences and thus does not suffer from social desirability bias.

Columns (3) to (6) focus on voting intentions. Columns (3) and (4) exclude undecided voters who had not yet made up their mind at the time of completing the survey. Columns (5) and (6) treat undecided voters as ‘no’-votes, as an alternative specification. According to the estimates in columns (3) and (4), the Effectiveness treatment led about 14% to 20% of respondents to shift to a ‘yes’-vote, while the Earmarking treatment led about 10% to 11% of respondents to shift to a ‘yes’-vote in terms of voting intentions, compared to the control group which was exposed to the neutral Baseline message. According to the estimates in columns (5) and (6), the Effectiveness treatment led about 13% to 17% of respondents to shift to a ‘yes’-vote, while the Earmarking treatment led about 10% of respondents to shift to a ‘yes’-vote in terms of voting intentions, compared to the control group.

Table 1: Survey: average treatment effects

Outcome	Voted ‘yes’		Will vote ‘yes’			
Sample	Already voted		No undecided voters		With undecided voters	
	(1)	(2)	(3)	(4)	(5)	(6)
Effectiveness	0.058 (0.076)	0.064 (0.069)	0.197*** (0.055)	0.144*** (0.053)	0.172*** (0.043)	0.129*** (0.045)
Earmarking	0.058 (0.076)	0.066 (0.070)	0.098* (0.056)	0.114** (0.053)	0.103** (0.043)	0.099** (0.045)
Controls	No	Yes	No	Yes	No	Yes
Adjusted R^2	-0.005	0.277	0.022	0.278	0.019	0.141
Respondents	263	242	494	442	717	613

Notes: Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Individual-level control variables include age, gender, income, education, household size, employment status and political party identification. Coefficients for all control variables are displayed in Table A.3 in the Appendix.

We also estimate a non-linear probit specification, reported in Table A.4 in the Appendix. The magnitudes of the marginal effects of our treatments are very similar to those estimated using the linear probability model.

As described, two main advantages of the survey data consist in the ability of observing voting intentions at the individual level as well as measuring the extent of potential social desirability bias. Yet another benefit of the survey data is that it allows us to investigate potential mechanisms behind the effects reported in Table 1 at the individual level. In particular, given our design, we are interested in belief revision related with the effectiveness of carbon taxes. To this end, the survey included two questions asking respondents to rate the effectiveness of I-1631 at reducing emissions as well as at reducing local pollution. We test whether these responses are affected by our treatments and report the results in Table 2. We also estimate a non-linear

probit specification, reported in Table A.6 in the Appendix.

We observe that the Effectiveness treatment leads to belief revision on both dimensions (emissions and local pollution), leading fewer respondents to rate I-1631's effectiveness as "Poor" and more respondents to rate it as "Good." The effects are very similar for emissions and local pollution. The Earmarking treatment seems to lead to effects in the same direction, albeit of a lower magnitude. Given comparable standard errors, the effects of the Earmarking treatment are not statistically significant at the 10% level.

Table 2: Survey: beliefs about impacts of I-1631

Survey question	How would you rate I-1631?			
Sub-category	Reducing emissions		Reducing local pollution	
Likert rating (outcome)	Poor	Good	Poor	Good
Effectiveness	-0.080*** (0.030)	0.079** (0.039)	-0.086*** (0.033)	0.071* (0.039)
Earmarking	-0.047 (0.032)	0.053 (0.039)	-0.048 (0.035)	0.046 (0.039)
Constant	0.091 (0.103)	0.236 (0.160)	0.206 (0.136)	0.556*** (0.162)
Controls	Yes	Yes	Yes	Yes
Adjusted R ²	0.138	0.138	0.101	0.125
Respondents	855	855	855	855

Notes: Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Individual-level control variables include age, gender, income, education, household size, employment status and political party identification. Coefficients for all control variables are displayed in Table A.5 in the Appendix.

5.2 Main effects in the field experiment

In what follows, we present our main results from the field experiment. As mentioned in Section 4, we start with the experimental sample and then extend to the rest of the state. Heterogeneity and mechanisms are then analyzed in Section 5.3.

5.2.1 Effects within the experimental sample

Table 3 presents our main results for the experimental sample. We consider two main outcome variables. The share of ‘yes’ votes and the number of ‘yes’ votes. Column (1) shows the intent-to-treat effect on the share of ‘yes’ votes for precincts assigned to each treatment arm compared to precincts in the Baseline treatment. In the precincts within our experimental sample, the Earmarking treatment causes a statistically significant 0.8 percentage point increase in share of ‘yes’ votes, compared to the precincts that are exposed to the Baseline treatment. Given that the average share of ‘yes’ votes in precincts in the Baseline treatment is 30.79%, our Earmarking treatment causes a 2.6% ($0.8 \times 100 / 31$) increase in the share of ‘yes’ votes compared to the Baseline treatment. As shown in column (2), most of the action seems to take place on the share rather than the number of ‘yes’ votes. The coefficient on the overall number of ‘yes’ votes is also positive, but not significant at the 10% level. Column (1) also shows that the Effectiveness treatment seems to cause an increase in ‘yes’ votes as well, but we are underpowered to detect its effect in a statistically significant way within the experimental sample. As in the case of the Earmarking treatment, the effect on the number of votes is also positive but not significant.

Table 3: Field experiment: average treatment effects within the experimental sample

Outcome	Share of ‘yes’ votes	Number of ‘yes’ votes
	(1)	(2)
Effectiveness	0.002 (0.004)	5.336 (7.656)
Earmarking	0.008* (0.005)	11.585 (7.611)
Controls	Yes	Yes
Adjusted R^2	0.875	0.738
Precincts	961	961

Notes: Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Precinct-level voting covariates include share of ‘yes’ votes on I-732, share of votes for Hillary Clinton in the 2016 Democratic primary, share of votes for the Republican candidate in the 2016 Presidential election, share of votes for the Democratic candidate in the 2018 midterm elections, and the number of registered voters. Coefficients on all control variables are displayed in Table B.11 in the Appendix.

This may be a lower bound of the true effect, since we follow an intent-to-treat approach. In our context, there are three reasons why the intent-to-treat effect may differ from the true average treatment effect. First, people can ignore our ads when they see them on Facebook. There is no way for us to know how many people decided to ignore our ads. We know that only a tiny fraction of people who were exposed to our ads clicked on the link to share their opinion, as it might have been expected. This fraction is very similar between the Effectiveness and the Earmarking treatments.

However, not clicking on the ads does not imply not having been exposed to the treatments. On this point, it is important to note that the possibility of people not noticing our ads does not make our estimates conservative for ad-based interventions, but may do so for interventions that can provide the same information in a more

salient way.

Second, it could be the case that some voters who see our ads may have already voted, since Washington state allows for early voting. In our analyses of individual-level survey data in Section 1, we use this as our “placebo” since we know if respondents have already voted. In the field experiment, estimated effects on actual voting behavior would occur only through those who are exposed to our ads before they vote.

Third, some voters in some precincts were not reached by our ads, either because of our budget constraint, or their absence from Facebook or its companion platform Instagram, or because of our experimental design, which aimed at limiting contamination from treated precincts to control precincts, thus not covering the entire area of treated precincts. On this point, as mentioned in Section 4.2, we can estimate the extent of coverage of our treatments since we know the share of voters plausibly exposed to our treatments, measured by what Facebook calls “reach”, i.e. the number of unique users who were exposed to the messages. We can use this share to investigate how much our main estimates in Table 3 could be diluted due to this reason.

Specifically, Facebook estimates the reach to be 285,281 (recall, however, that Facebook does not provide these data at the precinct level). We know that the number of registered voters in the precincts within our experiment was 572,242. This implies that, on average, 50% of registered voters in a precinct in the experimental sample are exposed to the treatments. We know that the share of ‘yes’ votes in precincts in our Baseline treatment, i.e. the proportion of people who would have

voted ‘yes’ in the absence of the Effectiveness or Earmarking treatments, is 0.31. This implies that the proportion of people who would have voted ‘no’ on the initiative in the absence of our information treatments is around 0.69. The number of valid votes on I-1631 as a percentage of the number of registered voters is 65% (see Table B.9). We can thus infer that for the representative voter within our experimental sample, being exposed to the Earmarking treatment leads to a 2.04 percentage point reduction in the likelihood of voting against the initiative.⁸

5.2.2 Effects over the entire state

As described in Section 4, we extend our analyses to the rest of the state. Recall that in this case two main differences apply. First, the comparison category is now precincts that are not selected into the experiment and therefore are exposed to no ads. Second, we need to address the fact that the precincts in the experimental sample have, by design, different socioeconomic characteristics compared to the rest of the state. Therefore, as described in Section 4, we condition on a large set of covariates, including those for which we observe imbalance between treated and control (rest of the state) precincts. Section B.5.4 shows corresponding estimates when conditioning on alternative sets of covariates.

We start commenting on our main results for the comparison between the in-

⁸Discounting the 0.8 percentage point increase caused by the Earmarking treatment by (i) the exposure rate (50%), (ii) the percentage of people who would have rejected the initiative in the absence of information, as indicated by the share of votes against the initiative in the Baseline treatment (69%), and (iii) the share of valid votes for the initiative (65%), implies a 2.04 percentage point change in favor of the initiative. For the Effectiveness treatment the same procedure implies a 0.60 percentage point change in voter preferences, noting however that the original coefficient of 0.2 percentage points is not statistically significant.

formational treatments and the rest of the state by examining the estimates from model 3, in which we condition on covariates. Table 4 presents the corresponding estimates. Consistently with our previous results, we focus mostly on the share of ‘yes’ votes, which is the outcome variable in column (1). As it may be expected, the Baseline treatment did not lead to any significant effect on voting behavior, although the positive coefficient may hint to the idea that pointing voters to this initiative and asking them for their opinion might have led them to acquire information on it, and potentially help with public support. If anything, any potential effect of our Baseline treatment implies another reason to consider the estimates in Table 3 as lower-bound estimates.

The Effectiveness treatment causes a 0.7 percentage point increase in the share of ‘yes’ votes and is statistically significant at the 10% level. This difference with Table 3 is driven by two factors. First, the point estimate is now larger, likely because we are comparing the effect against precincts that did not see any ads, and not against the neutral Baseline treatment. In Table 4, the difference with the Baseline treatment would not be statistically significant. Second, the number of observations is larger, leading to a decrease in the standard error.

Similar dynamics affect our estimate for the Earmarking treatment. In Table 4, the point estimate of 1.2 percentage points is larger and more precisely estimated than in Table 3, to the point that we can detect it at a significance level of less than 1%. Further, in Table 4 we also observe a statistically significant effect on the overall number of ‘yes’ votes, suggesting that the Earmarking treatment led to 13.48 additional votes per precinct in favor of I-1631.

From a 43% share of ‘yes’ votes in the precincts that did not see ads, the two treatments would cause a 1.6% ($0.7 \times 100 / 43$) and a 2.8% ($1.2 \times 100 / 43$) increase in the share of ‘yes’ votes.

Table 4: Field experiment: average treatment effects over the entire state

Outcome	Share of ‘yes’ votes	Number of ‘yes’ votes
	(1)	(2)
Baseline	0.004 (0.003)	-0.735 (5.044)
Effectiveness	0.007* (0.004)	4.732 (4.132)
Earmarking	0.012*** (0.004)	13.587*** (4.622)
Controls	Yes	Yes
Adjusted R^2	0.936	0.793
Precincts	7,083	7,083

Notes: Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Precinct-level voting covariates include share of ‘yes’ votes on I-732, share of votes for Hillary Clinton in the 2016 Democratic primary, share of votes for the Republican candidate in the 2016 Presidential election, share of votes for the Democratic candidate in the 2018 midterm elections, and the number of registered voters. Precinct-level demographic covariates include proportions of males, white people, commuters using public transport, commuters using cars, people with bachelors degrees, median income, and median age. Coefficients on all control variables are displayed in Table B.12 in the Appendix.

5.2.3 Robustness tests

We now briefly describe our findings from the robustness tests outlined in the previous section. Section B.5 in the Appendix contains results from all robustness specifications. First, the alternative non-linear estimates are similar in magnitude to our main estimates. The corresponding estimates are shown in Section B.5.1. Second, we instrument for precinct-level coverage of our treatment messages by treatment

assignment. In Section B.5.2, our LATE estimates confirm that the ITT effects that, for conservative reasons, we use throughout the main text are plausibly lower bounds of the average treatment effects. Third, since some of the buffers within which our ads are shown can potentially cross precinct boundaries, albeit to a limited extent, we also account for ‘extra exposure’ in Section B.5.3. On that front, we observe that accounting for extra exposure does not significantly affect our findings. Finally, we show that our coefficients are stable in magnitude and significance when including further control variables, as reported in Section B.5.4.

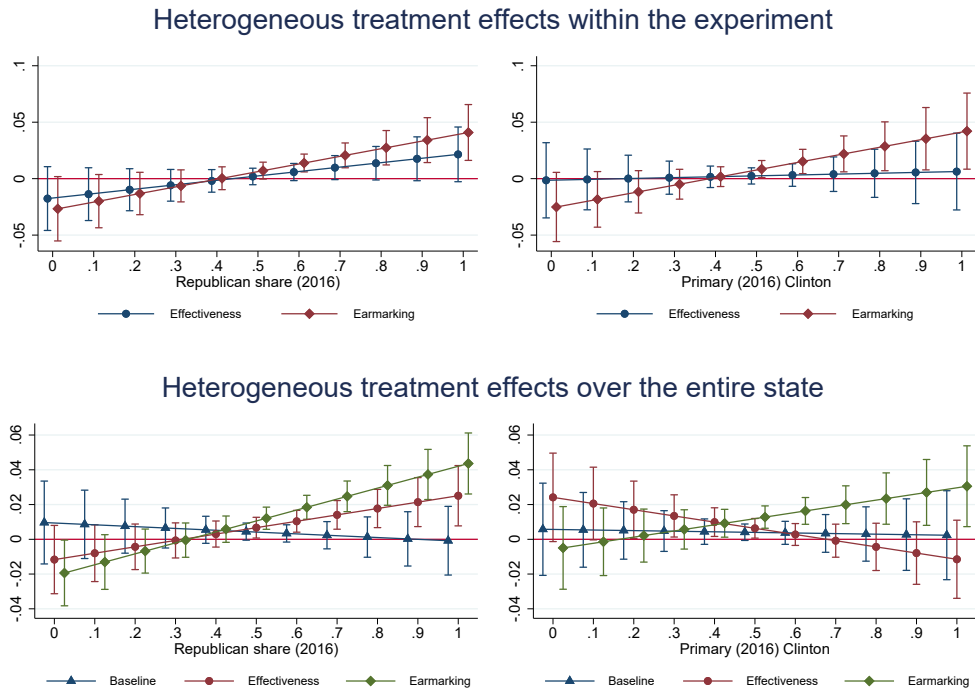
5.3 Heterogeneous treatment effects

In this context, as mentioned, one obvious dimension on which to analyze heterogeneous treatment effects is political partisanship. Recall that voting behavior for I-1631 relatively closely follows party lines and, among Democratic voters, I-1631 received higher support in precincts with a relatively higher share of supporters of Bernie Sanders than among precincts with a relatively higher share of supporters of Hillary Clinton, based on data on vote shares in the 2016 Democratic primary elections (see Table B.8 for the relevant correlations). The difference makes sense, since I-1631 relates closely to the idea of a Green New Deal, supported by the most progressive areas of the Democratic Party to which Sanders belongs, in that all revenues from the carbon tax would be earmarked for spending measures addressing environmental and social issues.

Hence, in this section we analyze heterogeneity along these two main dimensions: relative support to the Republican party versus the Democratic party, based on 2016

presidential elections, and relative support to Clinton versus Sanders, based on the 2016 Democratic primary. Figure 3 shows the marginal effects for the two dimensions of interest. Within the sample of precincts in the experiment, the Earmarking treatment has positive and significant marginal effects in precincts with more than 60% Republican vote share or with more than 50% vote share for Clinton in the 2016 Democratic primary. When considering results over the entire state, both the Effectiveness and Earmarking treatments are positive and significant in precincts with higher than 50% Republican vote share. Further, the marginal effects of the Earmarking treatment are positive and significant in precincts with higher than 40% vote share for Clinton in the 2016 primary elections. Tables B.13 and B.16 in the Appendix contain the estimates, with the coefficients of interest being those for the interaction terms between the treatment indicators and precinct-level election variables of interest. One could argue that an implication of our results is that the treatments “backfire” in precincts that have more Democratic voters or more voters who chose Bernie Sanders in the Democratic primary. However, it should be kept in mind, that our precinct selection strategy results in a sample of precincts which are more likely to vote Republican in the Presidential elections, and prefer Clinton over Sanders in the Democratic primary, compared to the average precinct in the state. Hence, we see this possible backfiring to have relatively small impact in our sample.

Figure 3: Field experiment: heterogeneous treatment effects



Notes:

The figures plot marginal effects for each treatment and 90% confidence intervals at all values of vote shares for the Republican candidate in the 2016 Presidential election and for Hillary Clinton in the 2016 Democratic primary elections as indicated in Tables B.13 and B.15 in the Appendix.

5.4 Drivers of heterogeneity

We see two potential explanations for these results, which we test separately in the following exploratory analyses. First, it may be that Republican voters, and Clinton voters among Democrats, are more likely to revise their beliefs following exposure to the treatments. To test the first potential explanation, we return to the survey data. Second, it may be that voters living in Republican-leaning precincts, and voters living in Clinton-leaning precincts, may be more likely to revise their beliefs following exposure to the informational treatments, even if they may not be particularly Republican-leaning or Clinton-leaning themselves. The latter effect could emerge if the location where one lives could affect the type of information that one consumes, either directly, or indirectly, through peers. To test the second hypothesis, we analyze media coverage of I-1631 and carbon taxes more in general, as well as campaign spending across areas of the state.

5.4.1 Belief revision and partisanship

We start with the first hypothesis, which, as mentioned, can be tested with survey data. Table A.7 in the Appendix shows heterogeneous treatment effects, using information on political preferences at the individual level. This is the dimension for which we observe the strongest heterogeneous treatment effects in Table B.13, in the context of our field experiment. Yet, when we turn to the survey data in Table A.7 and interact our treatment with an indicator for whether the survey respondent identifies as a Republican, we do not find any heterogeneous effect of our treatments on Republican voters, controlling for the lower average level of support for I-1631

among Republicans. We also do not observe meaningful differences in the data in how respondents affiliated to either party report that they do not know what their voting intentions are. Hence, we turn to the analysis of exposure to information.

5.4.2 Belief revision and access to information

We proceed with testing the second hypothesis in multiple ways. First, to analyze how exposure to various information about carbon taxes may play a role in driving our heterogeneous treatment effects, we turn to geographical differences in exposure to information and news about I-1631 and carbon taxes through television and print media. In this respect, we collect two sources of data. First, viewership shares of the major cable news organizations at the county level, accessed from SimmonsLocal, and readership shares of national and local newspapers at the county level, accessed from SimmonsLocal and the Alliance for Audited Media. Second, news segments and articles on the major cable news channels, national newspapers, and local newspapers that mention the term “carbon tax,” collected from archived text databases of Access World News, Factiva, and LexisNexis. The list of media outlets considered in this section is provided in Table C.25 in the Appendix, along with the respective sources for text corpora and summary statistics on viewership/readership shares.

Our first measure of exposure to information about carbon taxes consists of a simple count of news segments covering this topic, multiplied by the county-level share of viewership (for cable news) or readership (for newspapers). Table C.25 provides these counts for all media outlets. More formally, our first measure of

exposure, to which we refer simply as “news count,” looks as follows:

$$News\ count_j = \sum_{i=1}^N w_i \times X_{ij} \quad (4)$$

where w_i is the number of news segments that mention “carbon tax” in source i , and X_{ij} is the share of viewership/readership of source i in county j .

To test the effect of exposure to news covering carbon taxes on voting behavior, we interact our precinct-level treatment dummies with the county-level news count measure. Table 5 shows our results within the sample of precincts selected into the experiment. Column (1) shows results without controls, while column (2) includes results controlling for precinct-level political covariates. The result indicates that precincts that are less (more) exposed to news about carbon taxes respond more (less) to the Earmarking treatment. However, the analogous coefficient on the interaction term between Effectiveness and “news count” is not statistically significant. Extending our sample to the entire state, the coefficients on the interaction terms are larger and more precisely estimated in column (1) of Table C.26 in the Appendix. However, the effects are no longer statistically significant when precinct-level political and sociodemographic covariates are included.

Table 5: Field experiment: heterogeneous effects based on exposure measured as viewership / readership weighted news count within the experiment

Outcome	Share of ‘yes’ votes	
	(1)	(2)
Effectiveness \times News count	-0.003 (0.003)	0.001 (0.001)
Earmarking \times News count	-0.004 (0.003)	-0.003*** (0.001)
Effectiveness	0.048 (0.047)	-0.006 (0.020)
Earmarking	0.055 (0.041)	0.055*** (0.016)
News count	0.016*** (0.004)	0.003** (0.001)
Constant	0.111* (0.056)	-0.079 (0.065)
Controls	No	Yes
Precincts	961	961
Adjusted R^2	0.059	0.876

Notes: Standard errors clustered at county level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Precinct-level control variables include the share of ‘yes’ votes on I-732, share of votes for the Republican candidate in the 2016 Presidential election, share of votes for the Democratic candidate in the 2018 midterm elections, and the number of registered voters. Coefficients for all control variables are included in Table C.27 in the Appendix.

Tables C.28 and C.29 in the Appendix report analogous results for cable news and print newspapers, separately. It is worth noting that the results from cable news coverage are larger in magnitude and more consistent in direction, even when effects are not statistically significant.

Next, we acknowledge the limitation of simply counting the number of articles, which does not account for potential differences in the tone of coverage. For example,

even if there are a large number of news segments mentioning carbon taxes, it could be the case that all segments argue that carbon taxes are not an effective instrument. Therefore, we take additional steps to measure the slant of news coverage.

To this end, we take the textual corpora obtained from Factiva, NexisUni, and Access World News, where news segments on television or articles in print media mention the term “carbon tax.” We then proceed by having news segments evaluated based on their content by external freelancers recruited on Upwork, a popular clearing-house for freelance jobs. Freelancers are not aware of our research question and, hence, we do not expect them to be particularly biased in their coding of news segments from various organizations. We also de-identify the source of news segments to the extent possible.

We proceeded in two stages, in which each stage involved a new recruiting process on Upwork. In the first stage (stage 1), we asked two freelancers to independently go through each document that mentions the term “carbon tax,” identify, and extract segments of text that refer to the relationship between carbon taxes (or similar terms) and emissions. Implicitly, we asked freelancers to select segments that would refer to the effectiveness (or presumed lack thereof) of carbon taxes. All instructions to freelancers and questionnaires are included in Section F in the Appendix.

In the second stage (stage 2), we recruit four new freelancers to independently rate each segment selected in stage 1 on two criteria: (a) whether the selected segment is indeed relevant, in that it mentions the effect of carbon taxes on emissions (providing another round of checks on the selections made by the two freelancers in stage 1), and (b) whether the selected segment indicates that carbon taxes increase, do not affect,

or decrease emissions. The process of recruiting freelancers, selection and rating of segments was started in June 2020 and completed in January 2022.

5.4.3 Illustrative analysis of cable news slant

We now turn to the results of our analysis of media slant. We first consider whether the segments discuss the effectiveness of carbon taxes, and then how they do so. To evaluate whether a segment discusses the effectiveness of carbon taxes, we use a binary variable that takes value 1 if a segment selected by either of the two freelancers in stage 1 is evaluated to be relevant by a freelancer in stage 2, and 0 otherwise. For each segment, we add this binary variable for each of the four stage 2 freelancers, generating a measure that can take an integer value between 0 and 4 for each segment. We refer to this measure as “relevance score,” where a higher value indicates that the segment is more likely to contain a relevant discussion of the effectiveness (or lack thereof) of carbon taxes.

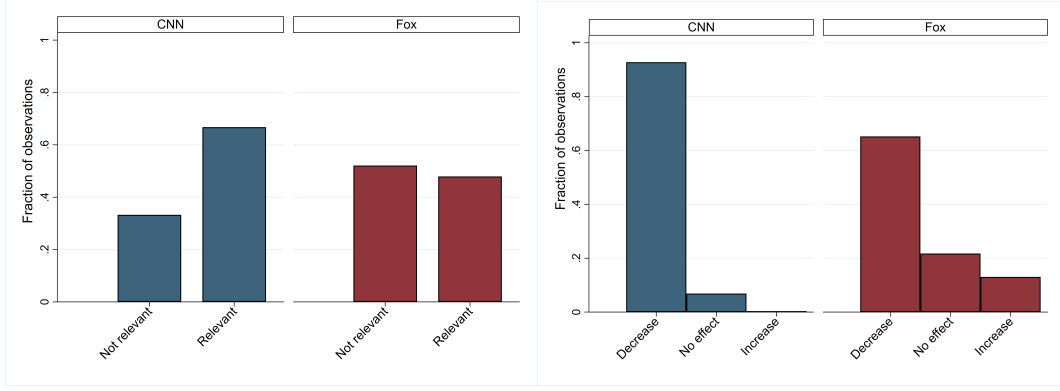
To evaluate how a segment argues for the effect of carbon taxes on emissions, we use a variable that takes value -1 if a freelancer in stage 2 rates the segment as arguing that carbon taxes reduce emissions, 0 if the freelancer rates the segment as arguing that carbon taxes do not affect emissions and 1 if the freelancer rates the segment as arguing that carbon taxes increase emissions. We sum this variable for each of the four stage 2 freelancers, resulting in a measure that can take integer values between -4 and 4 for each segment. We refer to this measure as the “direction score” for a segment, where a higher score indicates that the segment is less likely to argue that carbon taxes reduce emissions. We use these two measures as outcome

variables for our analysis.

As an illustration, we start by comparing the two measures for cable news segments, specifically those carried by CNN and Fox News. In the first pass (stage 1), freelancers selected 109 segments from CNN and 36 segments from Fox News as containing relevant discussions about how carbon taxes affect (or do not affect) emissions.

Examining how freelancers in stage 2 evaluated these segments, the first panel of Figure 4 shows that, on average, a segment on CNN that mentions “carbon tax” is more likely to be relevant than a similar segment carried by Fox News, i.e. it is more likely to mention whether carbon taxes affect emissions. The second panel shows that a majority of relevant news segments rated by freelancers in stage 2, for both CNN and Fox News, refer to carbon taxes as a policy instrument that reduces emissions, as one would expect. However, a higher fraction of segment-freelancer observations corresponding to Fox News, are evaluated as referring to carbon taxes as a policy instrument that is either unable to curb emissions or actually increases them. Some indicative examples of segments covering carbon taxes are provided in Section G in the Appendix.

Figure 4: Coverage of carbon taxes on CNN and Fox News



Notes: The panel on the left shows the fraction of segment-by-freelancer observations rated as “relevant” in terms of discussing the relationship between carbon taxes and emissions. The panel on the right shows the fraction of arguments made for each of the three directions (decrease, no change, or increase) of the effect of carbon taxes on emissions. The fraction is calculated as shares among segments that are rated to be relevant by at least one freelancer. Out of the 36 ‘relevant’ segments carried by Fox News, 12 (33%) are rated as arguing that carbon taxes do not decrease emissions by at least 1 freelancer. Compared to that, the same figure for CNN is 10 out of 109 segments (9.17%).

We find further strong evidence to support the difference in coverage of carbon taxes between CNN and Fox News in Table 6, where we estimate:

$$y_{ij} = \alpha + \beta * Fox\ News_i + \omega_j + \epsilon_{ij} \quad (5)$$

where the dependent variable y_{ij} is either (i) relevance score of segment i as rated by freelancer j , or (ii) direction score (carbon taxes decrease, do not affect or increase emissions) of segment i as rated by freelancer j , or (iii) a dummy variable that takes value 1 if segment i argues that carbon taxes do not decrease pollutant emissions as rated by freelancer j . $Fox\ News_i$ is a dummy variable that takes value 1 if segment i is from Fox News and 0 otherwise, ω_j denotes freelancer fixed effects, and

ϵ_{ij} is the error term. Columns (1)-(3) show estimates from linear probability model specifications for relevance score, direction score, and a dummy for news segments mentioning that carbon taxes either do not affect or increase emissions. Column (4) displays coefficients and column (5) displays the marginal effects from a probit model estimating the likelihood of a news segment mentioning that carbon taxes do not decrease emissions. For further context, 12 out of 36 segments (33%) carried by Fox News are rated as arguing that carbon taxes do not decrease emissions by at least 1 freelancer. Compared to that, the same figure for CNN is 10 out of 109 segments (9.17%).

Table 6: Slant of cable news coverage: Comparing coverage of carbon taxes in CNN and Fox News

Specification	Linear probability model			Probit (coefficients)	Probit (marginal effects)
Outcome variable	Relevance score	Direction score	Carbon taxes do not decrease emissions	Carbon taxes do not decrease emissions	Carbon taxes do not decrease emissions
	(1)	(2)	(3)	(4)	(5)
Fox News	-0.753*** (0.149)	1.435*** (0.615)	0.274*** (0.096)	1.067*** (0.310)	0.345*** (0.092)
Constant	2.670*** (0.125)	-3.389*** (0.140)	0.073*** (0.033)	-1.463*** (0.219)	
Freelancer IDs	Yes	Yes	Yes	Yes	Yes
Segments	145	90	90	90	90
Observations	580	360	360	360	360
Adjusted R^2	0.041	0.125	0.103	0.122	

Notes: Standard errors clustered at segment level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6 confirms that while segments on Fox News tend to provide less coverage on the relationship between carbon taxes and emissions, it is much more likely than CNN to suggest that carbon taxes are rather ineffective, including potentially increasing emissions. Specifically, a segment carried by Fox News is 75% less likely to be perceived by freelancers, acting as consumers, to be containing relevant information

about the effect of carbon taxes on emissions. A relevant segment carried by Fox News scores 1.435 higher in terms of the direction of implied effects (-1 being a negative effect, 0 being no effect, and 1 being a positive effect). Finally, a segment carried by Fox News is 34.5% more likely to argue that carbon taxes do not change or actually increase emissions compared to a segment carried by CNN.

5.4.4 Heterogeneous treatment effects based on media slant

Next, we turn to our full dataset on voting behavior as described in Section 4.1, and combine the results from the freelancers' evaluation of news segments with county-level viewership share of cable news and readership share of newspapers.

Similar to the procedure described in the illustrative example comparing the slant of CNN and Fox News coverage, we construct two variables that measure the degree of exposure as well as the nature of news coverage about carbon taxes for each precinct. We include a comprehensive description of the news organizations in our analysis, sources for text corpora and viewership or readership share, the number of articles mentioning 'carbon tax' for each news organization and the mean and standard deviation of each news organization's relevance score and direction score in Table C.25 in the Appendix. The important difference between this construction and the one in the illustrative example in Section 5.4.3 is that we weight the variables measuring tone of coverage by county-level readership and viewership shares for each media outlet.

The first variable in this analysis (CT Relevance) measures, as mentioned, relevance of news about carbon taxes as the sum of relevance scores of news organizations

weighted by their respective county-level viewership or readership shares. The second variable (CT Direction) measures the direction of the effect of carbon taxes on emissions as described by the media outlet in question, weighted by its respective county-level viewership or readership shares. We then estimate the following equation:

$$\begin{aligned}
y_{ij} = & \alpha + \beta_1 * Effectiveness_{ij} + \beta_2 * Earmarking_{ij} + \beta_3 * Baseline_{ij} + \gamma * News\ slant_j \\
& + \delta_1 * Effectiveness_{ij} * News\ slant_j + \delta_2 * Earmarking_{ij} * News\ slant_j \\
& + \delta_3 * Baseline_{ij} * News\ slant_j + \omega * X_{ij} + \epsilon_{ij}
\end{aligned}
\tag{6}$$

where $News\ slant_j$ is either the CT Relevance or the CT Direction measure for county j .

We first present results for precincts selected into the experiment in Table 7. Results for CT Relevance are included in odd-numbered columns, while those for CT Direction are in even-numbered columns. Columns (1) and (2) present estimates without controlling for precinct-level voting covariates, whereas columns (3) and (4) include these control variables.

The coefficients on interaction between our treatments and CT Relevance are negative while those on interaction between our treatments and CT Direction are positive, although the effect is statistically significant only for the Earmarking treatment. The direction of these coefficients supports the hypothesis that our treatments are likely to be providing information to people residing in locations where they may

otherwise be exposed to (i) less information about carbon taxes (negative interaction between CT Relevance and treatments), and (ii) more pessimistic information about the ability of carbon taxes to tackle greenhouse gas emissions (positive interaction between CT Direction and treatments).

Table 7: Field experiment: heterogeneous effects based on media slant within the experiment

Outcome	Share of ‘yes’ votes			
Margin of heterogeneity	CT Relevance	CT Direction	CT Relevance	CT Direction
	(1)	(2)	(3)	(4)
Effectiveness \times CT Relevance	-0.032 (0.050)		-0.017 (0.018)	
Earmarking \times CT Relevance	-0.019 (0.057)		-0.046** (0.019)	
Effectiveness \times CT Direction		0.031 (0.050)		0.013 (0.020)
Earmarking \times CT Direction		0.026 (0.062)		0.050** (0.020)
Effectiveness	0.040 (0.062)	0.033 (0.050)	0.023 (0.024)	0.015 (0.020)
Earmarking	0.027 (0.069)	0.030 (0.061)	0.063** (0.025)	0.057** (0.021)
CT Relevance	0.218** (0.081)		0.032* (0.017)	
CT Direction		-0.258*** (0.083)		-0.038** (0.019)
Constant	0.068 (0.096)	0.078 (0.080)	-0.073 (0.065)	-0.074 (0.065)
Controls	No	No	Yes	Yes
Precincts	961	961	961	961
Adjusted R^2	0.057	0.069	0.875	0.875

Notes: Standard errors clustered at county level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Precinct-level control variables include the share of ‘yes’ votes on I-732, share of votes for the Republican candidate in the 2016 Presidential election, share of votes for the Democratic candidate in the 2018 midterm elections, and the number of registered voters. Coefficients on all control variables are displayed in Table C.30 in the Appendix.

When we extend the analysis to the entire state in Table C.30 in the Appendix,

the direction of coefficients on the interaction terms remain the same, although when including the full suite of control variables estimates become noisier and we are underpowered to detect the remaining effect. Tables C.31 and C.32 in the Appendix provide results from estimating the same model separately for cable news and print newspapers. As is the case when we measure exposure as a simple count of news segments, the results tend to be more consistent for cable news segments than for articles in print newspapers.

In the preceding analyses, we have relied on freelancers’ evaluation of news coverage. We also use approaches relying on word-frequency and phrase-frequency models trained on text that has a known position on carbon taxes to estimate alternative measures of media slant. These approaches and our results are described in detail in Section C.1 in the Appendix. We also check whether advertising expenditure by the campaigns “for” and “against” I-1631 could potentially be a confounder to the estimates we find here. We do not find evidence for such an explanation as described in Section D in the Appendix.

Overall, our findings from this section indicate that the main driver of the heterogeneous effects observed in the previous section may relate to the type of information and narrative to which voters are exposed depending on where they live, which correlate with political preferences at the precinct level, rather than the political preferences of the voters themselves. This conclusion follows from the observation that media outlets whose consumption is dominant in Republican-leaning areas tend to cover carbon taxes less and, when they do, provide a less optimistic picture about their ability to reduce emissions. Thus, belief revision, as measured in the survey,

could be stronger in those areas, leading our informational treatments to be more effective at the margin.

6 Conclusions

Misperceptions and biased beliefs are a well-known issue that implies important deviations from the ideal of an informed citizenry that may be necessary for a well-functioning democracy, able to tackle the most important issues that present themselves in society. It is less known, however, how such biased beliefs may be addressed at scale. In this paper, we examine the effects of a large field experiment, whereby voters in Washington state were provided information through Facebook ads about carbon taxes, ahead of the vote on I-1631, an initiative aiming at implementing a carbon tax with revenues used mostly for environmental purposes. Facebook ads provided information directly to voters with precincts assigned to one control and two treatment arms. One treatment showed voters the potential of carbon taxes in reducing greenhouse gas emissions, leveraging the experience of the neighboring Canadian province of British Columbia. The other treatment provided information about the use of revenues for environmental purposes, responding to a demand for such use that may follow from a perception of ineffectiveness. We also implemented a survey experiment at the very same time on a representative sample of Washington state residents, using the same informational treatments.

Both treatments can increase support for carbon taxes, as observed in actual voting behavior in the context of the field experiment and as voting intentions elicited

in the survey. Specifically, in the field experiment, the effect of the Earmarking treatment is statistically significant, while both treatments are statistically significant in the survey. The survey also allows us to measure potential social desirability bias, by analyzing treatment exposure among the voters who declare to have already voted. We find limited evidence for social desirability bias. Overall, our findings do not seem to invalidate the use of survey experiments.

We also analyze heterogeneity along pre-registered dimensions, in particular in terms of political partisanship. Based on our survey data, where treatment is assigned at the individual level, we do not observe much evidence of such heterogeneity. Yet, we observe heterogeneity in the field experiment across Democrat- and Republican-leaning precincts. We reconcile these two findings by documenting important variation in how the media most consumed in respective precincts cover the issue of carbon taxes. It appears that response to our treatments is driven by the environment where people live and the information to which they are exposed, directly and indirectly, and not by individual political partisanship.

Our paper paves the way for future research analyzing more systematically sources of biased beliefs in particular related with media coverage, the role of narratives about Pigouvian policies and how relatively simple information treatments may affect biased beliefs through a more precise understanding of mental models, as well as how similar interventions may perform in different contexts.

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Appendix

A Survey experiment (additional tables)

In this section, we provide additional tables pertaining to the survey experiment, including summary statistics, balance of covariates and estimation results.

Table A.1: Survey: summary statistics of respondent characteristics and Washington state population

	Survey sample					Population
	Mean	Std. Dev.	Min.	Max.	N	Mean
Age	46.619	17.447	18	93	980	37.80
Female	0.535	0.499	0	1	980	0.50
Income Group 1 (At or less than \$13,500)	0.091	0.287	0	1	948	0.06
Income Group 2 (\$13,501-\$24,000)	0.095	0.293	0	1	948	0.04
Income Group 3 (\$24,001-\$34,500)	0.148	0.355	0	1	948	0.08
Income Group 4 (\$34,501-\$45,500)	0.097	0.296	0	1	948	0.08
Income Group 5 (\$45,501-\$59,000)	0.136	0.343	0	1	948	0.12
Income Group 6 (\$59,001-\$74,500)	0.118	0.323	0	1	948	0.18
Income Group 7 (\$74,501-\$94,000)	0.118	0.323	0	1	948	0.14
Income Group 8 (\$94,001-\$121,000)	0.097	0.296	0	1	948	0.16
Income Group 9 (\$121,001-\$170,500)	0.062	0.242	0	1	948	0.07
Income Group 10 (\$170,501 or more)	0.038	0.191	0	1	948	0.07
No education	0.007	0.085	0	1	971	0.001
Primary education	0.032	0.176	0	1	971	0.05
Secondary education	0.167	0.373	0	1	971	0.35
Higher education	0.794	0.405	0	1	971	0.60
Household size	2.665	1.458	1	9	980	2.57
Employed	0.607	0.490	0	1	980	0.59
Unemployed	0.055	0.229	0	1	980	0.04
Not in labor force (student, retired, other)	0.327	0.469	0	1	980	0.36
Democratic voter	0.514	0.500	0	1	895	0.54
Republican voter	0.364	0.481	0	1	895	0.38

Table A.2: Survey: balance of covariates

Variable	All respondents (1)		Baseline (2)		Effectiveness (3)		Earmarking (4)		(2) - (3)		Pairwise t-test (2) - (4)		(3) - (4)	
	N	Mean/(SE)	N	Mean/(SE)	N	Mean/(SE)	N	Mean/(SE)	N	Difference	N	Difference	N	Difference
Age	980	46.619 (0.557)	326	46.580 (0.947)	329	45.942 (1.005)	325	47.345 (0.943)	655	0.638	651	-0.765	654	-1.402
Female	980	0.535 (0.016)	326	0.549 (0.028)	329	0.538 (0.028)	325	0.517 (0.028)	655	0.011	651	0.032	654	0.021
Income Group 1 (At or less than \$13,500)	948	0.091 (0.009)	311	0.077 (0.015)	320	0.125 (0.019)	317	0.069 (0.014)	631	-0.048**	628	0.008	637	0.056**
Income Group 2 (\$13,501-\$24,000)	948	0.095 (0.010)	311	0.106 (0.017)	320	0.084 (0.016)	317	0.095 (0.016)	631	0.022	628	0.011	637	-0.010
Income Group 3 (\$24,001-\$34,500)	948	0.148 (0.012)	311	0.154 (0.021)	320	0.150 (0.020)	317	0.139 (0.019)	631	0.004	628	0.016	637	0.011
Income Group 4 (\$34,501-\$45,500)	948	0.097 (0.010)	311	0.109 (0.018)	320	0.109 (0.017)	317	0.073 (0.015)	631	-0.000	628	0.037	637	0.037
Income Group 5 (\$45,501-\$59,000)	948	0.136 (0.011)	311	0.132 (0.019)	320	0.106 (0.017)	317	0.170 (0.021)	631	0.026	628	-0.039	637	-0.064**
Income Group 6 (\$59,001-\$74,500)	948	0.118 (0.010)	311	0.100 (0.017)	320	0.113 (0.018)	317	0.142 (0.020)	631	-0.013	628	-0.042	637	-0.029
Income Group 7 (\$74,501-\$94,000)	948	0.118 (0.010)	311	0.116 (0.018)	320	0.116 (0.018)	317	0.123 (0.018)	631	0.000	628	-0.007	637	-0.007
Income Group 8 (\$94,001-\$121,000)	948	0.097 (0.010)	311	0.090 (0.016)	320	0.103 (0.017)	317	0.098 (0.017)	631	-0.013	628	-0.008	637	0.005
Income Group 9 (\$121,001-\$170,500)	948	0.062 (0.008)	311	0.058 (0.013)	320	0.059 (0.013)	317	0.069 (0.014)	631	-0.001	628	-0.012	637	-0.010
Income Group 10 (\$170,501 or more)	948	0.038 (0.006)	311	0.058 (0.013)	320	0.034 (0.010)	317	0.022 (0.008)	631	0.024	628	0.036**	637	0.012
No education	971	0.007 (0.003)	322	0.003 (0.003)	326	0.006 (0.004)	323	0.012 (0.006)	648	-0.003	645	-0.009	649	-0.006
Primary education	971	0.032 (0.006)	322	0.025 (0.009)	326	0.025 (0.009)	323	0.046 (0.012)	648	0.000	645	-0.022	649	-0.022
Secondary education	971	0.167 (0.012)	322	0.161 (0.021)	326	0.199 (0.022)	323	0.139 (0.019)	648	-0.038	645	0.022	649	0.060**
Higher education	971	0.794 (0.013)	322	0.811 (0.022)	326	0.770 (0.023)	323	0.802 (0.022)	648	0.041	645	0.009	649	-0.032
Employed	974	0.607 (0.016)	324	0.611 (0.027)	327	0.581 (0.027)	323	0.628 (0.027)	651	0.030	647	-0.017	650	-0.047
Democratic voter	895	0.514 (0.017)	294	0.503 (0.029)	304	0.539 (0.029)	297	0.498 (0.029)	598	-0.036	591	0.005	601	0.041
Republican voter	895	0.364 (0.016)	294	0.357 (0.028)	304	0.352 (0.027)	297	0.384 (0.028)	598	0.005	591	-0.027	601	-0.032

Table A.3: Survey: average treatment effects with control variables

Outcome Sample	Voted 'yes'		Will vote 'yes'			
	Already voted		No undecided voters		Undecided voters	
	(1)	(2)	(3)	(4)	(5)	(6)
Effectiveness	0.058 (0.076)	0.064 (0.069)	0.197*** (0.055)	0.144*** (0.053)	0.172*** (0.043)	0.129*** (0.045)
Earmarking	0.058 (0.076)	0.066 (0.070)	0.098* (0.056)	0.114** (0.053)	0.103** (0.043)	0.099** (0.045)
Age		-0.004** (0.002)		-0.006*** (0.002)		-0.004*** (0.001)
Female		-0.001 (0.063)		-0.046 (0.041)		-0.053 (0.038)
Household size		-0.027 (0.023)		0.004 (0.017)		-0.001 (0.015)
Income Group 1 (At or less than \$13,500)		0.298 (0.186)		0.119 (0.127)		0.027 (0.115)
Income Group 2 (\$13,501-\$24,000)		0.181 (0.188)		-0.094 (0.131)		-0.142 (0.112)
Income Group 3 (\$24,001-\$34,500)		0.107 (0.177)		0.016 (0.118)		-0.094 (0.106)
Income Group 4 (\$34,501-\$45,500)		-0.067 (0.186)		-0.083 (0.123)		-0.080 (0.112)
Income Group 5 (\$45,501-\$59,000)		0.034 (0.183)		-0.181 (0.122)		-0.156 (0.106)
Income Group 6 (\$59,001-\$74,500)		0.010 (0.181)		0.048 (0.117)		-0.087 (0.106)
Income Group 7 (\$74,501-\$94,000)		0.035 (0.190)		-0.074 (0.114)		-0.101 (0.104)
Income Group 8 (\$94,001-\$121,000)		0.026 (0.181)		-0.033 (0.119)		-0.032 (0.110)
Income Group 9 (\$121,001-\$170,500)		0.126 (0.203)		-0.120 (0.124)		-0.102 (0.113)
No education		-0.228 (0.238)		-0.398** (0.163)		-0.411*** (0.135)
Secondary education		-0.050 (0.218)		0.046 (0.133)		0.041 (0.133)
Higher education		-0.014 (0.209)		0.055 (0.124)		0.034 (0.126)
Employed		0.042 (0.067)		0.081 (0.050)		0.023 (0.043)
Democratic voter		0.440*** (0.113)		0.270*** (0.076)		0.248*** (0.059)
Republican voter		-0.065 (0.115)		-0.168** (0.078)		-0.081 (0.059)
Constant	0.459*** (0.054)	0.488 (0.315)	0.429*** (0.040)	0.594*** (0.212)	0.274*** (0.029)	0.454** (0.191)
N	263	242	494	442	717	613
Adjusted R^2	-0.005	0.277	0.022	0.278	0.019	0.141

Notes: Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Survey: probit estimation of average treatment effects

Outcome	Voted 'yes'		Will vote 'yes'			
Sample	Already voted		No undecided voters	With undecided voters		
	(1)	(2)	(3)	(4)	(5)	(6)
Effectiveness	0.058 (0.076)	0.069 (0.065)	0.195*** (0.053)	0.143*** (0.049)	0.172*** (0.042)	0.129*** (0.044)
Earmarking	0.058 (0.076)	0.061 (0.063)	0.096* (0.054)	0.116** (0.051)	0.106** (0.044)	0.102** (0.045)
Age		-0.004** (0.002)		-0.006*** (0.001)		-0.004*** (0.001)
Female		-0.005 (0.056)		-0.047 (0.040)		-0.054 (0.037)
Household size		-0.024 (0.021)		0.004 (0.016)		0.000 (0.014)
Income Group 1 (At or less than \$13,500)		0.299* (0.163)		0.120 (0.125)		0.022 (0.106)
Income Group 2 (\$13,501-\$24,000)		0.165 (0.153)		-0.082 (0.124)		-0.132 (0.105)
Income Group 3 (\$24,001-\$34,500)		0.102 (0.140)		0.013 (0.114)		-0.090 (0.098)
Income Group 4 (\$34,501-\$45,500)		-0.080 (0.146)		-0.089 (0.116)		-0.080 (0.103)
Income Group 5 (\$45,501-\$59,000)		0.026 (0.142)		-0.174 (0.114)		-0.156 (0.100)
Income Group 6 (\$59,001-\$74,500)		-0.001 (0.144)		0.042 (0.116)		-0.089 (0.097)
Income Group 7 (\$74,501-\$94,000)		0.025 (0.148)		-0.074 (0.109)		-0.103 (0.096)
Income Group 8 (\$94,001-\$121,000)		0.017 (0.141)		-0.034 (0.114)		-0.034 (0.101)
Income Group 9 (\$121,001-\$170,500)		0.116 (0.167)		-0.125 (0.117)		-0.108 (0.105)
Secondary education		-0.029 (0.187)		0.059 (0.120)		0.040 (0.124)
Higher education		0.008 (0.179)		0.066 (0.110)		0.033 (0.116)
Employed		0.027 (0.062)		0.084* (0.049)		0.027 (0.043)
Democratic voter		0.366*** (0.082)		0.237*** (0.063)		0.229*** (0.056)
Republican voter		-0.059 (0.092)		-0.150** (0.067)		-0.095 (0.062)
N	263	241	494	441	717	609

Notes: Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Coefficients are marginal effects of the treatments.

Table A.5: Survey: beliefs about impacts of I-1631

Survey question	How would you rate I-1631?			
Sub-category	Reducing emissions		Reducing local pollution	
Likert rating (outcome)	Poor	Good	Poor	Good
Effectiveness	-0.080*** (0.030)	0.079** (0.039)	-0.086*** (0.033)	0.071* (0.039)
Earmarking	-0.047 (0.032)	0.053 (0.039)	-0.048 (0.035)	0.046 (0.039)
Age	0.002*** (0.001)	-0.003** (0.001)	0.002** (0.001)	-0.005*** (0.001)
Female	-0.074*** (0.025)	0.012 (0.033)	-0.079*** (0.027)	-0.064* (0.033)
Household size	0.005 (0.010)	-0.016 (0.012)	0.016 (0.010)	0.012 (0.012)
Income Group 1 (At or less than \$13,500)	0.081 (0.071)	0.082 (0.102)	-0.004 (0.081)	0.008 (0.103)
Income Group 2 (\$13,501-\$24,000)	0.014 (0.065)	0.089 (0.101)	0.015 (0.082)	0.052 (0.101)
Income Group 3 (\$24,001-\$34,500)	0.086 (0.065)	0.067 (0.094)	0.007 (0.075)	0.039 (0.093)
Income Group 4 (\$34,501-\$45,500)	0.056 (0.066)	0.009 (0.098)	0.000 (0.079)	-0.010 (0.098)
Income Group 5 (\$45,501-\$59,000)	0.031 (0.066)	0.021 (0.096)	-0.031 (0.077)	-0.015 (0.094)
Income Group 6 (\$59,001-\$74,500)	0.055 (0.063)	0.037 (0.094)	0.004 (0.074)	-0.009 (0.094)
Income Group 7 (\$74,501-\$94,000)	0.107 (0.066)	0.071 (0.094)	0.053 (0.077)	0.006 (0.093)
Income Group 8 (\$94,001-\$121,000)	-0.036 (0.063)	0.056 (0.095)	-0.010 (0.079)	-0.028 (0.095)
Income Group 9 (\$121,001-\$170,500)	0.104 (0.073)	-0.065 (0.100)	0.042 (0.085)	-0.064 (0.101)
No education	0.077 (0.151)	-0.319** (0.130)	0.186 (0.234)	-0.264 (0.217)
Secondary education	0.044 (0.069)	0.103 (0.112)	-0.039 (0.099)	0.039 (0.110)
Higher education	0.052 (0.064)	0.150 (0.104)	-0.042 (0.093)	0.045 (0.103)
Employed	-0.048 (0.030)	0.051 (0.037)	-0.032 (0.033)	-0.010 (0.039)
Democratic voter	-0.128*** (0.040)	0.276*** (0.052)	-0.096** (0.042)	0.218*** (0.054)
Republican voter	0.125*** (0.047)	-0.072 (0.052)	0.147*** (0.048)	-0.101* (0.055)
Constant	0.091 (0.103)	0.236 (0.160)	0.206 (0.136)	0.556*** (0.162)
<i>N</i>	855	855	855	855
Adjusted <i>R</i> ²	0.138	0.138	0.101	0.125

Notes: Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Survey: probit estimates on beliefs about impacts of I-1631

Survey question	How would you rate I-1631?			
Sub-category	Reducing emissions		Reducing local pollution	
Likert rating (outcome)	Poor	Good	Poor	Good
Effectiveness	-0.076** (0.030)	0.077** (0.038)	-0.088*** (0.032)	0.071* (0.039)
Earmarking	-0.042 (0.029)	0.054 (0.039)	-0.045 (0.032)	0.050 (0.039)
Age	0.002** (0.001)	-0.003*** (0.001)	0.002** (0.001)	-0.005*** (0.001)
Female	-0.064*** (0.024)	0.014 (0.032)	-0.073*** (0.026)	-0.063* (0.032)
Household size	0.002 (0.009)	-0.016 (0.012)	0.015 (0.010)	0.012 (0.012)
Income Group 1 (At or less than \$13,500)	0.066 (0.086)	0.076 (0.096)	-0.021 (0.085)	0.005 (0.098)
Income Group 2 (\$13,501-\$24,000)	0.001 (0.088)	0.085 (0.093)	0.014 (0.084)	0.049 (0.095)
Income Group 3 (\$24,001-\$34,500)	0.070 (0.082)	0.066 (0.088)	-0.011 (0.079)	0.036 (0.088)
Income Group 4 (\$34,501-\$45,500)	0.044 (0.086)	0.002 (0.092)	-0.013 (0.083)	-0.014 (0.093)
Income Group 5 (\$45,501-\$59,000)	0.025 (0.084)	0.018 (0.090)	-0.041 (0.080)	-0.016 (0.089)
Income Group 6 (\$59,001-\$74,500)	0.035 (0.083)	0.029 (0.089)	-0.022 (0.079)	-0.012 (0.089)
Income Group 7 (\$74,501-\$94,000)	0.089 (0.082)	0.067 (0.088)	0.033 (0.078)	0.005 (0.089)
Income Group 8 (\$94,001-\$121,000)	-0.058 (0.088)	0.050 (0.089)	-0.022 (0.082)	-0.034 (0.091)
Income Group 9 (\$121,001-\$170,500)	0.080 (0.086)	-0.081 (0.097)	0.024 (0.085)	-0.071 (0.099)
No education	0.055 (0.148)	0.000 (.)	0.147 (0.175)	-0.282 (0.257)
Secondary education	0.036 (0.091)	0.099 (0.109)	-0.050 (0.089)	0.039 (0.106)
Higher education	0.040 (0.089)	0.142 (0.101)	-0.060 (0.084)	0.044 (0.099)
Employed	-0.042 (0.028)	0.053 (0.037)	-0.029 (0.031)	-0.008 (0.038)
Democratic voter	-0.142*** (0.037)	0.254*** (0.047)	-0.106** (0.042)	0.202*** (0.048)
Republican voter	0.088** (0.036)	-0.076 (0.052)	0.116*** (0.041)	-0.104** (0.052)
N	855	850	855	855

Notes: Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Coefficients reported are marginal effects.

Table A.7: Survey: heterogeneous treatment effects based on respondent party affiliation

Outcome	Voted 'yes'		Will vote 'yes'
Sample	Already voted	No undecided voters	With undecided voters
Effectiveness	0.044 (0.093)	0.145** (0.068)	0.143** (0.059)
Effectiveness \times Republican voter	0.046 (0.139)	0.003 (0.107)	-0.040 (0.088)
Earmarking	0.066 (0.096)	0.142** (0.071)	0.128** (0.062)
Earmarking \times Republican voter	-0.003 (0.141)	-0.073 (0.108)	-0.079 (0.086)
Republican voter	-0.075 (0.133)	-0.141 (0.103)	-0.041 (0.075)
Democratic voter	0.446*** (0.116)	0.271*** (0.076)	0.248*** (0.059)
Age	-0.004** (0.002)	-0.006*** (0.002)	-0.004*** (0.001)
Female	-0.001 (0.063)	-0.046 (0.042)	-0.054 (0.038)
Household size	-0.028 (0.023)	0.003 (0.017)	-0.001 (0.015)
Income Group 1 (At or less than \$13,500)	0.304 (0.190)	0.105 (0.128)	0.019 (0.116)
Income Group 2 (\$13,501-\$24,000)	0.182 (0.191)	-0.110 (0.133)	-0.151 (0.113)
Income Group 3 (\$24,001-\$34,500)	0.110 (0.182)	0.005 (0.118)	-0.102 (0.107)
Income Group 4 (\$34,501-\$45,500)	-0.067 (0.188)	-0.098 (0.124)	-0.089 (0.113)
Income Group 5 (\$45,501-\$59,000)	0.032 (0.185)	-0.188 (0.122)	-0.160 (0.107)
Income Group 6 (\$59,001-\$74,500)	0.016 (0.183)	0.034 (0.119)	-0.096 (0.107)
Income Group 7 (\$74,501-\$94,000)	0.039 (0.193)	-0.085 (0.114)	-0.110 (0.105)
Income Group 8 (\$94,001-\$121,000)	0.030 (0.183)	-0.047 (0.120)	-0.042 (0.110)
Income Group 9 (\$121,001-\$170,500)	0.130 (0.207)	-0.134 (0.125)	-0.110 (0.114)
No education	-0.242 (0.241)	-0.425** (0.168)	-0.417*** (0.136)
Secondary education	-0.048 (0.221)	0.042 (0.135)	0.038 (0.134)
Higher education	-0.012 (0.212)	0.053 (0.126)	0.034 (0.127)
Employed	0.042 (0.067)	0.078 (0.050)	0.022 (0.043)
Constant	0.488 (0.319)	0.601*** (0.215)	0.450** (0.193)
N	242	442	613
Adjusted R^2	0.271	0.276	0.139

Notes: Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Field experiment

B.1 Selection of precincts to the experimental sample

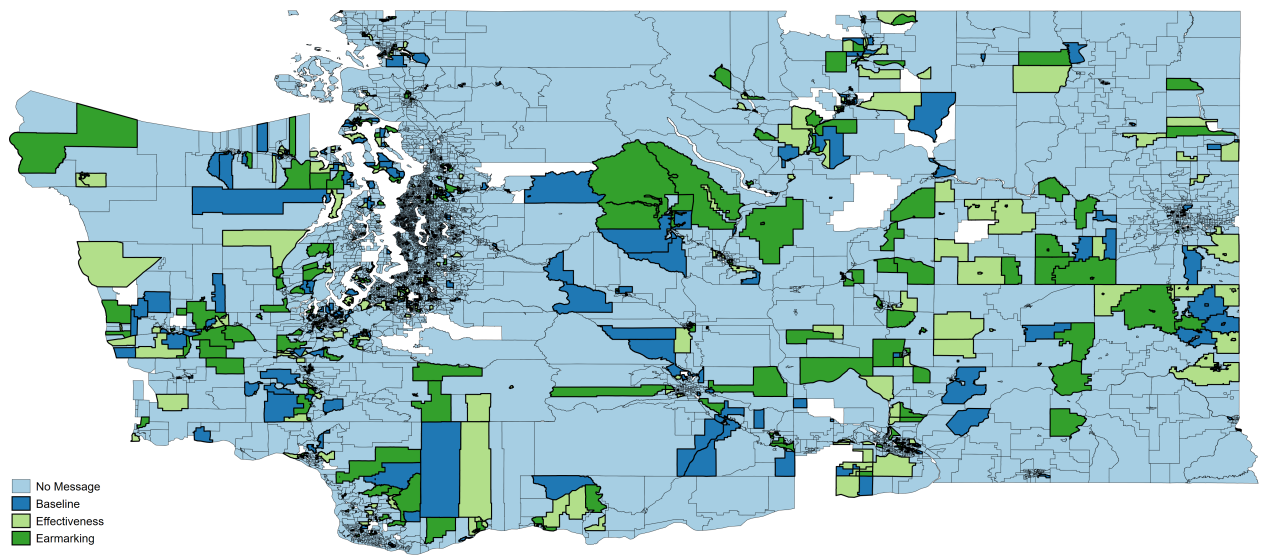
The selection of precincts into the experiment operated as follows. The state of Washington’s voter registry data was used to map addresses of registered voters to the geographic boundaries of voting precincts.⁹ Precincts were selected into the experimental sample in two waves with gradual relaxation of the selection criteria between the first and the second wave. The selection criteria are defined in terms of a minimum area of the treated precinct that at least needs to be covered to limit undertreatment as well as a maximum number of precincts that can be contaminated, with maximum limits to the area exposed to potential contamination within each contaminated precinct (at most, 10%).

1. In wave 1, the pin was dropped at the centroid of each precinct with buffers of radius 1 mile, and the following conditions applied.
 - (a) Minimizing undertreatment: The proportion of voter addresses within the precinct area but not covered by the buffer was capped at 30%.
 - (b) Minimizing contamination:
 - i. The number of contaminated precincts, i.e. other precincts whose area intersected the buffer, was capped at 6.
 - ii. the proportion of the contaminated area of the most contaminated precinct, among the 6 (at most), was capped at 5%.

⁹The data is available upon request to the Washington Secretary of State office at <http://www.sos.wa.gov/elections/vrdb/extract-requests.aspx> (last accessed, July 22, 2020).

2. In wave two, the pin was placed at an optimized coordinate and a radius of 1, 2, or 3 miles was chosen to reduce undertreatment while meeting the contamination criteria. The optimization was done by mapping voter addresses to latitude-longitude coordinates in Washington state and calculating the number of voters covered by the buffer around the optimized coordinate.
 - (a) Minimizing undertreatment: The proportion of voter addresses within the precinct but not covered by the circle centered at the optimized GPS coordinate was capped at 50%.
 - (b) Minimizing contamination:
 - i. the number of other precincts whose area intersected the optimized buffer was capped at 10.
 - ii. the proportion of the contaminated area of the most contaminated precinct, among the 10 (at most), was capped at 10%.

Figure B.1: Treated and control precincts in Washington state



Note: Colors indicate assignment of voting precincts in Washington state to each of the three experimental groups - “Baseline”, “Effectiveness” and “Earmarking” as well as precincts that were not exposed to any advertisements.

B.2 Descriptive statistics (additional tables and figures)

This section contains descriptive statistics pertaining to the field experiment, including correlates of vote shares for I-1631, precinct-level voting, and summary statistics for sociodemographic variables by treatment status, and balance of covariates between precincts within the experiment and for the rest of the state.

Table B.8: Associations between I-1631 outcomes and precinct-level characteristics among untreated precincts

	(1) Share of 'yes' votes	(2) Number of 'yes' votes	(3) Change from I-732	(4) Valid votes for I-1631
'Yes' share for I-732	0.439*** (0.025)	84.687*** (31.204)	-0.507*** (0.029)	-0.428*** (0.051)
Primary 2016 (Clinton)	-0.066*** (0.009)	-40.431*** (10.945)	-0.062*** (0.011)	0.129*** (0.015)
Democrat share (Senate 2018)	0.364*** (0.046)	82.235 (53.359)	0.393*** (0.048)	0.001 (0.078)
Republican share (2016)	-0.174*** (0.052)	-275.639*** (58.122)	-0.123** (0.053)	-0.172* (0.102)
Registered voters	0.000*** (0.000)	0.249*** (0.004)	0.000*** (0.000)	-0.000*** (0.000)
Male (percent)	-0.000** (0.000)	-0.317 (0.392)	-0.000 (0.000)	-0.001** (0.000)
White (percent)	0.001*** (0.000)	1.054*** (0.087)	0.001*** (0.000)	0.002*** (0.000)
Commuting by car (percent)	-0.001*** (0.000)	-0.356** (0.177)	-0.001*** (0.000)	0.000 (0.000)
Commuting by public transport (percent)	0.000 (0.000)	-0.985*** (0.314)	0.001** (0.000)	0.003*** (0.000)
Bachelors degree (percent)	0.002*** (0.000)	2.246*** (0.113)	0.002*** (0.000)	0.003*** (0.000)
Median income	-0.000*** (0.000)	-0.001*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)
Median age	0.001*** (0.000)	2.097*** (0.213)	0.001*** (0.000)	0.004*** (0.000)
Constant	0.096* (0.056)	-84.390 (65.714)	0.033 (0.063)	0.499*** (0.116)
Precincts	6122	6122	6122	6122
Adjusted R^2	0.938	0.786	0.435	0.523

Notes: Heteroscedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.9: Field experiment: precinct-level summary statistics by treatment group and for the entire state

	(1)	(2)	(3)	(4)	(5)
	All Precincts	No message	Baseline	Effectiveness	Earmarking
Share of ‘yes’ votes	0.426 (0.189)	0.446 (0.186)	0.308 (0.152)	0.284 (0.151)	0.298 (0.148)
Number of ‘yes’ votes	185.704 (121.960)	194.414 (119.075)	134.059 (120.340)	128.301 (127.925)	128.059 (128.417)
Change from I-732	0.032 (0.094)	0.036 (0.094)	0.005 (0.089)	0.000 (0.078)	0.010 (0.095)
Valid votes for I-1631	0.654 (0.134)	0.655 (0.129)	0.647 (0.173)	0.645 (0.174)	0.654 (0.138)
‘Yes’ share for I-732	0.402 (0.142)	0.419 (0.138)	0.305 (0.118)	0.289 (0.120)	0.292 (0.121)
Primary (2016) Clinton	0.504 (0.144)	0.515 (0.136)	0.440 (0.163)	0.432 (0.182)	0.438 (0.177)
Democrat share (Senate 2018)	0.577 (0.193)	0.601 (0.186)	0.435 (0.163)	0.414 (0.167)	0.421 (0.167)
Republican share (2016)	0.380 (0.198)	0.357 (0.191)	0.514 (0.176)	0.538 (0.182)	0.532 (0.182)
Registered voters	660.370 (371.777)	671.661 (360.637)	602.709 (421.039)	591.730 (442.001)	570.746 (425.906)
Male (percent)	49.786 (2.761)	49.680 (2.728)	50.283 (2.970)	50.687 (2.925)	50.415 (2.719)
White (percent)	78.802 (14.382)	77.632 (14.319)	86.872 (11.613)	86.015 (13.023)	85.907 (12.576)
Commuting by car (percent)	72.291 (11.544)	71.669 (11.993)	76.473 (6.771)	76.244 (6.852)	76.069 (7.084)
Commuting by public transport (percent)	6.033 (7.058)	6.714 (7.247)	1.860 (3.289)	1.580 (3.215)	1.619 (3.361)
Bachelor degree (percent)	35.901 (19.355)	37.619 (19.707)	25.618 (12.206)	24.328 (12.222)	24.888 (12.084)
Median income	43376.064 (15279.730)	44394.420 (15759.829)	36789.935 (9515.086)	36640.204 (9776.191)	37204.713 (9134.108)
Median age	39.991 (6.642)	39.349 (6.218)	44.573 (7.551)	44.070 (7.851)	43.605 (7.742)
Observations	7099	6137	320	319	323

Notes: Standard deviations in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.10: Field experiment: balance of covariates

Variable	(1)		(2)		(3)		(4)		(5)		(2)-(3)		(2)-(4)		(2)-(5)		(3)-(4)		(3)-(5)		(4)-(5)	
	N	Mean (SE)	No message	Baseline	N	Mean (SE)	Effectiveness	N	Mean (SE)	Emulating	N	Difference	N	Difference	N	Difference	N	Difference	N	Difference	N	Difference
Yes' share for L-732	7099	0.402 (0.002)	6137 (0.002)	6137 (0.002)	320	0.305 (0.040)	0.289 (0.007)	319	0.319 (0.007)	323	0.292 (0.007)	6457 (0.007)	6456 (0.007)	6460 (0.007)	6457 (0.007)	6456 (0.007)	6460 (0.007)	6457 (0.007)	6456 (0.007)	6460 (0.007)	6457 (0.007)	
Primary (2016) Clinton	7099	0.504 (0.002)	6137 (0.002)	6137 (0.002)	320	0.440 (0.009)	0.432 (0.010)	319	0.432 (0.010)	323	0.438 (0.010)	6457 (0.010)	6456 (0.010)	6460 (0.010)	6457 (0.010)	6456 (0.010)	6460 (0.010)	6457 (0.010)	6456 (0.010)	6460 (0.010)	6457 (0.010)	
Democrat share (Senate 2018)	7099	0.377 (0.002)	6137 (0.002)	6137 (0.002)	320	0.435 (0.009)	0.414 (0.010)	319	0.414 (0.010)	323	0.421 (0.010)	6457 (0.010)	6456 (0.010)	6460 (0.010)	6457 (0.010)	6456 (0.010)	6460 (0.010)	6457 (0.010)	6456 (0.010)	6460 (0.010)	6457 (0.010)	
Republican share (2016)	7099	0.380 (0.002)	6137 (0.002)	6137 (0.002)	320	0.514 (0.010)	0.538 (0.010)	319	0.538 (0.010)	323	0.532 (0.010)	6457 (0.010)	6456 (0.010)	6460 (0.010)	6457 (0.010)	6456 (0.010)	6460 (0.010)	6457 (0.010)	6456 (0.010)	6460 (0.010)	6457 (0.010)	
Registered voters	7099	660.370 (4.412)	6137 (4.604)	6137 (4.604)	320	602.709 (23.537)	591.720 (24.747)	319	591.720 (24.747)	323	570.746 (23.698)	6457 (23.698)	6456 (23.698)	6460 (23.698)	6457 (23.698)	6456 (23.698)	6460 (23.698)	6457 (23.698)	6456 (23.698)	6460 (23.698)	6457 (23.698)	
Male (percent)	7099	49.786 (0.033)	6137 (0.033)	6137 (0.033)	320	50.283 (0.166)	50.687 (0.164)	319	50.687 (0.164)	323	50.415 (0.151)	6457 (0.151)	6456 (0.151)	6460 (0.151)	6457 (0.151)	6456 (0.151)	6460 (0.151)	6457 (0.151)	6456 (0.151)	6460 (0.151)	6457 (0.151)	
White (percent)	7099	78.802 (0.171)	6137 (0.171)	6137 (0.171)	320	80.872 (0.706)	86.015 (0.706)	319	86.015 (0.706)	323	85.907 (0.706)	6457 (0.706)	6456 (0.706)	6460 (0.706)	6457 (0.706)	6456 (0.706)	6460 (0.706)	6457 (0.706)	6456 (0.706)	6460 (0.706)	6457 (0.706)	
Committing by car (percent)	7099	72.290 (0.137)	6137 (0.137)	6137 (0.137)	320	70.723 (0.370)	70.723 (0.370)	319	70.723 (0.370)	323	70.900 (0.394)	6457 (0.394)	6456 (0.394)	6460 (0.394)	6457 (0.394)	6456 (0.394)	6460 (0.394)	6457 (0.394)	6456 (0.394)	6460 (0.394)	6457 (0.394)	
Committing by public transport (percent)	7099	6.033 (0.084)	6137 (0.084)	6137 (0.084)	320	1.860 (0.184)	1.860 (0.184)	319	1.860 (0.184)	323	1.619 (0.187)	6457 (0.187)	6456 (0.187)	6460 (0.187)	6457 (0.187)	6456 (0.187)	6460 (0.187)	6457 (0.187)	6456 (0.187)	6460 (0.187)	6457 (0.187)	
Bachelor degree (percent)	7099	35.901 (0.230)	6137 (0.230)	6137 (0.230)	320	37.618 (0.682)	39.335 (0.682)	319	39.335 (0.682)	323	38.888 (0.672)	6457 (0.672)	6456 (0.672)	6460 (0.672)	6457 (0.672)	6456 (0.672)	6460 (0.672)	6457 (0.672)	6456 (0.672)	6460 (0.672)	6457 (0.672)	
Median income	7099	43376.064 (181.350)	6137 (181.350)	6137 (181.350)	320	36789.935 (201.175)	36789.935 (201.175)	319	36640.204 (197.362)	323	37294.713 (198.255)	6457 (198.255)	6456 (198.255)	6460 (198.255)	6457 (198.255)	6456 (198.255)	6460 (198.255)	6457 (198.255)	6456 (198.255)	6460 (198.255)	6457 (198.255)	
Median age	7099	39.991 (0.079)	6137 (0.079)	6137 (0.079)	320	39.349 (0.472)	39.349 (0.472)	319	39.349 (0.472)	323	39.349 (0.431)	6457 (0.431)	6456 (0.431)	6460 (0.431)	6457 (0.431)	6456 (0.431)	6460 (0.431)	6457 (0.431)	6456 (0.431)	6460 (0.431)	6457 (0.431)	

Notes: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.3 Power calculations

As mentioned in Section 3.3, we selected a total sample of 980 precincts into the experiment. Here, we briefly present our power calculations, based on ex-ante data. In particular, we rely on administrative data and existing analyses on I-732 to make assumptions about standard deviation and the share of variance in votes on I-1631 that could be explained by covariates other than our treatment. Specifically, we use the standard deviation of 0.12 in the share of ‘yes’ votes on I-732 over the precincts selected into the experiment. We also know that 88% of the variance in our outcome variable can be explained by political covariates that are not influenced by our treatment. Based on these parameters, at 80% statistical power and 10% level of significance, our experiment has a minimum detectable effect size of 0.007.

B.4 Treatment effects (additional tables and figures)

This section provides additional tables and figures pertaining to estimated results for our field experiment. We present average and heterogeneous treatment effects with control variables, within the experiment as well as for the entire state.

Table B.11: Field experiment: average treatment effects within experiment with control variables

Outcome	Share of 'yes' votes	Number of 'yes' votes	Change from I-732	Valid votes for I-1631
Effectiveness	0.002 (0.004)	5.336 (7.656)	0.005 (0.005)	-0.008 (0.011)
Earmarking	0.008* (0.005)	11.585 (7.611)	0.012* (0.006)	-0.003 (0.010)
'Yes' share for I-732	0.428*** (0.055)	60.587 (117.504)	-0.588*** (0.055)	-0.526*** (0.200)
Primary (2016) Clinton	-0.071*** (0.016)	-4.335 (32.014)	-0.067*** (0.018)	0.134*** (0.050)
Democrat share (Senate 2018)	0.571*** (0.050)	180.246** (70.968)	0.609*** (0.057)	0.175 (0.151)
Republican share (2016)	-0.041 (0.046)	-284.514*** (86.253)	-0.023 (0.053)	-0.182 (0.175)
Registered voters	0.000*** (0.000)	0.207*** (0.011)	0.000*** (0.000)	-0.000* (0.000)
Constant	-0.032 (0.047)	55.876 (85.872)	-0.062 (0.055)	0.807*** (0.175)
Precincts	961	961	961	961
Adjusted R^2	0.875	0.738	0.392	0.067

Notes: Heteroscedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.12: Field experiment: average treatment effects over entire state with control variables

Outcome	Share of 'yes' votes	Number of 'yes' votes	Change from I-732	Valid votes for I-1631
Baseline	0.004 (0.003)	-0.735 (5.044)	0.003 (0.004)	0.008 (0.006)
Effectiveness	0.007* (0.004)	4.732 (4.132)	0.009** (0.004)	0.002 (0.005)
Earmarking	0.012*** (0.004)	13.587*** (4.622)	0.015*** (0.005)	0.009 (0.005)
'Yes' share for I-732	0.438*** (0.023)	74.646** (30.239)	-0.518*** (0.027)	-0.427*** (0.046)
Primary (2016) Clinton	-0.066*** (0.008)	-40.909*** (9.865)	-0.062*** (0.010)	0.122*** (0.014)
Democrat share (Senate 2018)	0.381*** (0.043)	82.169* (49.412)	0.411*** (0.045)	0.009 (0.072)
Republican share (2016)	-0.153*** (0.047)	-273.259*** (52.817)	-0.108** (0.048)	-0.151* (0.091)
Registered voters	0.000*** (0.000)	0.243*** (0.003)	0.000*** (0.000)	-0.000*** (0.000)
Male (percent)	-0.000 (0.000)	-0.394 (0.363)	-0.000 (0.000)	-0.001*** (0.000)
White (percent)	0.001*** (0.000)	0.982*** (0.082)	0.001*** (0.000)	0.002*** (0.000)
Commuting by car (percent)	-0.001*** (0.000)	-0.490*** (0.165)	-0.001*** (0.000)	0.000 (0.000)
Commute by public transport (percent)	0.000 (0.000)	-1.041*** (0.302)	0.001** (0.000)	0.003*** (0.000)
Bachelor degree (percent)	0.002*** (0.000)	2.272*** (0.106)	0.002*** (0.000)	0.003*** (0.000)
Median income	-0.000*** (0.000)	-0.001*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)
Median age	0.001*** (0.000)	2.316*** (0.195)	0.000*** (0.000)	0.004*** (0.000)
Constant	0.076 (0.049)	-63.489 (59.617)	0.023 (0.056)	0.482*** (0.103)
Precincts	7,083	7,083	7,083	7,083
Adjusted R^2	0.936	0.793	0.437	0.524

Notes: Heteroscedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.13: Field experiment: heterogeneous treatment effects within the experiment

Outcome	Share of ‘yes’ votes	
	Republican share (2016) (1)	Primary (2016) Clinton vote share (2)
Effectiveness \times Republican share (2016)	0.039 (0.031)	
Earmarking \times Republican share (2016)	0.068** (0.031)	
Effectiveness \times Primary (2016) Clinton		0.008 (0.040)
Earmarking \times Primary (2016) Clinton		0.067* (0.038)
Effectiveness	-0.018 (0.017)	-0.001 (0.020)
Earmarking	-0.027 (0.017)	-0.025 (0.019)
Republican share (2016)	-0.074 (0.049)	-0.041 (0.046)
Primary (2016) Clinton	-0.073*** (0.016)	-0.096*** (0.029)
Controls	Yes	Yes
Adjusted R ²	0.875	0.875
Precincts	961	961

Notes: Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Precinct-level control variables include the share of ‘yes’ votes on I-732, share of votes for the Republican candidate in the 2016 Presidential election, share of votes for the Democratic candidate in the 2018 midterm elections, and the number of registered voters. Coefficients on all control variables are displayed in Table B.14.

Table B.14: Field experiment: heterogeneous treatment effects within the experiment with control variables

Outcome	Share of 'yes' votes			
Margin of heterogeneity	'Yes' share for I-732	Primary (2016) Clinton	Democratic share (Senate 2018)	Republican share (2016)
Effectiveness	0.003 (0.015)	-0.001 (0.020)	0.023 (0.015)	-0.018 (0.017)
Earmarking	0.037** (0.015)	-0.025 (0.019)	0.038** (0.015)	-0.027 (0.017)
'Yes' share for I-732	0.464*** (0.055)	0.424*** (0.055)	0.433*** (0.053)	0.431*** (0.054)
Effectiveness \times 'Yes' share for I-732	-0.003 (0.045)			
Earmarking \times 'Yes' share for I-732	-0.090** (0.046)			
Primary (2016) Clinton	-0.072*** (0.016)	-0.096*** (0.029)	-0.073*** (0.016)	-0.072*** (0.016)
Democratic share (Senate 2016)	0.572*** (0.049)	0.574*** (0.050)	0.606*** (0.057)	0.570*** (0.049)
Republican share (2016)	-0.038 (0.046)	-0.041 (0.046)	-0.039 (0.046)	-0.039 (0.046)
Registered voters	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000 (0.000)
Effectiveness \times Primary (2016) Clinton		0.008 (0.040)		
Earmarking \times Primary (2016) Clinton		0.067* (0.038)		
Effectiveness \times Democratic share (Senate 2018)			-0.045 (0.033)	
Earmarking \times Democratic share (Senate 2018)			-0.067** (0.034)	
Effectiveness \times Republican share (2016)				0.039 (0.031)
Earmarking \times Republican share (2016)				0.068** (0.031)
Effectiveness \times Registered voters				0.000 (0.000)
Earmarking \times Registered voters				0.000 (0.000)
Constant	-0.045 (0.047)	-0.021 (0.047)	-0.050 (0.048)	-0.015 (0.048)
Precincts	961	961	961	961
Adjusted R^2	0.875	0.875	0.875	0.875

Notes: Heteroscedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.15: Field experiment: heterogeneous treatment effects over the entire state

Outcome	Share of ‘yes’ votes	
	Republican share (2016)	Primary (2016) Clinton
	(1)	(2)
Baseline × Republican share (2016)	-0.010 (0.026)	
Effectiveness × Republican share (2016)	0.037* (0.021)	
Earmarking × Republican share (2016)	0.063*** (0.021)	
Baseline × Primary (2016) Clinton		-0.003 (0.031)
Effectiveness × Primary (2016) Clinton		-0.036 (0.028)
Earmarking × Primary (2016) Clinton		0.035 (0.028)
Baseline	0.010 (0.014)	0.006 (0.016)
Effectiveness	-0.012 (0.012)	0.024 (0.015)
Earmarking	-0.019* (0.011)	-0.005 (0.014)
Republican share (2016)	-0.157*** (0.047)	-0.152*** (0.047)
Primary (2016) Clinton	-0.066*** (0.008)	-0.066*** (0.009)
Controls	Yes	Yes
Adjusted R ²	0.936	0.936
Precincts	7,083	7,083

Notes: Heteroscedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Precinct-level voting covariates include share of ‘yes’ votes on I-732, share of votes for Hillary Clinton in the 2016 Democratic primary, share of votes for the Republican candidate in the 2016 Presidential election, share of votes for the Democratic candidate in the 2018 midterm elections, and the number of registered voters. Precinct-level demographic covariates include proportions of males, white people, commuters using public transport, commuters using cars, people with bachelors degrees, median income, and median age. Coefficients for all control variables are displayed in Table B.16.

Table B.16: Field experiment: heterogeneous treatment effects over the entire state with control variables

Outcome	Share of 'yes' votes					
	Margin of heterogeneity	'Yes' share for I-732	Primary (2016) Clinton	Democratic share (Senate 2018)	Republican share (2016)	Registered voters
Baseline		0.004 (0.012)	0.006 (0.016)	-0.008 (0.012)	0.010 (0.014)	0.018* (0.010)
Effectiveness		0.011 (0.012)	0.024 (0.015)	0.018* (0.011)	-0.012 (0.012)	0.013* (0.007)
Earmarking		0.044*** (0.012)	-0.005 (0.014)	0.036*** (0.011)	-0.019* (0.011)	0.013 (0.008)
'Yes' share for I-732		0.444*** (0.023)	0.438*** (0.023)	0.439*** (0.023)	0.439*** (0.023)	0.439*** (0.023)
Baseline × 'Yes' share for I-732		0.002 (0.039)				
Effectiveness × 'Yes' share for I-732		-0.011 (0.037)				
Earmarking × 'Yes' share for I-732		-0.095*** (0.037)				
Primary (2016) Clinton		-0.066*** (0.008)	-0.066*** (0.009)	-0.066*** (0.008)	-0.066*** (0.008)	-0.066*** (0.008)
Democratic share (Senate 2018)		0.382*** (0.042)	0.382*** (0.043)	0.383*** (0.042)	0.382*** (0.042)	0.381*** (0.043)
Republican share (2016)		-0.152*** (0.047)	-0.152*** (0.047)	-0.153*** (0.047)	-0.157*** (0.047)	-0.152*** (0.047)
Registered voters		0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
Male (percent)		-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
White (percent)		0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)
Commuting by car (percent)		-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)
Commute by public transport (percent)		0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Bachelor degree (percent)		0.002*** (0.000)	0.002*** (0.000)	0.002*** (0.000)	0.002*** (0.000)	0.002*** (0.000)
Median income		-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)
Median age		0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)
Baseline × Primary (2016) Clinton						
Effectiveness × Primary (2016) Clinton						
Earmarking × Primary (2016) Clinton						
Baseline × Democratic share (Senate 2018)				0.026 (0.028)		
Effectiveness × Democratic share (Senate 2018)				-0.024 (0.023)		
Earmarking × Democratic share (Senate 2018)				-0.052*** (0.022)		
Baseline × Republican share (2016)					-0.010 (0.026)	
Effectiveness × Republican share (2016)					0.037* (0.021)	
Earmarking × Republican share (2016)					0.063*** (0.021)	
Baseline × Registered voters						-0.000 (0.000)
Effectiveness × Registered voters						-0.000 (0.000)
Earmarking × Registered voters						-0.000 (0.000)
Constant		0.071 (0.049)	0.076 (0.050)	0.074 (0.049)	0.075 (0.049)	0.074 (0.049)
Precincts		7.083	7.083	7.083	7.083	7.083
Adjusted R^2		0.936	0.936	0.936	0.936	0.936

Notes: Heteroscedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Notes: Heteroscedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.5 Robustness tests

B.5.1 Non-linear estimations

Since the share of ‘yes’ votes and share of valid votes on I-1631 are bounded between 0 and 1, we complement the linear estimates in Table 3 by estimating a non-linear model. Specifically, we use a fractional logit model with the shares of ‘yes’ votes, and valid votes on I-1631, respectively, as our dependent variables. As in our preferred linear specifications, we use our treatment indicators and the corresponding political and demographic covariates as our independent variables for these specifications. Since these specifications do not allow for analytic weights, we use the number of registered voters in each precinct as frequency weights for these estimations. By construction, this approach involves an increase in the number of observations and reduction in standard errors. That is, our estimates from the fractional logit models confirm the point estimates obtained in our preferred specifications, as described shortly, but standard errors are smaller.

We first perform fractional logit estimations on the sample of precincts included in the field experiment, i.e. the experimental sample. Column (1) of Table B.17 presents marginal effects for the share of ‘yes’ votes on I-1631 and column (2) for the number of valid votes on I-1631 as outcome variables, respectively. The estimations in Table B.17 corroborate the result obtained from the linear estimation in Table 3, showing that the Earmarking treatment leads to a statistically significant increase in the share of votes for the initiative. The magnitude of the effect is 0.2 percentage points for Effectiveness and 0.6 percentage points for Earmarking, compared to 0.2 and 0.8 percentage points, respectively, in the linear specification. Hence, our esti-

mates from the fractional logit models largely confirm the point estimates obtained in our preferred specifications. As described earlier, standard errors are mechanically smaller in the estimations of Table B.17, leading the Effectiveness treatment to also be statistically significant at the 1% level.

Marginal effects on control variables are also in the same directions as in the linear specification of Table B.11.

Table B.17: Field experiment: marginal effects from fractional logit estimation of treatment effects within experiment

Outcome	Share of ‘yes’ votes (1)	Valid votes for I-1631 (2)
Effectiveness	0.002*** (0.000)	-0.008*** (0.000)
Earmarking	0.006*** (0.000)	-0.003*** (0.000)
‘Yes’ share for I-732	0.385*** (0.003)	-0.532*** (0.009)
Primary (2016) Clinton	-0.068*** (0.001)	0.130*** (0.002)
Democrat share (Senate 2018)	0.546*** (0.003)	0.169*** (0.014)
Republican share (2016)	-0.053*** (0.003)	-0.193*** (0.017)
Registered voters	0.000*** (0.000)	-0.000*** (0.000)
Precincts	961	961
Pseudo- R^2	0.057	0.003
Observations (accounting for frequency weights)	565,980	565,980

Notes: Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The number of observations is calculated by multiplying each precinct-level observation by the number of registered voters in that precinct.

Second, we test whether the heterogeneous effects of our treatments on share of ‘yes’ votes presented in Table B.13 are corroborated by the non-linear fractional logit specification. Table B.18 shows average effects and interaction terms that capture

heterogeneous effects of the treatments, as in Table B.14, but estimated with fractional logit. Columns (1) and (3) of Table B.18 present heterogeneous effects on the basis of Republican versus Democrat vote share, and columns (2) and (4) present heterogeneous effects on the basis of Clinton vs. Sanders vote shares. We find that the heterogeneous effects estimated with our preferred specification in Table B.14 are corroborated by the non-linear estimates provided in Table B.18. Specifically, both treatments have stronger effects on the share of ‘yes’ votes in Republican-leaning precincts, while the Earmarking treatment has stronger effects in Clinton-leaning precincts, as also graphically displayed in Figure B.2.

Table B.18: Field experiment: fractional logit estimation of heterogeneous treatment effects within the experiment

Outcome	Share of ‘yes’ votes		Valid votes for I-1631	
	Republican share (2016)	Primary (2016) Clinton	Republican share (2016)	Primary (2016) Clinton
Margin of heterogeneity	(1)	(2)	(3)	(4)
Effectiveness \times Republican share (2016)	0.133*** (0.006)		0.000 (0.017)	
Earmarking \times Republican share (2016)	0.259*** (0.006)		-0.273*** (0.016)	
Effectiveness \times Primary (2016) Clinton		0.047*** (0.009)		1.054*** (0.026)
Earmarking \times Primary (2016) Clinton		0.317*** (0.009)		0.403*** (0.023)
Effectiveness	-0.057*** (0.003)	-0.015*** (0.005)	-0.036*** (0.010)	-0.542*** (0.013)
Earmarking	-0.099*** (0.003)	-0.127*** (0.004)	0.126*** (0.009)	-0.206*** (0.011)
Republican share (2016)	-0.381*** (0.017)	-0.264*** (0.017)	-0.800*** (0.073)	-0.937*** (0.076)
‘Yes’ share for I-732	1.886*** (0.012)	1.850*** (0.012)	-2.413*** (0.039)	-2.351*** (0.039)
Primary (2016) Clinton	-0.341*** (0.004)	-0.450*** (0.007)	0.596*** (0.010)	0.075*** (0.020)
Democrat share (Senate 2018)	2.654*** (0.015)	2.671*** (0.015)	0.750*** (0.063)	0.671*** (0.064)
Registered voters	0.000*** (0.000)	0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)
Precincts	961	961	961	961
Pseudo R ²	0.057	0.057	0.003	0.004
Observations	565,980	565,980	565,980	565,980

Notes: Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Number of observations accounts for frequency weights for each precinct

Third, we perform a similar fractional logit estimation for all precincts in the

state, which includes precincts that do not belong to the experimental sample and thus are not exposed to any ads. The estimates are presented in Table B.19. Column (1) presents results for the share of ‘yes’ votes on I-1631 and column (2) for the share of valid votes on I-1631. We find effects in the same direction and similar order of magnitude compared to the linear specification in Table 4. Specifically, the Baseline treatment causes a 0.3 percentage point increase in the share of ‘yes’ votes compared to precincts that are not exposed to any ads. The Effectiveness and Earmarking treatments cause increases of 0.5 percentage points and 1 percentage point, respectively.

Table B.19: Field experiment: Marginal effects from fractional logit estimation of treatment effects over the entire state

Outcome	Share of ‘yes’ votes	Valid votes for I-1631
	(1)	(2)
Baseline	0.003*** (0.000)	0.009*** (0.000)
Effectiveness	0.005*** (0.000)	0.003*** (0.000)
Earmarking	0.010*** (0.000)	0.009*** (0.000)
‘Yes’ share for I-732	0.444*** (0.001)	-0.423*** (0.002)
Primary (2016) Clinton	-0.060*** (0.000)	0.132*** (0.001)
Democrat share (Senate 2018)	0.371*** (0.001)	0.018*** (0.003)
Republican share (2016)	-0.152*** (0.002)	-0.152*** (0.004)
Registered voters	0.000*** (0.000)	-0.000*** (0.000)
Male (percentage)	-0.000*** (0.000)	-0.001*** (0.000)
White (percentage)	0.001*** (0.000)	0.002*** (0.000)
Commuting by car (percentage)	-0.001*** (0.000)	0.000*** (0.000)
Commuting by public transport (percentage)	0.000*** (0.000)	0.003*** (0.000)
Bachelor degree (percentage)	0.002*** (0.000)	0.003*** (0.000)
Median income	-0.000*** (0.000)	-0.000*** (0.000)
Median age	0.000*** (0.000)	0.004*** (0.000)
Precincts	7,083	7,083
Pseudo R ²	0.082	0.023
Observations	4,687,966	4,687,966

Notes: Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Number of observations accounts for frequency weights for each precinct.

Fourth, we test for heterogeneous effects of our treatments using the same non-linear specification for all precincts in the state. Table B.20 shows the average effects and interaction terms that capture heterogenous effects of the treatments. The coef-

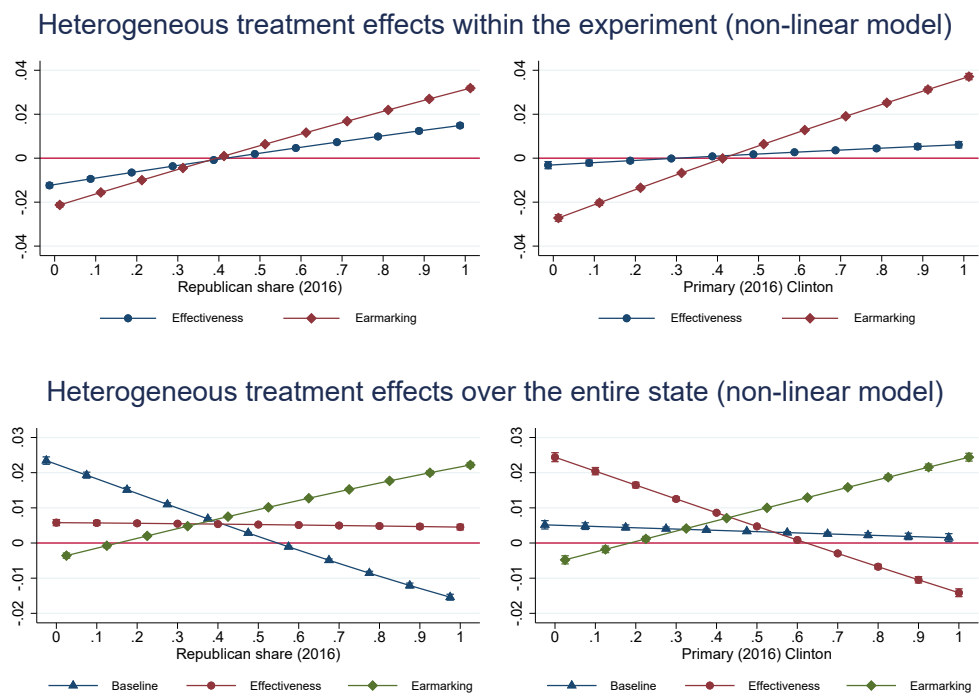
ficients are mostly in the same direction as the linear estimation on the sample of all precincts, except for the interaction of the effectiveness treatment with Republican vote share, which is negative but not statistically significantly different from 0.

Table B.20: Field experiment: fractional logit estimation of heterogeneous treatment effects over the entire state

Outcome	Share of 'yes' votes			Valid votes for I-1631		
	Republican share (2016)	Primary (2016)	Clinton	Republican share (2016)	Primary (2016)	Clinton
Margin of heterogeneity	(1)	(2)		(3)	(4)	
Baseline x Republican share (2016)	-0.178*** (0.005)			0.193*** (0.008)		
Effectiveness x Republican share (2016)	-0.003 (0.004)			0.254*** (0.010)		
Earmarking x Republican share (2016)	0.122*** (0.004)			0.024*** (0.007)		
Baseline x Primary (2016) Clinton		-0.016** (0.006)			-0.264*** (0.015)	
Effectiveness x Primary (2016) Clinton		-0.176*** (0.007)			0.233*** (0.016)	
Earmarking x Primary (2016) Clinton		0.135*** (0.006)			-0.203*** (0.010)	
Baseline	0.102*** (0.003)	0.023*** (0.003)		-0.056*** (0.004)	0.168*** (0.008)	
Effectiveness	0.025*** (0.002)	0.110*** (0.003)		-0.114*** (0.005)	-0.097*** (0.008)	
Earmarking	-0.016*** (0.002)	-0.022*** (0.003)		0.030*** (0.004)	0.140*** (0.005)	
'Yes' share for I-732	2.036*** (0.004)	2.031*** (0.004)		-1.915*** (0.009)	-1.909*** (0.009)	
Republican share (2016)	-0.691*** (0.007)	-0.696*** (0.007)		-0.711*** (0.016)	-0.688*** (0.016)	
Primary (2016) Clinton	-0.276*** (0.002)	-0.271*** (0.002)		0.601*** (0.003)	0.608*** (0.003)	
Democrat share (Senate 2018)	1.697*** (0.006)	1.700*** (0.006)		0.085*** (0.012)	0.080*** (0.012)	
Registered voters	0.000*** (0.000)	0.000*** (0.000)		-0.000*** (0.000)	-0.000*** (0.000)	
Male (percent)	-0.001*** (0.000)	-0.001*** (0.000)		-0.006*** (0.000)	-0.006*** (0.000)	
White (percent)	0.004*** (0.000)	0.004*** (0.000)		0.008*** (0.000)	0.008*** (0.000)	
Commuting by car (percent)	-0.004*** (0.000)	-0.004*** (0.000)		0.001*** (0.000)	0.001*** (0.000)	
Commuting by public transport (percent)	0.002*** (0.000)	0.002*** (0.000)		0.011*** (0.000)	0.012*** (0.000)	
Bachelor degree (percent)	0.008*** (0.000)	0.008*** (0.000)		0.015*** (0.000)	0.015*** (0.000)	
Median income	-0.000*** (0.000)	-0.000*** (0.000)		-0.000*** (0.000)	-0.000*** (0.000)	
Median age	0.002*** (0.000)	0.002*** (0.000)		0.019*** (0.000)	0.019*** (0.000)	
Constant	-2.026*** (0.008)	-2.029*** (0.008)		-0.124*** (0.018)	-0.127*** (0.018)	
Precincts	7,083	7,083		7,083	7,083	
Pseudo R ²	0.082	0.082		0.023	0.023	
Observations	4,687,966	4,687,966		4,687,966	4,687,966	

Notes: Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Number of observations accounts for frequency weights for each precinct.

Figure B.2: Field experiment: heterogeneous treatment effects (non-linear estimations)



B.5.2 Instrumental variable approach

We do not observe whether individual voters in treated precincts see and internalize the content of our treatment messages. Therefore, we expect the intent-to-treat effect obtained utilizing treatment assignment to be a lower bound of the true average treatment effect. We know that our treatments reached approximately 50% of the voting-eligible population of precincts that were selected into the experiment (see Section 5.2). However, Facebook does not provide information on reach at the precinct level, but rather at the aggregate level by treatment. Dividing our main estimates (intent to treat) by the compliance rate, the effect of our Effectiveness treatment within the experiment would be 0.35%, given a compliance rate of approximately 57%. The effect of our Earmarking treatment within the experiment would be 1.5%, given a compliance rate of approximately 53%. Compared to not seeing any messages, i.e. precincts in the rest of the state, the Effectiveness and Earmarking treatments lead to increases of 1.2% and 2.3%, respectively, given the same compliance rates of 57% and 53%.

We further utilize an instrumental variable (IV) approach with three endogenous variables and three instruments to investigate differences between intent-to-treat versus average treatment effects, since this approach allows us to use precinct-level data on voting-eligible population to get a more precise estimate of the local average treatment effect. Specifically, we use precinct-level treatment assignment to instrument for coverage of voters within a precinct, i.e. the share of voters who are exposed to our treatment messages via Facebook. In order to have a more disaggregated measure of actual exposure to treatment at the precinct level, we utilize the geo-coded

voter registry database obtained from the WA SOS data. From this database, we infer the share of voters who are potentially exposed to the treatment if their registered address falls in the buffer within which the Facebook advertisements are shown. To summarize, we make two assumptions to obtain a precinct-level measure of coverage of our treatments. First, we assume that the 50% share of exposure among the voting-eligible population is constant across precincts. Second, we assume that voters reside at their registered address and view the messages on Facebook at this address. Our measure of coverage of our treatment for precinct i is formulated as follows: $Coverage_i = \frac{Reach}{Population} \times Share\ within\ buffer_i$.

In the empirical IV specification, we estimate the following first-stage equations:

$$\begin{aligned}
\widehat{Coverage(Baseline)}_i &= \hat{\nu}_0 + \hat{\nu}_1 * Baseline_i + \hat{\nu}_2 * Effectiveness_i + \\
&\hat{\nu}_3 * Earmarking_i + \hat{\gamma}_1 * X_i \\
\widehat{Coverage(Effectiveness)}_i &= \hat{\mu}_0 + \hat{\mu}_1 * Baseline_i + \hat{\mu}_2 * Effectiveness_i + \\
&\hat{\mu}_3 * Earmarking_i + \hat{\gamma}_2 * X_i \\
\widehat{Coverage(Earmarking)}_i &= \hat{\eta}_0 + \hat{\eta}_1 * Baseline_i + \hat{\eta}_2 * Effectiveness_i + \\
&\hat{\eta}_3 * Earmarking_i + \hat{\gamma}_3 * X_i
\end{aligned} \tag{7}$$

where $Baseline_i$, $Effectiveness_i$, and $Earmarking_i$ are precinct-level treatment assignments, and $Coverage(Baseline)_i$, $Coverage(Effectiveness)_i$, and $Coverage(Earmarking)_i$ are precinct-level coverage of our treatments as measured using the formula mentioned earlier. As before, X_i includes the standard set of

precinct-level covariates on demographics and voting outcomes.

The second-stage estimation is, therefore:

$$y_i = \alpha + \beta_1 * \widehat{Coverage(Baseline)}_i + \beta_2 * \widehat{Coverage(Effectiveness)}_i + \beta_3 * \widehat{Coverage(Earmarking)}_i + \gamma_4 * X_i + \varepsilon_i \quad (8)$$

where y_i is the share of ‘yes’ votes on I-1631 in precinct i . The main coefficients of interest are β_1 , β_2 , and β_3 , i.e. the effect of a unit increase in coverage of voters in precinct i on share of ‘yes’ votes for that precinct.

Table B.21 describes results from the IV specifications, and compares them with the intent-to-treat effects described in the linear specifications. Odd-numbered columns include results from linear specifications, as already described in Tables B.11 and B.12. Column (1) provides results from the linear specification for precincts selected into the experiment and column (3) provides linear estimates for all precincts when the sample is expanded to all precincts in the state. Even-numbered columns include results from the corresponding IV specifications, with column (2) describing results for precincts selected into the experiment and column (4) describing results when extending the sample to all the precincts in the state. As can be seen from the magnitudes of the coefficients, the intent-to-treat results are indeed potentially a lower bound of the average treatment effect, with coefficients on coverage in the IV specification being more than double in magnitude in all specifications.

Table B.21: Field experiment: instrumental variable estimation of effect of coverage of treatments

Sample	Within experiment		Entire state	
Specification	Linear	IV	Linear	IV
	(1)	(2)	(3)	(4)
Baseline			0.004 (0.003)	
Coverage (Baseline)				0.010 (0.007)
Effectiveness	0.002 (0.004)		0.007* (0.004)	
Coverage (Effectiveness)		0.005 (0.011)		0.017* (0.009)
Earmarking	0.008* (0.005)		0.012*** (0.004)	
Coverage (Earmarking)		0.019* (0.011)		0.031*** (0.010)
‘Yes’ share for I-732	0.428*** (0.055)	0.427*** (0.055)	0.438*** (0.023)	0.438*** (0.023)
Primary (2016) Clinton	-0.071*** (0.016)	-0.071*** (0.016)	-0.066*** (0.008)	-0.066*** (0.008)
Democrat share (Senate 2018)	0.571*** (0.050)	0.570*** (0.050)	0.381*** (0.043)	0.381*** (0.042)
Republican share (2016)	-0.041 (0.046)	-0.041 (0.046)	-0.153*** (0.047)	-0.153*** (0.047)
Registered voters	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
Male (percent)			-0.000 (0.000)	-0.000 (0.000)
White (percent)			0.001*** (0.000)	0.001*** (0.000)
Commuting by car (percent)			-0.001*** (0.000)	-0.001*** (0.000)
Commuting by public transport (percent)			0.000 (0.000)	0.000 (0.000)
Bachelor degree (percent)			0.002*** (0.000)	0.002*** (0.000)
Median income			-0.000*** (0.000)	-0.000*** (0.000)
Median age			0.001*** (0.000)	0.001*** (0.000)
Constant	-0.032 (0.047)	-0.031 (0.047)	0.076 (0.049)	0.076 (0.049)
Precincts	961	961	7,083	7,083
Adjusted R^2	0.875	0.874	0.936	0.936
Cragg-Donald F-stat		4,010.537		44,247.357

Notes: Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.5.3 Addressing extra exposure across precincts

As described in Section 3.3, our process of selecting precincts into the experiment and implementing the treatment messages involved some extra exposure beyond treatment assigned. By extra exposure, we refer to voters residing near but outside the border of a given precinct who may have been exposed to the treatment assigned to the precinct since their residence falls within the buffer over which treatment messages are displayed via Facebook. The extent of contamination was limited by our design aimed at minimizing undertreatment and contamination, and thus we expect its impact to be very limited. Still, in this section we explicitly account for this potential contamination.

From the geo-coded WA SOS voter registry database, we can calculate the share of the population in a precinct that could have potentially been exposed to a treatment assigned to a contiguous precinct. Performing this calculation for all precincts in the state, we can obtain a measure of treatment that incorporates both our randomized treatment assignment and potential contamination of treatments from neighboring precincts. Formally, our combined measures of contamination and treatment assignment at the precinct level are as follows:

$$\begin{aligned}
 \textit{Baseline exposure}_i &= \textit{Baseline}_i + \textit{Extra Exposure Baseline}_i \\
 \textit{Effectiveness exposure}_i &= \textit{Effectiveness}_i + \textit{Extra Exposure Effectiveness}_i \quad (9) \\
 \textit{Earmarking exposure}_i &= \textit{Earmarking}_i + \textit{Extra Exposure Earmarking}_i
 \end{aligned}$$

where $\textit{Baseline}_i$, $\textit{Effectiveness}_i$ and $\textit{Earmarking}_i$ are dummy variables that

take value 1 if precinct i is assigned the Baseline, Effectiveness or Earmarking treatment, respectively, and 0 otherwise. *Extra Exposure Baseline_i*, *Extra Exposure Effectiveness_i*, and *Extra Exposure Earmarking_i* are fractions that represent the share of the population in voting precinct i that could have been exposed to the Baseline, Earmarking, and Effectiveness treatments, respectively, from a neighboring precinct that was assigned to one of the three treatments. Using these combined measures of extra exposure and treatment assignment, we estimate the following equation:

$$\begin{aligned}
y_i = & \alpha + \beta_1 \times \text{Baseline exposure}_i \\
& + \beta_2 \times \text{Effectiveness exposure}_i + \beta_3 \times \text{Earmarking exposure}_i \\
& + \gamma \times X_i + \epsilon_i
\end{aligned} \tag{10}$$

where y_i is the share of ‘yes’ votes on I-1631 in precinct i , *Baseline exposure_i* is the combined exposure to the Baseline treatment for precinct i , *Effectiveness exposure_i* is the combined exposure to the Effectiveness treatment for precinct i , *Earmarking exposure_i* is the combined exposure to the Earmarking treatment for precinct i , X_i are relevant political or political and demographic covariates for precinct i , and ϵ_i is the error term. The coefficients of interest are β_1 , β_2 , and β_3 , which are the combined effects of assignment to the Baseline, Effectiveness, and Earmarking treatments, respectively, while adding in the accumulated exposure to each of these treatments through contamination between precincts.

Table B.22 describe the results, with column (1) describing the results reported

earlier (without extra exposure) and column (2) describing the results accounting for extra exposure. Comparing coefficients in columns (1) and (2), we see that the treatment effects within the sample of precincts included in the experiment remain similar when extra exposure from other precincts is taken into account.

Table B.22: Field experiment: average treatment effects accounting for extra exposure

Sample	Within experiment		Entire state	
	Main Results	Extra Exposure	Main Results	Extra Exposure
	(1)	(2)	(3)	(4)
Effectiveness	0.002 (0.004)		0.007* (0.004)	
Earmarking	0.008* (0.005)		0.012*** (0.004)	
Baseline			0.004 (0.003)	
Effectiveness exposure		0.006 (0.009)		0.001 (0.003)
Earmarking exposure		0.022* (0.012)		0.007** (0.003)
Baseline exposure				-0.012 (0.009)
‘Yes’ share for I-732	0.428*** (0.055)	0.430*** (0.056)	0.438*** (0.023)	0.438*** (0.024)
Primary (2016) Clinton	-0.071*** (0.016)	-0.070*** (0.016)	-0.066*** (0.008)	-0.067*** (0.008)
Democrat share (Senate 2018)	0.571*** (0.050)	0.571*** (0.050)	0.381*** (0.043)	0.380*** (0.042)
Republican share (2016)	-0.041 (0.046)	-0.040 (0.047)	-0.153*** (0.047)	-0.151*** (0.047)
Registered voters	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
Male (percent)			-0.000 (0.000)	-0.000 (0.000)
White (percent)			0.001*** (0.000)	0.001*** (0.000)
Commuting by car (percent)			-0.001*** (0.000)	-0.001*** (0.000)
Commute by public transport (percent)			0.000 (0.000)	0.000 (0.000)
Bachelor degree (percent)			0.002*** (0.000)	0.002*** (0.000)
Median income			-0.000*** (0.000)	-0.000*** (0.000)
Median age			0.001*** (0.000)	0.001*** (0.000)
Constant	-0.032 (0.047)	-0.033 (0.047)	0.076 (0.049)	0.074 (0.050)
Precincts	961	961	7,083	7,083
Adjusted R^2	0.875	0.874	0.936	0.936

Notes: Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.5.4 Different sets of control variables

This section provides our estimates for the average treatment effect within precincts included in the experiment and those for the entire state, testing robustness to the addition of control variables. For precincts within the experiment, we start with our main specification in column (1) of Table B.23. For the set of precincts within the experiment, column (2) reports estimates when adding the set of demographic covariates that we use as controls for the results for the entire state. Columns (3) and (4) report estimates when adding in more control variables from the American Community Survey (ACS) data, specifically the median home value and proportion of people below poverty line (column (3)) and the proportion of workers employed in agriculture and mining, and manufacturing (column (4)). For results over the entire state, we report our main specification in column (1) of Table B.24. In columns (2) and (3), we include further demographic controls as described earlier. Our estimated treatment effects do not change when including these covariates, either within the experiment or for the entire state.

Table B.23: Field experiment: average treatment effects within the experiment with different sets of covariates

	Main specification	Demographic controls	Additional controls 1	Additional controls 2
	(1)	(2)	(3)	(4)
Effectiveness	0.002 (0.004)	0.002 (0.004)	0.002 (0.004)	0.001 (0.004)
Earmarking	0.008* (0.005)	0.008* (0.004)	0.008* (0.004)	0.007* (0.004)
‘Yes’ share for I-732	0.428*** (0.055)	0.400*** (0.060)	0.400*** (0.059)	0.382*** (0.056)
Primary (2016) Clinton	-0.071*** (0.016)	-0.073*** (0.016)	-0.072*** (0.016)	-0.062*** (0.015)
Democrat share (Senate 2018)	0.571*** (0.050)	0.571*** (0.046)	0.573*** (0.046)	0.555*** (0.045)
Republican share (2016)	-0.041 (0.046)	0.027 (0.046)	0.027 (0.044)	0.011 (0.043)
Registered voters	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
Male (percent)		0.001 (0.001)	0.001 (0.001)	0.001* (0.001)
White (percent)		-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
Commuting by car (percent)		-0.000 (0.000)	-0.001 (0.000)	-0.001*** (0.000)
Commuting by public transport (percent)		0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
Bachelor degree (percent)		0.002*** (0.000)	0.002*** (0.000)	0.001*** (0.000)
Median income		-0.000** (0.000)	-0.000** (0.000)	-0.000*** (0.000)
Median age		0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)
Median home value			0.000 (0.000)	0.000 (0.000)
Below poverty line (percent)			-0.001 (0.001)	-0.000 (0.001)
Agri and mining workers (percent)				-0.001*** (0.000)
Manufacturing workers (percent)				-0.001** (0.000)
Constant	-0.032 (0.047)	-0.102 (0.066)	-0.088 (0.065)	0.007 (0.067)
Precincts	961	961	961	961
Adj R ²	0.875	0.889	0.888	0.892

Notes: Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.24: Field experiment: average treatment effects for entire state with different sets of covariates

	Main specification	Additional controls 1	Additional controls 2
	(1)	(2)	(3)
Baseline	0.004 (0.003)	0.004 (0.003)	0.004 (0.003)
Effectiveness	0.007* (0.004)	0.007* (0.004)	0.007** (0.004)
Earmarking	0.012*** (0.004)	0.012*** (0.004)	0.013*** (0.004)
‘Yes’ share for I-732	0.438*** (0.023)	0.437*** (0.024)	0.431*** (0.023)
Primary (2016) Clinton	-0.066*** (0.008)	-0.066*** (0.008)	-0.068*** (0.008)
Democrat share (Senate 2018)	0.381*** (0.043)	0.381*** (0.043)	0.379*** (0.043)
Republican share (2016)	-0.153*** (0.047)	-0.152*** (0.047)	-0.163*** (0.047)
Registered voters	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
Male (percent)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
White (percent)	0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)
Commuting by car (percent)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)
Commuting by public transport (percent)	0.000 (0.000)	0.000* (0.000)	0.000* (0.000)
Bachelor degree (percent)	0.002*** (0.000)	0.002*** (0.000)	0.001*** (0.000)
Median income	-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)
Median age	0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)
Median home value		0.000 (0.000)	0.000* (0.000)
Below poverty line (percent)		0.000* (0.000)	0.000 (0.000)
Agri and mining workers (percent)			-0.000* (0.000)
Manufacturing workers (percent)			-0.002*** (0.000)
Constant	0.076 (0.049)	0.065 (0.049)	0.091* (0.049)
Precincts	7,083	7,083	7,083
Adj R ²	0.936	0.936	0.937

Heteroskedasticity-robust standard errors in parentheses. *: $p < 0.10$; **: $p < 0.05$ ***: $p < 0.01$.

C Analysis of media coverage

This section contains tables pertaining to our analysis of media slant as a mechanism for heterogeneous treatment effects. We start with describing relevant statistics for each of the news organizations in Table C.25.

Table C.25: News text sources, coverage, and media slant scores

News source	Source of corpora	Source of county-level viewership/readership	% of households viewing/reading	Number of articles mentioning ‘carbon tax’	Relevance score	Direction score
Cable news						
Fox News	Factiva	SimmonsLocal	18.128	59	1.917 (1.593)	-1.000 (1.993)
CNN	Factiva	SimmonsLocal	15.359	116	2.700 (1.416)	-2.468 (1.5450)
Print - national newspapers						
New York Times	Factiva	SimmonsLocal	11.357	211	2.920 (1.131)	-2.769 (1.254)
Wall Street Journal	Factiva	SimmonsLocal	9.955	121	2.671 (1.204)	-2.364 (1.479)
Washington Post	Factiva	proxied by New York Times readership	11.357	220	2.838 (1.188)	-2.638 (1.393)
Print - regional newspapers						
Seattle Times	Access World News	Alliance for Audited Media	1.260	99	1.804 (1.148)	-1.578 (1.256)
Spokesman Review	NexisUni	Alliance for Audited Media	1.359	96	1.792 (1.122)	-1.450 (1.337)
Bellingham Herald	Access World News	Alliance for Audited Media	0.200	76	2.069 (1.186)	-1.843 (1.314)
Daily News	Access World News	Alliance for Audited Media	0.795	33	2.048 (1.166)	-1.274 (1.454)
Daily World	Access World News	Alliance for Audited Media	0.359	23	1.600 (1.040)	-1.400 (1.103)
Tri-city Herald	Access World News	Alliance for Audited Media	0.641	86	2.028 (1.139)	-1.561 (1.341)
Kitsap Sun	Access World News	Alliance for Audited Media	0.256	25	2.007 (1.136)	-1.599 (1.411)
Columbian	Access World News	Alliance for Audited Media	0.333	57	1.796 (1.168)	-1.467 (1.274)
Anacortes American	Access World News	Alliance for Audited Media	0.103	1	2.500 (1.291)	-2.500 (1.291)
Skagit Valley Herald	Access World News	Alliance for Audited Media	0.415	18	2.175 (1.178)	-2.075 (1.209)
Olympian	Access World News	Alliance for Audited Media	0.308	72	2.028 (1.139)	-1.561 (1.341)

Notes: Standard deviation of media slant scores in parentheses.

Table C.26 shows heterogeneous treatment effects for precincts that are more exposed to coverage about carbon taxes, measured as the number of news segments or articles mentioning the term “carbon tax” weighted by the county-level viewership or readership of each news source. The sample includes all precincts in the state. In column (1), where the suite of control variables are not included in the specification, the results indicate that precincts that are more exposed to news about carbon taxes are less likely to increase support for I-1631 in response to our treatments. However, when the control variables are included, the effects on the interaction terms are not precisely estimated.

Table C.26: Field experiment: heterogeneous effects based on exposure measured as viewership / readership weighted news count over the entire state

Outcome	Share of ‘yes’ votes	
	(1)	(2)
Effectiveness \times News count	-0.013*** (0.004)	0.001 (0.002)
Earmarking \times News count	-0.013*** (0.003)	-0.001 (0.001)
Baseline \times News count	-0.009** (0.004)	0.001 (0.001)
Effectiveness	0.127* (0.063)	-0.011 (0.024)
Earmarking	0.134** (0.050)	0.024 (0.021)
Baseline	0.079 (0.055)	-0.010 (0.022)
News count	0.025*** (0.002)	-0.001 (0.001)
Constant	0.032 (0.041)	0.098 (0.138)
Controls	No	Yes
Precincts	7,083	7,083
Adjusted R^2	0.309	0.936

Notes: Standard errors clustered at county level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Precinct-level control variables include the share of ‘yes’ votes on I-732, share of votes for the Republican candidate in the 2016 Presidential election, share of votes for the Democratic candidate in the 2018 midterm elections, the number of registered voters, proportions of males, white people, commuters using public transport, commuters using cars, proportion of people with bachelors degrees, median income, and median age. Coefficients for all control variables are included in Table C.27.

Tables C.27 to C.29 provide additional estimates of heterogenous treatment effects, including coefficients on all covariates, as well as breaking down the measure

of exposure for cable news and print news, respectively.

Table C.27: Field experiment: heterogeneous effects based on exposure measured as viewership/readership weighted news count

Outcome Sample of precincts	Share of 'yes' votes			
	Within experiment	Entire state	Within experiment	Entire state
	(1)	(2)	(3)	(4)
Effectiveness	0.048 (0.047)	0.127* (0.063)	-0.006 (0.020)	-0.011 (0.024)
Earmarking	0.055 (0.041)	0.134** (0.050)	0.055*** (0.016)	0.024 (0.021)
News count	0.016*** (0.004)	0.025*** (0.002)	0.003** (0.001)	-0.001 (0.001)
Effectiveness × News count	-0.003 (0.003)	-0.013*** (0.004)	0.001 (0.001)	0.001 (0.002)
Earmarking × News count	-0.004 (0.003)	-0.013*** (0.003)	-0.003*** (0.001)	-0.001 (0.001)
Baseline		0.079 (0.055)		-0.010 (0.022)
Baseline × News count		-0.009** (0.004)		0.001 (0.001)
'Yes' share for I-732			0.430*** (0.057)	0.429*** (0.074)
Primary (2016) Clinton			-0.071*** (0.016)	-0.064*** (0.013)
Democrat share (Senate 2018)			0.571*** (0.057)	0.390** (0.189)
Republican share (2016)			-0.031 (0.065)	-0.154 (0.163)
Registered voters			0.000** (0.000)	0.000* (0.000)
Male (percent)				-0.000 (0.000)
White (percent)				0.001*** (0.000)
Commuting by car (percent)				-0.001*** (0.000)
Commute by public transport (percent)				0.001 (0.000)
Bachelor degree (percent)				0.002*** (0.000)
Median income				-0.000** (0.000)
Median age				0.001** (0.000)
Constant	0.111* (0.056)	0.032 (0.041)	-0.079 (0.065)	0.098 (0.138)
Precincts	961	7,083	961	7,083
Adjusted R^2	0.059	0.309	0.876	0.936

Notes: Standard errors clustered at county level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.28: Field experiment: Heterogeneous effects based on exposure measured as viewership/readership weighted cable news segment count

Outcome	Share of 'yes' votes			
Sample	Within experiment	Entire state	Within experiment	Entire state
	(1)	(2)	(3)	(4)
Effectiveness	0.169*	0.631***	0.100**	0.047
	(0.094)	(0.170)	(0.041)	(0.044)
Earmarking	0.046	0.507***	0.104**	0.046
	(0.098)	(0.146)	(0.044)	(0.044)
Cable news count	0.020**	0.059***	0.002	-0.003
	(0.009)	(0.014)	(0.003)	(0.003)
Effectiveness	-0.012*	-0.051***	-0.007**	-0.003
× Cable news count	(0.006)	(0.012)	(0.003)	(0.003)
Earmarking ×	-0.003	-0.042***	-0.007**	-0.002
Cable news count	(0.007)	(0.010)	(0.003)	(0.003)
Baseline		0.462***		-0.035
		(0.157)		(0.045)
Baseline ×		-0.039***		0.003
Cable news count		(0.011)		(0.003)
'Yes' share for I-732			0.425***	0.430***
			(0.057)	(0.071)
Primary (2016)			-0.074***	-0.068***
Clinton			(0.017)	(0.015)
Democrat share			0.570***	0.392**
(Senate 2018)			(0.053)	(0.182)
Republican share			-0.051	-0.158
(2016)			(0.061)	(0.160)
Registered voters			0.000	0.000**
			(0.000)	(0.000)
Male (percent)				-0.000
				(0.000)
White (percent)				0.001***
				(0.000)
Commuting by car				-0.001***
(percent)				(0.000)
Commute by public				0.000
transport (percent)				(0.000)
Bachelor degree				0.002***
(percent)				(0.000)
Median income				-0.000***
				(0.000)
Median age				0.001**
				(0.000)
Constant	0.039	-0.423**	-0.045	0.125
	(0.133)	(0.190)	(0.071)	(0.142)
Precincts	961	7,083	961	7,083
Adjusted R^2	0.030	0.255	0.876	0.936

Notes: Standard errors in parentheses clustered at county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.29: Field experiment: Heterogeneous effects based on exposure measured as viewership/readership weighted print news article count

Outcome	Share of 'yes' votes			
Sample	Within experiment	Entire state	Within experiment	Entire state
	(1)	(2)	(3)	(4)
Effectiveness	0.027 (0.033)	0.082* (0.045)	-0.012 (0.014)	-0.011 (0.019)
Earmarking	0.033 (0.029)	0.088** (0.039)	0.038*** (0.012)	0.020 (0.016)
Print news count	0.010*** (0.003)	0.018*** (0.002)	0.002** (0.001)	-0.001 (0.001)
Effectiveness × Print news count	-0.002 (0.002)	-0.010*** (0.003)	0.001 (0.001)	0.001 (0.001)
Earmarking × Print news count	-0.002 (0.002)	-0.010*** (0.002)	-0.002*** (0.001)	-0.001 (0.001)
Baseline		0.055 (0.041)		-0.004 (0.018)
Baseline × Print news count		-0.008** (0.003)		0.001 (0.001)
'Yes' share for I-732			0.428*** (0.057)	0.431*** (0.074)
Primary (2016) Clinton			-0.071*** (0.016)	-0.064*** (0.013)
Democrat share (Senate 2018)			0.574*** (0.057)	0.389** (0.190)
Republican share (2016)			-0.030 (0.064)	-0.153 (0.164)
Registered voters			0.000** (0.000)	0.000* (0.000)
Male (percent)				-0.000 (0.000)
White (percent)				0.001*** (0.000)
Commuting by car (percent)				-0.001*** (0.000)
Commute by public transport (percent)				0.000 (0.000)
Bachelor degree (percent)				0.002*** (0.000)
Median income				-0.000*** (0.000)
Median age				0.001** (0.000)
Constant	0.184*** (0.042)	0.130*** (0.037)	-0.068 (0.063)	0.091 (0.138)
Precincts	961	7,083	961	7,083
Adjusted R^2	0.049	0.297	0.876	0.936

Notes: Standard errors in parentheses clustered at county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Tables C.30 to C.32 present estimates of heterogeneous effects based on slant of news coverage overall, and broken down by slant of cable news and print news, respectively.

Table C.30: Field experiment: Heterogeneous effects based on slant of news coverage with controls

Outcome	Share of 'yes' votes			
Sample	Within experiment		Entire state	
Margin of heterogeneity	CT Relevance	CT Direction	CT Relevance	CT Direction
	(1)	(2)	(3)	(4)
Effectiveness \times CT Relevance	-0.017 (0.018)		-0.012 (0.026)	
Earmarking \times CT Relevance	-0.046** (0.019)		-0.028 (0.024)	
Effectiveness \times CT Direction		0.013 (0.020)		0.008 (0.026)
Earmarking \times CT Direction		0.050** (0.020)		0.030 (0.025)
Baseline \times CT Relevance			0.002 (0.027)	
Baseline \times CT Direction				-0.004 (0.028)
Effectiveness	0.023 (0.024)	0.015 (0.020)	0.021 (0.032)	0.015 (0.027)
Earmarking	0.063** (0.025)	0.057** (0.021)	0.047 (0.033)	0.041 (0.028)
Baseline			0.001 (0.035)	0.000 (0.030)
CT Relevance	0.032* (0.017)		-0.006 (0.028)	
CT Direction		-0.038** (0.019)		0.008 (0.029)
'Yes' share for I-732	0.432*** (0.057)	0.433*** (0.057)	0.436*** (0.073)	0.435*** (0.073)
Primary (2016) Clinton	-0.071*** (0.016)	-0.071*** (0.016)	-0.067*** (0.014)	-0.066*** (0.014)
Democrat share (Senate 2018)	0.568*** (0.056)	0.569*** (0.056)	0.384* (0.192)	0.384* (0.192)
Republican share (2016)	-0.037 (0.064)	-0.034 (0.065)	-0.153 (0.166)	-0.154 (0.166)
Registered voters	0.000* (0.000)	0.000* (0.000)	0.000** (0.000)	0.000** (0.000)
Male (percent)			-0.000 (0.000)	-0.000 (0.000)
White (percent)			0.001*** (0.000)	0.001*** (0.000)
Commuting by car (percent)			-0.001*** (0.000)	-0.001*** (0.000)
Commute by public transport (percent)			0.000 (0.000)	0.000 (0.000)
Bachelor degree (percent)			0.002*** (0.000)	0.002*** (0.000)
Median income			-0.000*** (0.000)	-0.000*** (0.000)
Median age			0.001** (0.000)	0.001** (0.000)
Constant	-0.073 (0.065)	-0.074 (0.065)	0.082 (0.140)	0.082 (0.139)
Precincts	961	961	7,083	7,083
Adjusted R^2	0.875	0.875	0.936	0.936

Notes: Standard errors clustered at county level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.31: Field experiment: heterogeneous effects based on slant of cable news coverage

Outcome	Share of 'yes' votes							
	Within experiment				Entire state			
	CT Relevance	CT Direction	CT Relevance	CT Direction	CT Relevance	CT Direction	CT Relevance	CT Direction
Sample of precincts	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Margin of heterogeneity								
Effectiveness \times CT Relevance Cable News	-0.230*		-1.056***		-0.142**		-0.060	
	(0.135)		(0.282)		(0.053)		(0.063)	
Earmarking \times CT Relevance Cable News	-0.050		-0.876***		-0.132**		-0.048	
	(0.140)		(0.244)		(0.062)		(0.061)	
Effectiveness \times CT Direction Cable News		0.287*		1.202***		0.164**		0.066
		(0.153)		(0.270)		(0.067)		(0.072)
Earmarking \times CT Direction Cable News		0.070		0.985***		0.166**		0.056
		(0.164)		(0.232)		(0.070)		(0.067)
Baseline \times CT Relevance Cable News			-0.826***				0.052	
			(0.270)				(0.067)	
Baseline \times CT Direction Cable News				0.915***				-0.071
				(0.249)				(0.071)
Effectiveness	0.173	0.162*	0.699***	0.585***	0.108**	0.093**	0.052	0.043
	(0.103)	(0.087)	(0.208)	(0.153)	(0.042)	(0.040)	(0.048)	(0.041)
Earmarking	0.042	0.045	0.569***	0.467***	0.107**	0.101**	0.049	0.044
	(0.105)	(0.093)	(0.180)	(0.131)	(0.049)	(0.041)	(0.049)	(0.041)
Baseline			0.536**	0.422***			-0.035	-0.035
			(0.196)	(0.137)			(0.051)	(0.041)
CT Relevance Cable News	0.342*		1.168***		0.026		-0.064	
	(0.198)		(0.328)		(0.054)		(0.066)	
CT Direction Cable News		-0.536**		-1.451***		-0.042		0.082
		(0.211)		(0.305)		(0.064)		(0.077)
'Yes' share for I-732					0.424***	0.426***	0.430***	0.430***
					(0.057)	(0.057)	(0.071)	(0.070)
Primary (2016) Clinton					-0.074***	-0.074***	-0.068***	-0.068***
					(0.017)	(0.017)	(0.015)	(0.015)
Democrat share (Senate 2018)					0.574***	0.568***	0.393**	0.391**
					(0.052)	(0.054)	(0.183)	(0.182)
Republican share (2016)					-0.047	-0.052	-0.156	-0.160
					(0.059)	(0.063)	(0.160)	(0.160)
Registered voters					0.000	0.000	0.000**	0.000**
					(0.000)	(0.000)	(0.000)	(0.000)
Male (percent)							-0.000	-0.000
							(0.000)	(0.000)
White (percent)							0.001***	0.001***
							(0.000)	(0.000)
Commuting by car (percent)							-0.001***	-0.001***
							(0.000)	(0.000)
Commuting by public transport (percent)							0.000	0.000
							(0.000)	(0.000)
Bachelor degree (percent)							0.002***	0.002***
							(0.000)	(0.000)
Median income							-0.000***	-0.000***
							(0.000)	(0.000)
Median age							0.001**	0.001**
							(0.000)	(0.000)
Constant	0.073	0.029	-0.454*	-0.393**	-0.046	-0.045	0.124	0.125
	(0.150)	(0.121)	(0.238)	(0.167)	(0.069)	(0.072)	(0.141)	(0.142)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Precincts	961	961	7,083	7,083	961	961	7,083	7,083
Adjusted R ²	0.018	0.036	0.228	0.267	0.877	0.876	0.936	0.936

Notes: Standard errors clustered at county level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.32: Field experiment: heterogeneous effects based on slant of print news coverage

Outcome	Share of 'yes' votes							
	Within experiment		Entire state		Within experiment		Entire state	
	CT Relevance	CT Direction	CT Relevance	CT Direction	CT Relevance	CT Direction	CT Relevance	CT Direction
Sample of precincts	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Margin of heterogeneity								
Effectiveness × CT Relevance Print News	0.013 (0.065)		-0.273*** (0.070)		0.021 (0.024)		0.011 (0.029)	
Earmarking × CT Relevance Print News	-0.027 (0.069)		-0.313*** (0.089)		-0.042 (0.027)		-0.029 (0.027)	
Effectiveness × CT Direction Print News		-0.016 (0.067)		0.285*** (0.070)		-0.021 (0.026)		-0.011 (0.031)
Earmarking × CT Direction Print News		0.028 (0.074)		0.329*** (0.092)		0.047 (0.028)		0.032 (0.028)
Baseline × CT Relevance Print News			-0.286*** (0.082)				-0.007 (0.037)	
Baseline × CT Direction Print News				0.301*** (0.087)				0.007 (0.039)
Effectiveness	-0.005 (0.029)	-0.006 (0.028)	0.057 (0.038)	0.054 (0.036)	-0.007 (0.011)	-0.006 (0.011)	0.002 (0.015)	0.002 (0.015)
Earmarking	0.016 (0.031)	0.015 (0.031)	0.078 (0.048)	0.074 (0.046)	0.026** (0.012)	0.027** (0.012)	0.025 (0.015)	0.026 (0.015)
Baseline			0.062 (0.040)	0.059 (0.039)			0.007 (0.019)	0.007 (0.019)
CT Relevance Print News	0.275** (0.114)		0.561*** (0.097)		0.049* (0.026)		0.004 (0.037)	
CT Direction Print News		-0.298** (0.122)		-0.599*** (0.098)		-0.054* (0.027)		-0.003 (0.038)
'Yes' share for I-732					0.432*** (0.058)	0.432*** (0.058)	0.439*** (0.073)	0.439*** (0.073)
Primary (2016) Clinton					-0.071*** (0.016)	-0.071*** (0.016)	-0.066*** (0.014)	-0.066*** (0.014)
Democrat share (Senate 2018)					0.571*** (0.055)	0.570*** (0.055)	0.380* (0.193)	0.381* (0.193)
Republican share (2016)					-0.029 (0.061)	-0.029 (0.062)	-0.153 (0.167)	-0.153 (0.167)
Registered voters					0.000** (0.000)	0.000** (0.000)	0.000** (0.000)	0.000** (0.000)
Male (percent)							-0.000 (0.000)	-0.000 (0.000)
White (percent)							0.001*** (0.000)	0.001*** (0.000)
Commuting by car (percent)							-0.001*** (0.000)	-0.001*** (0.000)
Commuting by public transport (percent)							0.000 (0.001)	0.000 (0.001)
Bachelor degree (percent)							0.002*** (0.000)	0.002*** (0.000)
Median income							-0.000*** (0.000)	-0.000*** (0.000)
Median age							0.001** (0.000)	0.001** (0.000)
Constant	0.208*** (0.049)	0.208*** (0.048)	0.145*** (0.048)	0.148*** (0.046)	-0.063 (0.061)	-0.063 (0.061)	0.074 (0.136)	0.075 (0.136)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Precincts	961	961	7,083	7,083	961	961	7,083	7,083
Adjusted R ²	0.056	0.059	0.273	0.279	0.876	0.876	0.936	0.936

Notes: Standard errors clustered at county level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.1 Analysis of media slant including word-frequency and phrase-frequency models

In the analysis of text by freelancers, we use multiple freelancers and try to minimize the references to the source. In this section, we use two text analysis methods to provide further evidence on the positions of news organizations with respect to coverage of climate change and carbon taxes. The first method is the “wordfish” model, which is a popular text-classification model that estimates a latent “document position” measure, and thus measure the “slant” of coverage in news organizations. The measure is based on the frequency of occurrence of words in each document (Slapin and Proksch 2008). The document position measure can then be used to compare the stance of different organizations based on the documents they produce. For our purpose, we estimate the latent document position of three sets of documents. The first two sets of documents serve as reference. First, we examine reports from the Intergovernmental Panel on Climate Change (IPCC), the United Nations-body tasked with analyzing and condensing the scientific consensus about climate change, as well as its climate-skeptic counterpart, the Nongovernmental International Panel on Climate Change (NIPCC). For the IPCC we access reports from 1992 to 2018, for the NIPCC we access reports from 2009 to 2019. Second, we estimate the position of several reports concerning the effectiveness of carbon taxes (or carbon pricing more in general), such as the Stern-Stiglitz report from the High Level Commission on Carbon Prices (Stiglitz et al. 2017) and other similar reports calling for a broader use of carbon taxes.

The third set of documents, to which we compare the first two sets, consists of

the text selected by freelancers in Stage 1 for CNN and Fox News. That is, we focus on articles relevant to the effectiveness of carbon taxes.

Figure C.3 shows that the coverage on Fox News is more closely related to coverage more skeptical of carbon taxes and of the need to address climate change (as represented by the NIPCC reports), while CNN coverage is closer to reports that argue for the need to have carbon taxes and to address climate change (as represented by the IPCC reports).

The second approach uses phrase frequency analysis to measure the “slant” of coverage in news organizations, following the approach of Beattie (2020), which in itself builds upon the seminal work of Gentzkow and Shapiro (2010). While the wordfish approach compares relative frequencies of single words, the phrase frequency approach calculates the relative frequency of two-word phrases between documents. The comparison is done using a normalized measure of slant based on the relative frequencies of these two-word phrases calculated at the document level. We conduct this analysis for cable news and local newspapers in the state of Washington. Our reference texts consist of one of three sets of documents: (i) IPCC and NIPCC reports covering climate change, or (ii) reports published by organizations that are for or against the effectiveness of carbon taxes as a policy instrument, or (iii) articles published by other sources that are clearly for or clearly against the effectiveness of carbon taxes as a policy instrument.

Figure C.4 plots the coefficients documenting how closely each news organization is correlated with base texts that argue for action on climate change and the need for carbon taxes (positive coefficients) or against action on climate change and the

Figure C.3: Slant of news coverage: Wordfish model comparisons of text from cable news and IPCC/NIPCC reports and carbon tax reports

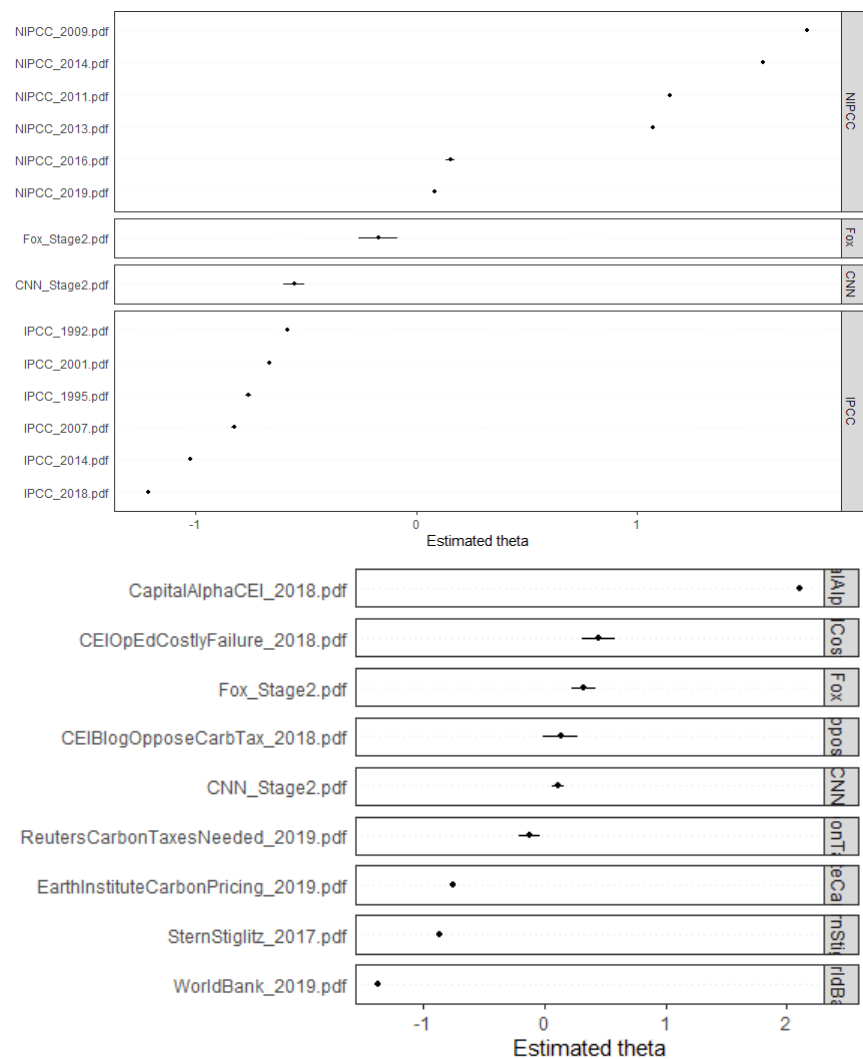
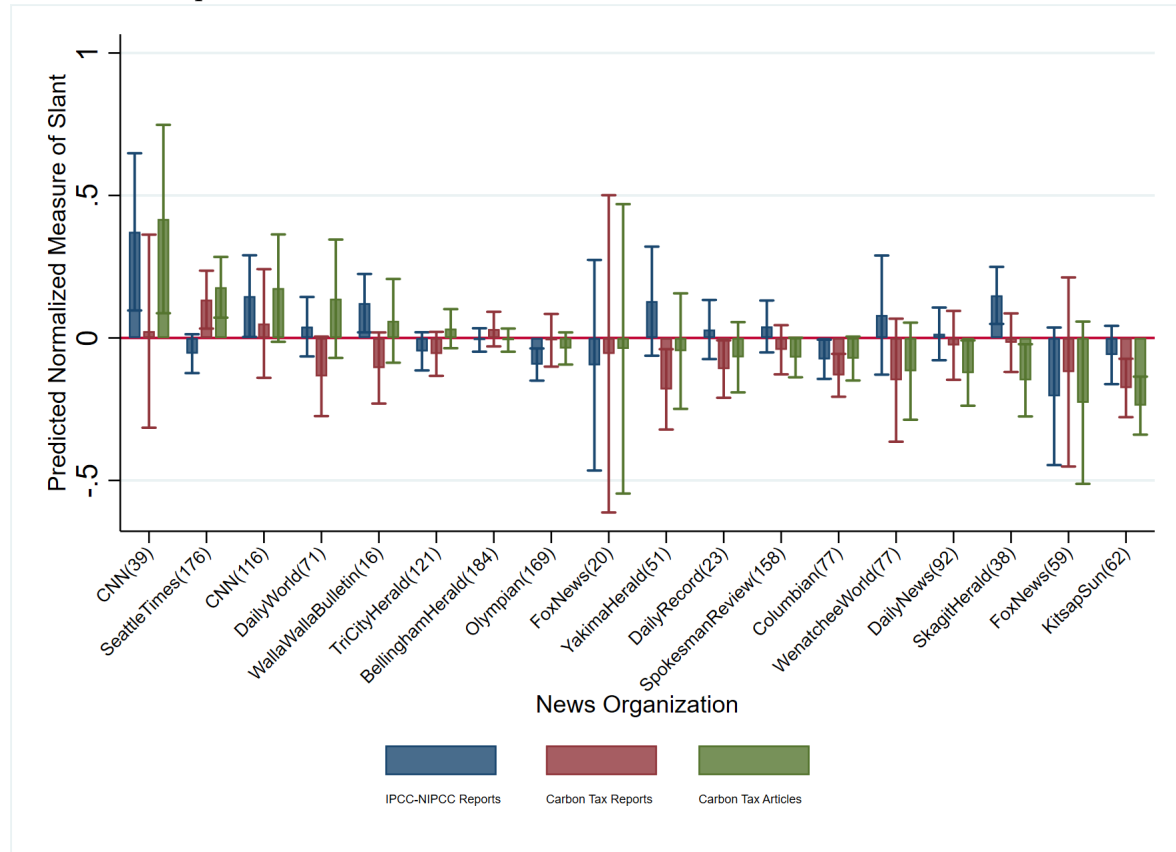


Figure C.4: Slant of news coverage: media organizations compared to IPCC/NIPCC, carbon tax reports and news articles



need for carbon taxes (negative coefficients).

From Figure C.4, we have two main findings. First, there is a difference in coverage of carbon taxes between CNN (CNN(39) that includes the 39 segments that mention carbon taxes more than once and CNN(116) that includes the 116 segments that mention carbon taxes at least once) and Fox News (FoxNews(20) that includes the 20 segments that mention carbon taxes more than once and FoxNews(59) that includes the 59 segments that mention carbon taxes at least once). The slant of news

coverage from CNN is closer to reports and articles that argue in favor of the need for carbon taxes, while the slant of news coverage from Fox News is closer to reports and articles that are more skeptical about the need for carbon taxes. Second, there is a lot of variation in the slant of local news outlets, with no clear patterns emerging.

Table C.33: Field experiment: heterogeneous effects based on slant of news coverage

Outcome	Share of 'yes' votes			
	IPCC and NIPCC reports	Carbon tax reports	Carbon tax articles	All
Slant comparison base texts	(1)	(2)	(3)	(4)
PhraseSlant_ClimateChange	-0.789 (1.870)			
Effectiveness	-0.001 (0.013)	0.003 (0.004)	-0.002 (0.006)	-0.001 (0.007)
Earmarking	-0.002 (0.009)	0.008* (0.004)	-0.000 (0.004)	-0.004 (0.005)
Effectiveness x PhraseSlant_ClimateChange	-0.607 (2.455)			
Earmarking x PhraseSlant_ClimateChange	-2.060 (1.846)			
PhraseSlant_CarbonTaxReports		1.605 (2.066)		
Effectiveness x PhraseSlant_CarbonTaxReports		-0.694 (1.446)		
Earmarking x PhraseSlant_CarbonTaxReports		-4.400*** (1.219)		
PhraseSlant_CarbonTaxArticles			-0.084 (0.901)	
Effectiveness x PhraseSlant_CarbonTaxArticles			-0.888 (1.052)	
Earmarking x PhraseSlant_CarbonTaxArticles			-2.065** (0.895)	
PhraseSlant_Average				0.066 (1.902)
Effectiveness x PhraseSlant_Average				-1.125 (1.897)
Earmarking x PhraseSlant_Average				-4.089** (1.509)
'Yes' share for I-732	0.429*** (0.057)	0.425*** (0.055)	0.432*** (0.057)	0.432*** (0.057)
Primary (2016) Clinton	-0.071*** (0.015)	-0.072*** (0.016)	-0.070*** (0.016)	-0.070*** (0.015)
Democrat share (Senate 2018)	0.565*** (0.055)	0.571*** (0.056)	0.550*** (0.058)	0.552*** (0.057)
Republican share (2016)	-0.048 (0.062)	-0.044 (0.070)	-0.067 (0.071)	-0.066 (0.071)
Registered voters	0.000* (0.000)	0.000* (0.000)	0.000* (0.000)	0.000* (0.000)
Constant	-0.030 (0.060)	-0.030 (0.064)	-0.011 (0.065)	-0.011 (0.064)
Precincts	961	961	961	961
Adjusted R^2	0.875	0.875	0.875	0.876

Notes: Standard errors clustered at county level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

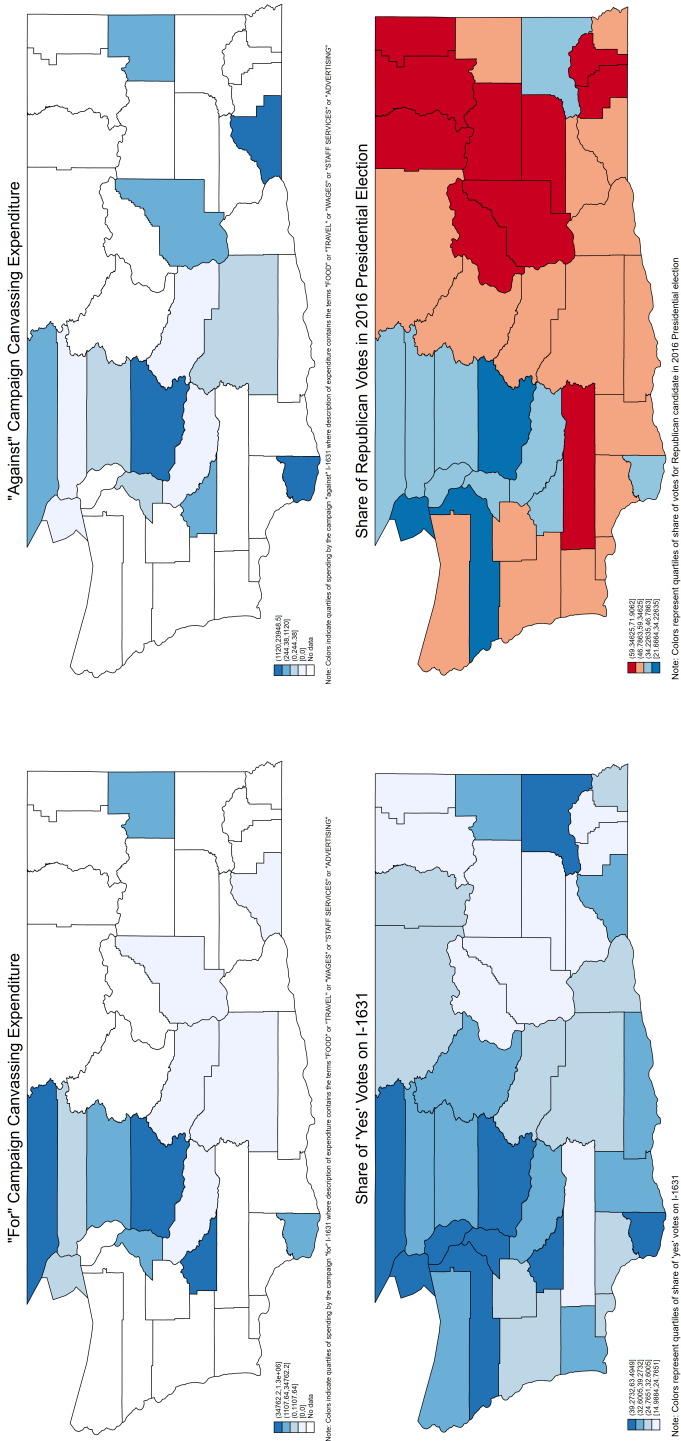
D Campaign spending for I-1631 across space

We also verify that the effects of media exposure identified above are not driven by the fact that the “for” I-1631 camp might have reached out mostly to voters in Republican-leaning precincts, potentially making them especially receptive to our informational treatment. To this end, we proceed as follows. We access data on campaign spending from Washington state’s Public Disclosure Commission (PDC) database. The database contains information on canvassing expenses incurred by (and in-kind contributions to) registered political entities. The database allows us to link some of the expenses to a given area of the state.

Each expenditure item contains a non-standardized, textual description of the type of expenditure (food, travel, advertising, etc.). From this database, we obtain all publicly listed expenditures “for” or “against” I-1631. The full database indicates that total spending by the “for” campaign was at \$16.4 million, while the “against” campaign spent \$31.2 million. Since we are interested in geographical differences in campaign spending in Washington state, we focus our analysis on all bills paid to entities registered within the state that have a description containing any combination of the words “FOOD”, “TRAVEL”, “TV”, “STAFF SERVICES”, “WAGES”, “STAFF TIME”, or “ADVERTISING”. According to this criteria, the total spending on the ground by the “for” and “against” campaigns is at \$1,318,673 and \$38,848, respectively. We look at canvassing expenditures broadly, according to the criteria identified earlier, and also specifically at expenditures on advertising in local newspapers that can be traced from the description of expenditure items. We are looking at these expenditures because we are interested in specific local targeting,

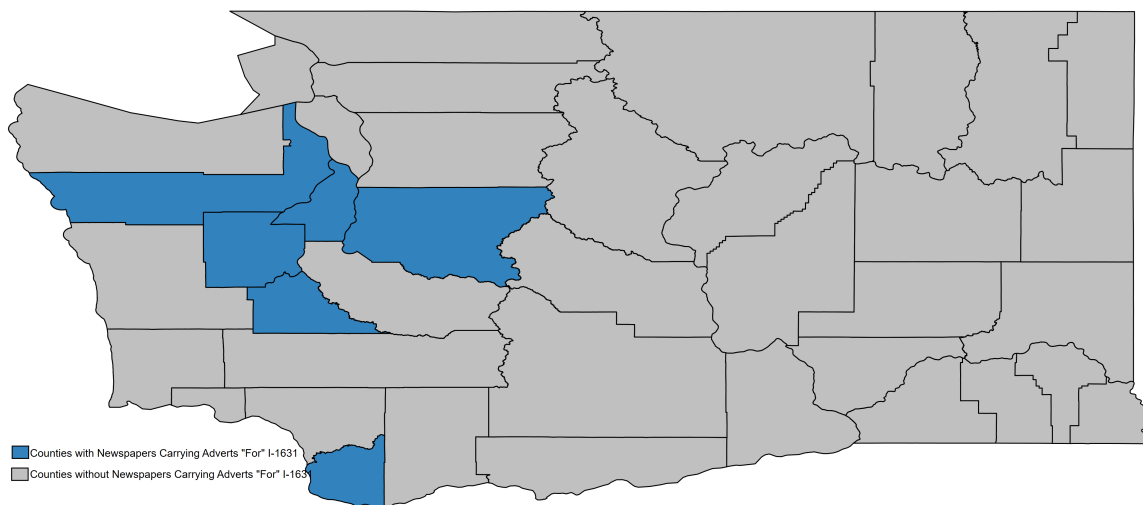
as an indication of a particular predisposition of the “for” campaign (relative to the “against” campaign) to try to mobilize votes, and possibly align voters’ preferences with theirs, in some specific areas. To this end, we first show the overall geographical spending pattern by the “for” and “against” campaigns at the county level in Figure D.5, against the share of ‘yes’ votes for I-1631 and the share of votes for Republicans in the 2016 Presidential election. The spending pattern reveals that the “for” campaign’s local canvassing expenditures were relatively more concentrated in areas that had a higher share of votes for the initiative, and a lower share of votes for the Republican party.

Figure D.5: Distribution of local canvassing expenditures by the “For” and “Against” campaigns and vote shares



We then look in particular at advertisement in local newspapers. Turning to advertising in local newspapers, the PDC dataset shows that the campaign “for” I-1631 purchased advertisements in local newspapers, including *The Seattle Times* and *The Stranger*, which are distributed widely in King county, *The Columbian*, which is distributed in Clark county, *The Olympian*, which is distributed in Thurston county, and *The Kitsap Sun*, which is distributed in Kitsap, Jefferson, and Mason counties. The coverage is shown in Figure D.6.

Figure D.6: Distribution of advertising by the “For” campaign in print newspapers



Notes: The “For” campaign registered spending on purchasing advertising in the *The Seattle Times* and *The Stranger* (King county), *The Columbian* (Clark county), *The Olympian* (Thurston county), and *The Kitsap Sun* (Kitsap, Jefferson, and Mason counties). Local newspaper expenditures by the “Against” campaign cannot be traced to specific newspapers from this dataset since descriptions of spending by the “Against” camp are not detailed in the same way as that in the “For” campaign.

We see that the campaign supporting I-1631 purchased advertising in newspapers distributed in counties that expressed above-average support for I-1631 compared to counties that had lower levels of support for the initiative. Overall, the available

information about campaign spending points towards spending being disproportionately targeted towards Democratic-leaning areas. Hence, we conclude that campaign spending does not contribute to explaining the heterogeneity in treatment effects observed in Section 5.3.

E Survey instrument

This section provides the full survey instrument:

“This survey focuses on Initiative 1631, the Washington Carbon Emissions Fee and Revenue Allocation Initiative. We are interested in your opinions. This survey may allow us to better understand people’s preferences, and to provide better recommendations to policy-makers. This is a public opinion survey and there are no right or wrong answers. This survey is completely anonymous. We will not ask any or collect any identifying information. Thus, no one will be able to identify you or your answers. Initiative 1631 aims to reduce pollution by investing in clean air, clean energy, clean water, healthy forests, and healthy communities with revenues from a fee on large emitters based on their pollution. The tax would start at \$15 per ton of carbon dioxide in 2020, and increase by \$2 plus inflation every year from 2021, until the State’s greenhouse gas reduction goal has been met. Your participation in this survey is voluntary. You may refuse to take part in the research or exit the survey at any time without penalty. You are free to decline to answer any particular question you do not wish to answer for any reason. You will receive no direct benefits from participating in this research study. Your responses may help us understand attitudes about climate policies. There are no foreseeable risks involved in participating in this study other than those encountered in day-to-day life.

Do you agree to participate in this study?

- Yes
- No

Screening questions (SQ):

[SQ1] How old are you?

- Numerical input [Allow 10 to 200; terminate if below 18 or above 110]
- Prefer not to answer

[SQ2] What is your gender?

- Male
- Female
- Other
- Prefer not to answer

[SQ3] Which State do you live in? [Standard list of U.S. states; plus option for abroad]

[SQ4] Are you a registered voter in Washington State?

- Yes
- No

[Terminate if answer is “No”]

[Treatment randomization]

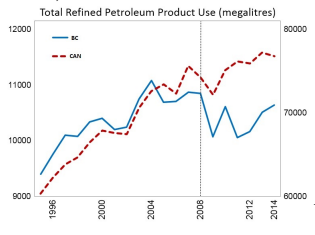
[Baseline]

Washington Initiative 1631 is on the ballot this November 6th. What do you think? Your input can help policymakers.



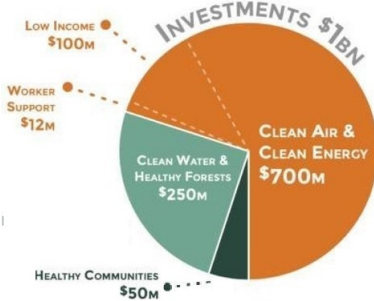
[Effectiveness]

Initiative 1631 is on the ballot. When British Columbia introduced a carbon tax in 2008, emissions of CO₂ decreased by up to 10%.



[Earmarking]

Initiative 1631 is on the ballot. Unlike the 2016 carbon pricing referendum, it would invest about \$1 billion in clean air, water, energy, and communities.



[Q1a] What will be or what was (given early voting) your vote on Initiative 1631?

[Randomize order of pairs of yes and no]

- Already votes “Yes”
- Already voted “No”

- Will vote “Yes”
- Will vote “No”
- Do not know yet what I will vote

[Q1b] How certain are you of your decision? [Only for those who responded “Will vote “Yes”” or “Will vote “No”” to Q1a; 7-point scale where 1 = very uncertain and 7 = very certain]

[Q2a] On November 8, 2016, voters in Washington State rejected a carbon pricing proposal, Initiative 732, also known as “Washington Carbon Emission Tax and Sales Tax Reduction”. Did you vote? [Randomize order of yes and no]

- Yes
- No
- Do not remember
- Did not vote
- Was not eligible to vote

[Q2b] Did you vote in favor (“yes”) or against (“no”) Initiative 732? [Do not ask this question if answer to Q2a is different from “Yes”; randomize order of yes and no]

- Yes
- No
- Do not remember

The current Initiative 1631 and the 2016 Initiative 732 differ in key areas. The next set of questions concern the current Initiative 1631.

[Q3a1] How would you rate the current Initiative 1631 on these five outcomes?

[Options are “Poor”, “Neutral” or “Good”]

- Reducing greenhouse gas emissions
- Reducing local pollution
- Impact on disadvantaged households
- Impact on businesses
- Ability to generate funds for environmental protection

The next set of questions concern the 2016 Initiative 732.

[Q3a2] How would you rate the 2016 Initiative 732 on these five outcomes? [Op-

tions are “Poor”, “Neutral” or “Good”]

- Reducing greenhouse gas emissions
- Reducing local pollution
- Impact on disadvantaged households
- Impact on businesses
- Ability to generate funds for environmental protection

[Q3b] How important is each of these five outcomes for your decision to support or oppose a policy initiative? [Options are “Low”, “Average” or “High”]

- Reducing greenhouse gas emissions
- Reducing local pollution
- Impact on disadvantaged households
- Impact on businesses
- Ability to generate funds for environmental protection

[Q7] Recently, you may have noticed that global warming has been getting some attention in the news. Global warming refers to the idea that the world's average temperature has been increasing over the past 150 years, may be increasing more in the future, and that the world's climate may change as a result. What do you think?

Do you think that global warming is happening?

- Yes
- No
- Don't know

[Q8a] How sure are you that global warming is happening? [Q8a if Q7 = "Yes"]

- Extremely sure
- Very sure
- Somewhat sure
- Not at all sure

[Q8b] How sure are you that global warming is not happening? [Q8b if Q7 = “No”]

- Extremely sure
- Very sure
- Somewhat sure
- Not at all sure

[Q9] Assuming global warming is happening, do you think it is . . . [Order of responses 1-2 is randomly rotated]

- Caused mostly by human activities
- Caused mostly by natural changes in the environment
- Other
- Neither because global warming isn’t happening

[Q10] How worried are you about global warming?

- Not at all worried
- Not very worried
- Somewhat worried
- Very worried

[Q11] This survey relates to climate policy and hence we ask questions about climate change. Please select “agree” to show that you are paying attention: [Test question; terminate if respondent answers anything other than “agree”]

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree
- Strongly disagree

[Q12] What is the approximate annual gross (before tax) income of your household?

- \$13,500 or less
- \$13,501 - \$24,000
- \$24,001 - \$34,500
- \$34,501 - \$45,500
- \$45,501 - \$59,000
- \$59,001 - \$74,500
- \$74,501 - \$94,000
- \$94,001 - \$121,000
- \$121,001 - \$170,500

- \$170,501 or more
- Don't know
- Prefer not to answer

[Q13] How many people are there currently in your household including yourself?

- Numerical input [Allow 1-99]
- Prefer not to answer

[Q14] What is your highest level of education?

- No formal education
- Primary school
- Secondary school
- Higher education (university, college or equivalent)
- Prefer not to answer

[Q15] What is your employment status?

- Employed
- Self-employed
- House person or carer
- Student

- Retired
- Unemployed
- Unable to work (e.g. disability)
- Other
- Prefer not to answer

[Q16] If there was a House of Representatives election today, which party would you most likely vote for? [Order of responses 1-2 is randomly rotated]

- Democratic Party
- Republican Party
- Other
- Don't know
- Prefer not to answer"

F Instruments for freelancers analyzing text segments on Upwork

F.1 Stage 1 - Selecting relevant news segments

“The task consists of copying text from pdf documents that satisfy the criterion below and inputting them into an Excel sheet. This process comprises three steps:

Step 1. Read articles that mention carbon taxes which we provide.

Step 2. Select segments of the articles that discuss how carbon taxes may or may not affect greenhouse gas emissions. Importantly, you need to keep in mind that we are equally interested in text saying either (i) carbon taxes are ineffective, for instance by suggesting that while some behavioral change may occur, most people will not change behavior, or (ii) praising carbon taxes as being an effective way to reduce pollution. However, do not include text segments mentioning carbon taxes in contexts unrelated to their effectiveness as just described. Keep in mind that articles may use other terms to refer to carbon taxes, including carbon levy, CO2 taxes, pollution tax, carbon pricing, emissions trading schemes, cap and trade, emissions trading are some examples. You should include these cases. Also, keep in mind that some articles may refer to how carbon taxes affect or do not affect pollution in general terms, local air pollution, some specific pollutants, or help or not help to tackle climate change (by, implicitly, decreasing emissions). Include these cases as well.

Step 3. Copy and paste or write the relevant segments into an Excel sheet along with the filename of the article.

-- If an article contains no discussion of how carbon taxes may or may not affect greenhouse gas emissions, please write N/A in the corresponding row.

-- If there is only one specific segment discussing how carbon taxes may or may not affect greenhouse gas emissions, please copy and paste or write it in the corresponding row.

-- If there are multiple segments discussing how carbon taxes may or may not affect greenhouse gas emissions, please copy and paste or write all of them in separate rows.”

F.2 Stage 2 - Coding slant of coverage in relevant news segments

“The task is organized as follows. You will be given links to a survey with approximately 100 text segments and you will need to categorize each of them. To categorize a segment, you will need to answer a set of predefined questions in the survey. Please make sure to read the full content of each text segment before answering the questions.

In particular, we will ask you to

- (i) determine whether a text segment talks about the effect of carbon taxes on pollutant emissions (some text segments may not qualify);
- (ii) if it does, determine how effective the segment considers carbon taxes to be;
- (iii) and what arguments the segment uses in this respect.

We will review your responses and you will be paid after you complete all questions in the survey and send us your completion ID. We may also provide feedback

based on the quality of your work.

Survey questionnaire

[NEWS SEGMENT SELECTED IN STAGE 1 IS DISPLAYED AT THE TOP FOR EACH SURVEY QUESTION]

Q2.1 Does the text segment displayed here talk about the effect of carbon taxes on pollutant emissions? Note that segments may use other terms to refer to carbon taxes or similar policies, including carbon levy, CO2 taxes, pollution tax, carbon pricing, emissions trading schemes, cap and trade, emissions trading are some examples. You should consider these cases. Also keep in mind that some segments may refer to how carbon taxes affect or do not affect pollution in general terms, local air pollution, some specific pollutants, or help or not help to tackle climate change (by decreasing emissions). Consider these cases to be relevant as well.

- Yes (1)
- No (2)
- Do not know (3)

Q2.2 The text displayed here indicates that:

- Carbon taxes increase pollutant emissions (1)
- Carbon taxes decrease pollutant emissions (2)
- Carbon taxes do not affect pollutant emissions (3)

Q2.3 Which of the following best describes the content of the text? (Increase/decrease/do not affect; displayed conditional on response to Q2.2)

- Explicitly and quantifiably supports the idea that carbon taxes [increase/decrease/do not affect] pollutant emissions (1)
- Explicitly but not quantifiably supports the idea that carbon taxes [increase/decrease/do not affect] pollutant emissions (2)
- Implicitly supports the idea that carbon taxes [increase/decrease/do not affect] pollutant emissions (3)
- None of these (4)

Q2.4 Which of the following sentiments/arguments do you think most closely represents the idea in the text? Select all that apply.

[FOR “INCREASE”]

- As the price of gas or other carbon-intensive goods increases due to the tax, the quantity sold also goes up. (1)
- People end up driving more and firms pollute more in response to a carbon tax. (2)
- Carbon taxes lead to a shift of polluting production to other countries, leading to an increase in global emissions. (3)
- Producing clean goods like solar panels or electric cars also lead to emissions. (4)

- Other argument (please specify) (5) _____
- No specific argument provided (6)

[FOR “DECREASE”]

- Carbon taxes change the behavior of consumers and firms towards cleaner practices (such as driving less, using public transit, investing in energy-efficient technology, using solar or wind energy etc.) (1)
- Carbon taxes decrease emissions when tax revenues are used to sponsor environmental programs, e.g. supporting the deployment of renewable energy technologies (2)
- Carbon taxes increase the relative price of carbon-intensive goods, which may incentivize people and firms to change their behavior towards cleaner practices (3)
- Other argument (please specify) (4) _____
- No specific argument provided (5)

[FOR “DO NOT AFFECT”]

- Some people may change behavior in response to a carbon tax, but most people will still drive and take flights like before (1)
- Carbon taxes lead to a shift of polluting production to other countries, so that global emissions remain unchanged.(2)

- Other argument (please specify) (3) _____
- No specific argument provided (4)”

G Examples of text segments

G.1 CNN

Fareed Zakaria GPS, CNN, 7 February 2016. (CNN article #6). “Sweden, one of the first nations to adopt a tax on carbon in 1991, has seen its GDP increase almost 60 percent since that time while its emissions have dropped 23 percent, according to government figures. Denmark has been taxing carbon since 1992 and its economy has also done very well with emissions falling.....”

...“Carbon pricing isn’t painless, of course. It adds dollars to your heating bill, your electric bill, and at the pump. But economists generally like a carbon tax as a solution for climate change. That’s why the arch-conservative, George Schulz, former secretary of state and Treasury, former dean of the Chicago Business School, argues forcefully for one.....”

...“A carbon tax is simple and doesn’t require complicated, expensive regulations like the ones the United States now has, one economist points out. It allows customers a lot of choice in how they live with it. Some might buy a more fuel-efficient car; others might use more public transportation. And with oil prices at historic lows, introducing one now would be relatively painless.....”

...“Since the tax went into effect, British Columbia’s emissions have decreased significantly while its economy was on par with its neighbors, with the lowest personal income tax rate in the nation, according to The Economist.”

G.2 Fox News

The Journal Editorial Report, Fox News, 12 January 2019. (Fox Article #13, page 7)

“PAUL GIGOT, FOX HOST: A new report out this week shows carbon emissions rising in the United States in 2018 after three years of decline. That news renewing calls for a plan to reduce greenhouse gases. And at least one idea is getting buy-in from some Republicans. Former GOP Secretaries of State James Baker and George Schultz recently unveiled a climate plan that calls for taxes on carbon emissions and returning the revenue as a dividend to Americans. Could this be the year that Congress enacts a carbon tax? Is it a good idea? Let’s ask Mark Mill. He’s a senior fellow at the Manhattan Institute and a partner in Cottonwood Venture Partners, an energy tech venture fund. So, Mr. Mills, good to have you here. You wrote for the “Wall Street Journal” this week a piece that did not really support a carbon tax. But I want to ask you, for regular viewers out there, when people talk about a carbon tax, what do they mean? MARK MILLS, SENIOR FELLOW, MANHATTAN INSTITUTE & STRATEGIC PARTNER, COTTONWOOD VENTURE PARTNERS: That’s actually the critical question, isn’t it? Everything in our society, everything, directly or indirectly, uses hydrocarbon, oil, coal or natural gas. GIGOT: Right. MILLS: Taxing the hydrocarbon, the carbon in that, means that you’re taxing, for the first time, I think, in American history, everything. So it levies a tax on all activities, all manufacturing, all processes. Everything uses energy and all of our energy is directly or indirectly linked to hydrocarbons, 85 percent directly, and the other 15 percent, making windmills, making solar arrays. You use hydrocarbons to make

them. You use hydrocarbons to operate them, to manufacture all goods. We use hydrocarbons. It's a tax on everything. GIGOT: So that would -- this tax would filter out, in through the whole economy, the amount you pay for gasoline, the amount you pay for electricity, the amount you pay for anything that is made with carbon energy power. OK. So you don't like the idea. (LAUGHTER) Well, look, there are a lot of economists, free market economists, who say (CROSSTALK) GIGOT: -- look, if you tax something, you get less of it. We don't like carbon emissions. We do like income. We do like investment. Why don't you swap? Let's tax carbon emissions and we'll reduce the tax rate on income and investment? What's wrong with that trade? MILLS: It sounds good. Except it fails on the two counts that are claimed by all the advocates of such a tax. It fails, first, because any politically possible, and I think any possible level of taxation, would have no measurable effect on the consumption of the thing we're taxing. We know this because we've already done the experiment. The kind of tax that caused the Yellow Vest Revolution in France, for Macron, was a 5 percent increase on the cost of diesel fuel. GIGOT: Right. MILLS: We know that when the cost of oil went up 200 percent in the decade prior to the Great Recession, oil demand increased by 10 million barrels per day. So an economist could argue the increase might have been more like 12 million barrels per day instead of 10. My point is, it went up. The point of the tax, they claim it will go down. It would need a 300 or 400 percent tax to drive oil consumption down. It fails there. It fails on the other claimed goal, which is to repurpose the money to get more green energy, more windmills and solar power. It would get more of that but it wouldn't make a significant difference to the U.S. energy economy. You only

get 3 percent of our total energy in America from wind and solar, and this is after a couple of decades and a couple hundred billion dollars of subsidies. GIGOT: All right. MILLS: You could double that, it doesn't make much difference. GIGOT: I want to ask you about another tradeoff that a lot of supporters argue in favor of the carbon tax. That is, it's a more efficient way if you want to reduce carbon emissions, than regulation, than the Obama clean power plan. Let's impose a carbon tax and we'll get rid of all the regulation and that's a much better way to do it. MILLS: First of all, I think it's naive to think that we'll end up in the politics of horse trading a tax for no regulation. GIGOT: Right. MILLS: I think that's -- I'm a physicist. I'm a simple guy. I think there's a law of physics here. That won't happen. (LAUGHTER) GIGOT: OK. MILLS: It just won't happen. But let's say you're naive enough to believe it might happen, which you know my opinion on that. Look, it fails the test. You're not going to get a reduction in oil or hydrocarbon or gas use from this tax. It won't go down. The whole premise was that the market is more efficient, if I tax it, people use less of it. Of course, in theory, there's some reduction in the rate of increase of demand for oil and gas and coal. That's what we're reducing. To get an absolute reduction in the use of oil, gas and coal, you have to kill the economy. We have a booming economy and, shazam, we have carbon emissions going up because the economy's doing better. You can reduce carbon dioxide emissions. It's called a recession. We did that in 2008. The east block managed to do that. It's so far proven that the only effective way to reduce carbon dioxide emissions. I don't think that gets a lot of political support. GIGOT: All right, thank you, Mark Mills. Appreciate you coming in. MILLS: Thank you."

The Ingraham Angle, Fox News, 17 December 2018. (Fox Article #16, page 14-15)

“MORANO: They are rejecting it in France. It’s spreading across Europe. Brazil’s new government, the foreign minister called it a Marxist hoax. Canada has a carbon tax rebellion. Donald Trump was criticized by French President Emmanuel Macron by promoting nationalism and pulling out of the U.N. Paris agreement. And then a week later, Paris is ablaze. What Brad tries, he tries to call it poison weather. You can look at everything he just mentioned, wildfires, droughts, hurricanes. On every climate timescale, 50 years and beyond, there are either no trend or declining. That’s how you look at the science. California had much worst droughts in previous centuries. Global drought is setting no trends. And this is even buried in the U.N. reports. They can’t even hype that kind of thing. INGRAHAM: But then you see the icebergs and big pieces of ice coming off, and you see the polar bear really lonely on a piece of ice. And I start to tear up and I cry. I’m sure a lot of other people cry because it looks really awful. And we are changing the planet, no doubt about it. Correct, we are changing the planet? MORANO: Right, but if we actually -- INGRAHAM: Species are being wiped out because we do dumb things. MORANO: Yes, but that’s one thing we know how to do now is save species. One of the biggest success stories is we know even now how to, when you do logging and other -- instead of clear cutting vast swaths, they do what they call sustainable logging and they cut different sections. But the bottom line here is this, the carbon tax would actually increase emissions, because if you hamstring the developed world, the United States, Canada, western Europe, all that’s going to do is move industry over to China, India,

places without that. There is actually peer reviews studies and other studies showing that emissions for decades to come will go up. So even if that's your goal, it's the wrong way to go."

The Journal Editorial Report, Fox News, 14 October 2018. (Fox Article #26, page 11)

"FINLEY: This is miss to Exxon-Mobil who donated \$1 million to campaign to promote a carbon tax, as well as Bill Gates, who endorsed the carbon tax in a referendum in Washington State. In Washington, it would start at \$15 per ton of carbon. The IPCC noted this week that you would need a carbon tax of \$5,500 to do anything to reduce climate change. This is just another redistribution mechanism for our electric-car subsidies, et cetera."