# Reaching across the aisle: Does affective polarization hinder grassroots climate mobilization?

Lucy Page
MIT (Job Market Paper)

Hannah Ruebeck
MIT

November 2023

**Link to current version (updated frequently)**

**Abstract**

Citizens may actively recruit others to join in grassroots political activism, and in doing so they may shape legislators' perceptions of constituent beliefs. If citizens work to spread grassroots action, do they differentially seek to build broad, bipartisan coalitions or to recruit others like them? Mobilizing bipartisan citizen action could help to build bipartisan voting coalitions in Congress, but record-high affective polarization may impede cross-party cooperation among citizens. In online experiments with 25,000 participants, we connect Democrats with other Americans across the political spectrum (all of whom believe climate change is human-caused) to understand whether and how they work to recruit others to advocate for climate policy. Democrats are motivated to engage others in climate action—they are 10 percent more likely to email Congress when doing so allows them to invite others to act. Even while the vast majority of Democrats say that a bipartisan climate movement would be more effective than a liberal-only movement, however, they are 27% more likely to invite other liberals than conservatives to email Congress. This gap does not arise from Democrats' own distaste for engaging with counter-partisans, but rather can be explained by their correct beliefs that their invitation will have only about half as much impact on conservatives' action. Nonetheless, these beliefs are shaped by second-order affective polarization: Democrats believe that conservatives would respond more to invitations that hid their own party leanings.

*Page: PhD candidate, MIT Economics, lucypage@mit.edu (corresponding author). Ruebeck: PhD candidate, MIT Economics, hruebeck@mit.edu. We are grateful for generous advising by Frank Schilbach, Esther Duflo, Ben Olken, and Abhijit Banerjee. We also thank Rohini Pande, Daron Acemoglu, Kelsey Jack, Lisa Ho, and the participants of MIT's behavioral and applied microeconomics lunches for their helpful comments. This project was supported by the JPAL King Climate Action Initiative, the George and Obie Shultz Fund at MIT, and Esther Duflo's research fund. We are also supported by the National Science Foundation Graduate Research Fellowship under Grant No. 1745302. The pre-registration for this project can be found at https://www.socialscienceregistry.org/trials/11250. IRB approval for the project was obtained from the Massachusetts Institute of Technology Committee on the Use of Humans as Experimental Subjects (Protocol #2212000839).

# 1 Introduction

Every year, large shares of Americans contact politicians at local, state, and national levels: in 2018, 23% of Americans said they'd done so in the last year, while 40% said they'd done so in the last five years (Oliphant, 2018). Constituent lobbying changes legislators' behavior (Bergan, 2009; Butler and Nickerson, 2011; Bergan and Cole, 2015; Broockman and Skovron, 2018), so the composition of those participating in grassroots political action could crucially affect government action (Broockman and Skovron, 2018). Citizens may play key roles in shaping this composition. Long-standing research in sociology (e.g. McAdam (1986); Gould (1991, 1993); Opp and Gern (1993); McAdam and Paulsen (1993)) and more recent causal work in economics and political science (Nickerson, 2008; Bond et al., 2012; González, 2020; Bursztyn et al., 2021) find that political and social movements spread through networks, so politically-engaged citizens may have power to shape political outcomes both through their own advocacy and by recruiting others into the grassroots political coalition.

Do citizens actively try to engage others in political advocacy? If so, what kind of political coalitions do they try to build? In particular, do they differentially attempt to mobilize broad, bipartisan citizen action or to recruit others like them? On the one hand, recruiting bipartisan citizen action may more effectively promote policy change by contributing to bipartisan legislative support in Congress. Split-party governmental control is the norm in the US—a single party controlled the presidency, Senate, and House for only 16 of the last 52 years—and passing legislation typically requires minority-party votes even under single-party control (Curry and Lee, 2020).[1] Then, achieving long-term policy progress on any issue likely requires building a cross-party legislative coalition to support it. On the other hand, record-high affective polarization (Iyengar and Krupenkin, 2018; Boxell et al., 2022)—dislike or distrust of those across the political aisle—could pose a substantial friction to recruiting bipartisan citizen action. Prior work documents that Americans favor co-partisans over counter-partisans in real-stakes choices in a range of domains, from job callbacks (Gift and Gift, 2015) to romantic relationships (Klofstad et al., 2013; Huber and Malhotra, 2017; Iyengar et al., 2018).

We examine these questions in the context of climate change, one of the most polarizing political issues in the US (Atske, 2020). Climate activism skews heavily to the political left (Leiserowitz et al., 2023), but a substantial minority of Americans on the political right could in theory be recruited to act: 50% or more of GOP-leaners support policies such as tougher restrictions on power-plant emissions and tax credits for carbon capture and storage (Tyson, 2021), and 19% of moderate Republicans say they would contact government officials about climate change (Leiserowitz et al., 2023). We examine whether and how Democrats, majority members of the climate movement, work to build this coalition.

A key challenge in identifying whether citizens differentially act to build political movements along or across party lines is that social networks are politically segregated, so activists likely have differential access to co-partisans (Jones et al., 2022). To solve this challenge, we recruit 25,000 participants from social media and online panels to an online survey in which we exogenously connect Democrats with other Americans across the political spectrum. Within this basic social network, we provide Democrats with a series of

[1] Notable single-party legislative successes include the 2010 Affordable Care Act, the 2017 Tax Cuts and Jobs Act, and the 2022 Inflation Reduction Act. However, 90% of laws that passed the House and 75% that passed the Senate since 2011 got positive votes from at least half of minority party legislators, with similar rates on landmark bills and minor legislation (Mayhew 2023).

opportunities to recruit others for climate advocacy.

We create these influence opportunities between two waves of participants. First, we invite a Wave 1 of "influencers," all of whom belong to the Democratic party, to email Congress about climate change via a form embedded in an online survey. Later, we will recruit a second wave of study participants (Wave 2, or the "targets"), who will also have opportunities to email Congress via our form. Across several experimental designs, we randomly provide some Wave-1 Democrats with costly opportunities to try recruiting Wave-2 targets to join them in emailing Congress by passing on simple, semi-anonymous invitations (Figure 1a). Crucially, we randomly match Wave-1 influencers with Wave-2 targets who lean towards either the Democratic party (referred to hereafter as "liberals") or the Republican party (referred to hereafter as "conservatives"). We observe detailed data on whether all Wave-1 and Wave-2 participants email Congress about climate change, as well as when and to whom Wave-1 Democrats extend action invitations. Examining Democrats' choices to try recruiting others along and across party lines then provides a detailed picture of whether and to whom they seek to expand the climate movement.[2]

While we focus most of our analysis on Wave-1 Democrats' efforts to influence others, we first establish a key piece of context: Wave-1 participants can indeed influence others to act in this setting, but they have less absolute impact on conservatives. We truthfully and randomly show Wave-1 invitations to Wave-2 targets, allowing us to estimate the invitations' true impact on liberals' and conservatives' climate action. In Wave-2, seeing an action invitation from a Wave-1 Democrat makes liberals (N = 5,027) and conservatives (N = 2,954) 5.8pp (23%) and 2.1pp (25%) more likely to email Congress, respectively.

Democrats internalize these impacts and try to recruit others for climate action. In the *Wave-1 Action experiment* (N = 8,937), we test whether Wave-1 Democrats are more likely to email Congress when doing so lets them invite others to join. We randomize participants between two arms: an "Invitation" arm, who are told that if they email Congress, we will show up to 10 future Wave-2 participants invitations to join them in action (Figure 1a); and a pure control group, who decide whether to email Congress with no mention of others knowing that they did so. Participants in the Invitation arm are 16pp (34%) more likely to start the process of emailing Congress and 16pp (48%) more likely to ultimately do so.

This comparison does not isolate influence motives, however. Because invitations state that the sender emailed Congress, Wave-1 participants may be more likely to email Congress in the Invitation condition in part due to the self- or social-image benefits of telling others that they did so. To cleanly identify whether participants act in order to influence others' political action, we thus randomize one third of Wave-1 to a "Tell after" arm: we will tell up to 10 Wave-2 participants if they email Congress, but only *after* those targets decide whether to email Congress or not. Then, the only difference between those in the Tell after and Invitation groups is that Invitation participants' choice to email Congress or not can affect whether Wave-2 participants do the same; comparing rates of climate advocacy across these groups cleanly identifies whether

[2]We require that all Wave-1 and Wave-2 participants believe that climate change is mostly human-caused, and Wave-1 participants always see targets' political affiliation alongside their climate beliefs. This restriction shuts down the relatively uninteresting possibility that Democrats avoid mobilizing conservatives for climate action simply because they expect them not to believe in anthropogenic climate change. Moreover, while the email is almost entirely customizable, its subject line is fixed and explicitly supports climate action. As much as possible, then, we minimize concerns that Democrats may avoid engaging conservatives in political action because they expect them to email Congress to oppose climate policy. Instead, we are interested in exploring a setting where liberals can choose whether to pursue allied political action from conservatives, but may or may not choose to.

Democrats act to mobilize others. Indeed, influence motives significantly increase action: participants in the Invitation group are an additional 3 pp (6%) more likely to email Congress than those in the Tell-after group.

While Democrats work to build the climate movement overall, they are more likely to try to recruit other liberals than to reach across party lines. We test *whom* Democrats seek to engage in climate advocacy in a second experiment embedded among participants who email Congress in the Wave-1 pure control group. In the *Willingness-to-Pay (WTP) experiment*, we show each participant (N = 1,023) twenty past study participants who will be returning to take a second survey, during which they will have an opportunity to email Congress via our form. One of these returning participants will be randomly matched to see a profile of the WTP participant. For each of the 20 possible matches, we ask WTP participants to make incentivized choices between two options: (1) including that the WTP participant emailed Congress in the profile that the returning match would see, or (2) passing on only a basic demographic profile and making a small carbon-offset donation from our research funds. We randomize whether each possible match would see the WTP participant's profile before or after themselves deciding whether to email Congress; we then identify efforts to engage others in the climate movement by how much more likely participants are to tell matches that they emailed Congress when it can affect their action. Observing 20 binary choices for each WTP participant powers us to test for heterogeneous influence motives by match traits.

When WTP participants do not know what possible matches believe about climate change, they are more than twice as likely to try to mobilize other liberals than to invite conservatives. Showing that all possible matches believe that climate change is mostly human-caused reduces this gap, but Democrats remain 27% more likely to try to mobilize other liberals when these common-ground beliefs are clear. These partisan gaps dwarf differential attempts to influence targets by any other match trait, including influencer-target similarity on any non-political dimension.

In summary, while Democrats try to engage others in political advocacy on climate change, they differentially reach out to other liberals rather than conservatives. Motivated by a theoretical framework, we then use a series of follow-up experiments to decompose the preferences and beliefs that hold back Democrats' efforts to build a bipartisan citizen movement. First, in a sample of Democrats recruited alongside Wave 1 (N = 194), we measure incentivized beliefs about the impacts of invitations. We cannot reject that Democrats' beliefs are correct: they estimate that invitations will make liberals about 6pp more likely to email Congress, while making conservatives only about 3pp more likely to do so.

Additional follow-up experiments suggest that this gap in influence beliefs fully explains the gap in Democrats' efforts to reach out to liberals versus conservatives. First, we recruit a separate sample of 1,083 Democrats and elicit their preferences for emails to Congress from Americans across demographic groups when those emails can be *obtained with certainty*: we ask participants to make incentivized choices, for a range of demographic groups, over whether our research team should donate a moderate amount to carbon offsets or enlist someone like that group to email Congress. This set-up thus shuts down both the possibility of differential impact on liberal and conservatives' action and the need for any direct interaction between influencers and possible targets. When we do so, Democrats are equally likely to choose emails from liberals and conservatives. Thus, Democrats' relative unwillingness to reach across the political aisle does not arise from differential preferences over having a co- versus counter-partisan involved in climate action per se.

Moreover, we find no evidence that the partisan outreach gap arises because Democrats prefer to *personally reach out* to liberals versus conservatives, independent of their impacts on those targets' action. In a second round of the WTP experiment (N = 995), we randomly allow some influencers to hide their own political leanings from the profiles they can choose to pass on. Comparing this treatment's effect on the gaps in Democrats' *perceived influence* and *influence attempts* for liberal versus conservative targets, we cannot reject that closing the partisan gap in Democrats' influence beliefs would make them equally likely to invite conservatives and liberals to act.[3] Together, these patterns suggest that Democrats differentially try to engage other liberals in climate action because they don't expect cross-party outreach to succeed, not from a distaste for cooperating politically with counter-partisans.

On the other hand, second-order affective polarization plays a key role: Democrats expect their invitations to have less impact on conservatives precisely because they expect those conservatives to dislike or mistrust them. Indeed, participants in the round-two WTP experiment who have the option to hide their politics from possible targets estimate that hiding their political leanings from conservative recipients would make their invitations more than three times as effective. Moreover, they choose to hide their political leanings from conservatives 6 times as often as from other liberals when their profiles cannot affect whether targets email Congress and significantly more often when they can.

Our paper contributes to five primary literatures in economics and political science. First, we contribute to a growing literature on the diffusion of social and political movements across space (Qin et al., 2019; Aidt et al., 2022; García-Jimeno et al., 2022) and within social networks (Nickerson, 2008; González, 2020; Bursztyn et al., 2021).[4] Our paper is the first to show that citizens internalize their spillovers on others' political action and, in fact, intentionally try to spread political movements. We also document substantial diffusion from a particular form of interaction: sending invitations to join in emailing Congress. In experimentally introducing and evaluating a tool to facilitate diffusion, our work is most closely related to Bond et al. (2012) and Jones et al. (2017), who find that Facebook banners showing friends who reported voting in national elections increased voting both among viewers and their friends. Our paper is unique in experimentally creating interactions between political agents not connected by organic social or geographic networks, allowing us to causally estimate how spillovers are mediated by both political and non-political similarity.

We also contribute to a related literature that experimentally tests interventions to mobilize citizens for political action. We show that giving people opportunities to set examples for others motivates political action like contacting legislators; we thus join a small body of work exploring tools to engage citizens in collective action between election cycles (Han, 2016; Han et al., 2017; Bursztyn et al., 2021; Turnbull-Dugarte et al., 2022; Hager et al., 2023), while most prior work focuses on voter turnout (e.g. Gerber and Green 2000; Green et al. 2013; Pons 2018). A key strain of the turnout literature focuses on the power of social-image returns (Dellavigna et al., 2017; Gerber et al., 2017); we show that influence motives complement social-image concerns in making publicizing political action a powerful mobilization tool.

Third, we add to a growing literature exploring the role of affective polarization and partisanship across

[3]Letting participants hide their political leanings generates only a small first-stage effect on the partisan gap in influence beliefs, so our Wald-style estimate here is imprecisely estimated.

[4]Related papers test how information about aggregate participation in collective action affects others' participation, finding strategic substitution in some contexts (Cantoni et al., 2019; Hager et al., 2023) and complementarity in others (Hager et al., 2022).

American life.[5] Most related, Broockman and Ryan (2016) find that citizens differentially contact co-partisan legislators. We show that Democrats are also more likely to engage with co-partisans in building grassroots movements, even when common-ground beliefs with counter-partisans are clear.[6] We provide some of the first evidence on how beliefs versus preferences contribute to these engagement gaps. With a few exceptions (Dimant, 2023; Zhang and Rand, 2023), most prior work on partisan gaps implicitly attributes them to preference-based affective polarization. In contrast, we find that Democrats' relative unwillingness to reach across the political aisle can be entirely explained by their beliefs that they have less influence on conservatives. If beliefs are more malleable than preferences, this account is relatively hopeful for the future of political cooperation among citizens. On the other hand, while Americans misperceive counter-partisans on a range of other dimensions (Graham et al., 2012; Mildenberger and Tingley, 2019; Bordalo et al., 2020; Bursztyn and Yang, 2022), Democrats' influence beliefs are accurate in our setting. Thus, no obvious belief-correction intervention would promote cross-party cooperation.

Fourth, our paper adds to a small literature finding evidence for influence motives in some pro-social domains but not others (Reinstein and Riener, 2012; Karlan and McConnell, 2014; Esguerra et al., 2023). Esguerra et al. (2023) use a similar experimental design in contemporaneous work to show that Germans are more likely to register for COVID-19 vaccinations when doing so may encourage others to do the same. While they find that participants have no impact on others' vaccination in their setting, however, we find that Americans in our setting can in fact influence others' behavior, a feedback cycle that may be necessary to sustain influence motives in the long run. Together, our paper and Esguerra et al. (2023) suggest that offering opportunities to motivate others could be an important tool for facilitating pro-social behavior. In addition to extending the literature on influence motives to politics, we also decompose for the first time the beliefs and preferences that underlie attempts to influence others.

Finally, we add to the literature on mobilizing climate action. Prior work has primarily studied the determinants of beliefs about climate policy (e.g. Drews and van den Bergh, 2016; Maestre-Andrés et al., 2019; Dechezleprêtre et al., 2022) and, where it focuses on action, on shifting consumers to lower-carbon behavior (e.g. Allcott, 2011; Bilen, 2023; Ho and Page, 2023) or climate-friendly donations (Andre et al., 2021; Bernard et al., 2023). Together with Page et al. (2023), this project is to our knowledge the first experimental work focusing on real-world measures of political climate action.

The paper proceeds as follows. First, Section 2 describes the experimental context and Section 3 lays out a framework for influence motives. Section 4 describes the impacts of invitations in the Wave-2 sample, and Sections 5 and 6 experimentally test whether and to whom Democrats try to build the climate movement.

[5] Americans favor co-partisans over counter-partisans in real-stakes choices across non-political domains: dating and marriage (Klofstad et al., 2013; Huber and Malhotra, 2017; Iyengar et al., 2018), job callbacks (Gift and Gift, 2015), labor supply choices (McConnell et al., 2018), and mutual fund holdings (Wintoki and Xi, 2020). In the lab, Americans cooperate less with counter-partisans in dictator, public-goods, trust games (Hernández-Lagos and Minor, 2020; Dimant, 2023; Robbett and Matthews, 2023). Americans also favor co-partisans in hypothetical choices on roommates (Shafranek, 2021), college admissions (Munro et al., 2010), scholarship awards (Iyengar and Westwood, 2015), and residential choice (Gimpel and Hui, 2015).

[6] We also find that liberals react more strongly to political invitations from Democrats than do conservatives, though we cannot separate whether this gap arises from shared partisanship per se or because conservatives and liberals are differentially responsive overall. Previous work shows that Americans more highly value advice from co-partisans in non-political domains (Zhang and Rand, 2023; Marks et al., 2019) and differentially seek and respond to information that is ideologically congenial or from in-group sources (e.g. Jerit and Barabas, 2012; Peterson et al., 2021; Peterson and Iyengar, 2021).

Finally, Section 7 examines the mechanisms underlying Democrats' differential efforts to reach out along party lines, Section 8 tests the robustness of our main results, and Section 9 concludes.

# 2 Experimental context: A constructed social network for climate action

## 2.1 Real-stakes email advocacy

In this study, we recruit about 25,000 Americans to an online survey that includes an opportunity for real-stakes climate advocacy. In particular, our surveys embed a form through which participants can directly email their national Senators and US House Representative about climate change. (See Appendix Section B.1 for details and screenshots of the form.) We observe all emails sent via this form; throughout the project, we analyze both participants' own climate action—whether they email Congress—and their attempts to encourage others to do the same.

Engagement with the embedded climate-advocacy form provides an ideal measure of climate action. First, the email form is extremely externally valid. We license the form from a software company that provides nearly identical forms to advocacy organizations across the US, so our form matches those commonly used to mobilize citizen advocacy. Second, emailing Congress via our form is a meaningful form of climate action. Politicians systematically misperceive constituents' opinions (Broockman and Skovron 2018), and experimental evidence suggests that citizen lobbying by email and phone can affect politicians' voting behavior (Bergan 2009; Bergan and Cole 2015). Finally, emailing Congress in our setting is a high-effort action that may be relatively impervious to experimenter demand effects. Participants must customize the email body, and participants who write spend an average of 8 minutes composing their emails. On the other hand, this form of climate action is not so costly that few survey-takers complete it: across all of our experimental samples, about 32% match to records of emails sent via our form.

In the context of our research questions, one additional feature of this email form warrants particular note: while the email body is fully customizable, the email subject line is uneditable and always strongly supports climate action.[7] We impose these fixed subject lines to mitigate the possibility that Democrats avoid mobilizing conservatives because they expect their emails to oppose climate policy; throughout the experiment, participants know that no email sent via our form can fully oppose climate action. Thus, this restriction allows us to analyze whether Democrats choose to pursue allied political action from conservatives.

## 2.2 Basic network structure and influence opportunities

We examine liberals' efforts to build the climate movement by constructing a simple online network through which we connect over 25,000 Americans spanning the contiguous United States, age ranges, genders, and education levels. We recruit all of these participants via ads on social media and online survey panels.

**Wave 1 (Influencers) and Wave 2 (Targets):** Our quasi-social network relies on recruiting two waves of study participants: a Wave 1 of "influencers" and a Wave 2 of "targets." We first recruit Wave 1, all of whom have an opportunity to email Congress about climate change. At the same time, some Wave-1

[7] In particular, the subject line is randomized by participant across a set of phrases such as "My strong support for US climate policy," "Please address climate change", and "Ambitious climate action in the US." See Appendix Section B.1 for details.

participants have opportunities to invite future Wave-2 participants to join in climate advocacy. Across a series of experimental designs, the influence technology we provide to Wave-1 participants is to pass on basic invitations like that shown in Figure 1a. These invitations show influencers' basic demographics and avatar,[8] say that they emailed Congress, and invite the Wave-2 participant to join in action. We then truthfully and randomly pass on Wave-1 invitations to Wave-2 participants, allowing us to estimate the invitations' true impact on Wave-2 participants' action.

**Democratic influencers and bipartisan targets:** We require all Wave-1 participants to identify as members of the Democratic party. In contrast, Wave-2 participants may either belong to or lean towards the Democratic party or belong to or lean towards the Republican party; for simplicity, we refer to these Wave-2 groups henceforth as "liberals" and "conservatives," respectively. Across Waves 1 and 2, we require that all participants believe that climate change is mostly human-caused. (Appendix B.3 details these screening questions.) These sampling requirements allow us to approximate key dynamics in the real-world climate movement. We identify efforts to spread the climate movement among modal members of the US climate movement—Democrats who support climate policy—when they are connected with potential allies on each side of the political aisle.

**Matching Wave-1 and Wave-2 demographic groups:** To test whether Democrats differentially try to spread the climate movement along partisan or demographic lines, we randomly pair Wave-1 Democratic influencers with Wave-2 targets across demographic and partisan groups. In particular, we first construct demographic cells defined by 15-year age bins, gender, educational attainment, and 13 clusters of politically-similary and nearby US states.[9] We then randomly pair each Wave-1 demographic cell with a demographic cell in Wave 2: Wave-1 participants in a particular demographic cell have opportunities to influence Wave-2 participants in their paired demographic cell. Within demographic cells, we vary whether Wave-1 Democrats are paired to influence either Wave-2 liberals or conservatives. In turn, some Wave-2 participants will be randomized to see invitations to email Congress from members of their paired Wave-1 demographic group.[10]

Feasibly implementing this network structure—in particular, passing on invitations from Wave-1 participants to paired Wave-2 participants—requires that no demographic cell in Waves 1 or 2 be too small. To ensure that demographic cells are sufficiently large, we restrict both the Wave-1 and Wave-2 experimental samples to participants who live within the contiguous United States, are within the ages of 20 and 79,

[8] Throughout the experimental design, all participants will create basic demographic profiles showing the traits and avatar included in Figure 1a. Appendix Figure A6 shows a sample profile. These demographic profiles will be the medium through which participants learn about others across all of our experimental designs.

[9] In total, we generate 208 Wave-1 and Wave-2 demographic cells from the interactions of two gender categories, two education categories (with or without a 4-year college degree), four age groups (20-34, 35-49, 50-64, 65-79), and 13 state groups.

[10] Pairing up demographic groups of Wave-1 and Wave-2 participants, rather than individually randomizing Wave-1 participants to invite Wave-2 participants with particular demographics, allows us to both truthfully pass on invitations from Wave-1 participants to particular types of Wave-2 targets and to estimate the impacts of invitations on Wave-2 participants' action without bias. To see this, imagine that we individually randomized Wave-1 participants to try to influence particular Wave-2 demographic groups and that Wave-1 participants chose to pass on invitations only to Wave-2 participants on whom they thought their invitations would be particularly effective. Then, the nature of invitations we passed on in Wave 2 would be endogenous to Wave-2 participants' own traits, and any estimates of heterogeneity in invitations' effects across Wave-2 demographic groups would be biased. Instead, our randomization structure allows us to ensure that Wave-2 participants see invitations from exogeneously-matched Wave-1 participants. Note that demographic cells are defined on the same traits as shown in the Wave-1 invitation profiles, so these invitations are homogeneous within each group except for variation in name and avatar.

identify as a man or woman, and identity as white.

**Testing influence by climate-policy marginality:** In addition to testing whether Democrats differentially try to mobilize liberals versus conservatives, we will also test whether they differentially act to mobilize climate advocacy in states where it may be more impactful. One key demographic trait shown in participants' demographic profiles throughout the study is the politically similar and geographically close set of states in which they live. We construct these groups of states to fall into three categories of climate policy marginality: "blue states," where legislators would be likely to vote in favor of a climate bill even if not many residents called to support it; "red states," where legislators would be unlikely to vote in favor of a climate bill even if a fair number of residents called to supported it; and "purple states," where legislators could be convinced to vote for a climate bill if enough of their constituents supported it. We use surveys of Democrats recruited on Facebook and Twitter to construct 13 groups of geographically nearby states that participants tended to assign to the same policy-marginality group. (See Appendix Section B.2 for details.) We use these groups throughout the paper to test whether Wave-1 Democrats differentially seek to mobilize climate action where it may be particularly impactful in advancing climate policy.

### 2.3 Motivating descriptive evidence

Next, we briefly describe motivating evidence from a survey of participants that we recruited alongside the experimental samples of Democratic influencers we analyze throughout this project.

Democrats in our sample want bipartisanship in Congress on climate change: 89% at least somewhat agree that building support for climate policy among both Democratic and Republican politicians in Congress is crucial for reducing emissions, and 87% agree that the US government can only pass ambitious climate legislation in future if lawmakers of both parties support it (Appendix Figure A2). Moreover, they see bipartisan citizen advocacy as a key part of building bipartisanship in Congress: 84% at least somewhat agree that advocacy by conservatives, rather than liberals, could more effectively increase Republican lawmakers' support climate policy (Panel B, Appendix Figure A3), and 97% say that a bipartisan climate movement would be more effective than a purely liberal movement in advancing US climate policy (Panel A, Appendix Figure A3). Democrats see a role for liberals like themselves in building this bipartisan climate movement: 82% agree that liberals should try to get more conservatives involved in political climate advocacy (Appendix Figure A4).

However, few Democrats in the survey sample have themselves tried to engage conservatives in political climate action. While 58% says that they've invited someone to join in climate advocacy in the last 5 years, they've primarily reached out to other liberals (Appendix Figure A5). Only 50% can recall a particular instance in which they invited a conservative to join in political advocacy in the last 5 years, while 95% can recall a particular instance of inviting a liberal. While this gap could arise simply because Democrats are primarily close to other liberals (Appendix Figure A5), it could arise from more subtle beliefs or preferences. In this project, we give Democrats opportunities to invite a range of Americans, across the US and across the political spectrum, to join them in political climate advocacy. This set-up will let us look in detail at the political coalitions that Democrats try to build when these opportunities exist.

# 3 Conceptual framework

In this section, we describe a simple theoretical framework for influence motives, how they depend on key beliefs and preferences, and how these drivers might vary across conservative and liberal targets.

## 3.1 The value of inviting others

Consider a Wave-1 citizen who has just emailed Congress via our survey, and imagine that she then has an opportunity to invite a future Wave-2 participant to do the same; in particular, she can pass on an invitation like that shown in Figure 1a. What are the returns to doing so?

**A simple "neoclassical" model:** Consider first the simplest "neoclassical" model of influence motives, where the influencer cares only about the impacts of her invitation on the likelihood that Congress passes climate policy. Then, the value of inviting a future participant is as follows:

$$V(Invite) = \Delta P(Email) * V(Email\ Impact)\ + \Delta P(Target\ Acts\ After) * V(Action\ After\ Impact) - C$$

First, $\Delta P(Target\ Emails)$ captures the sender's beliefs about the impact of her invitation on the probability that the target emails Congress, and $V(Email\ Impact)$ captures how highly she values the target's email impact; the product of these terms then captures the net impact of the influencers' invitation via the target's email. Next, inviting participants to join in emailing Congress during the survey could also affect whether they take pro-climate action following the survey. The "neoclassical" value of an invitation thus also includes the product of how much more likely the target is to take post-survey pro-climate action—$\Delta P(Target\ Acts\ After)$—and the impact of this action—$V(Action\ After\ Impact)$. Finally, we subtract $C$, any opportunity cost of sending this invitation.

**A richer model with affective returns:** In the neoclassical model above, Wave-1 influencers choose whether and how to expand the climate movement to maximize impact, with no scope either for self- or social-image benefits from telling others that you emailed Congress or for emotional returns to influencing others. Moreover, this model allows no scope for *differential* affective returns to inviting co- versus counter-partisans, which the literature on affective polarization suggests could be substantial. Consider, instead, a model that creates scope for these emotional returns:

$$\begin{aligned} V(Invite) = {} & A(Image) + A(Try\ Influence\ After) + A(Try\ Influence\ Email) \\ & + \Delta P(Email) * [V(Email\ Impact) + A(Target\ Involved)] \\ & + \Delta P(Target\ Acts\ After) * [V(Action\ After\ Impact) + A(Target\ Involved)] - C \end{aligned}$$

Here, passing on an invitation saying that you emailed Congress could activate self-image or social-image benefits or costs, $A(Image)$. Next, there may be emotional returns from knowing that you're doing your part to spread climate action during the survey or after, given by $A(Try\ Influence\ Email)$ and $A(Try\ Influence\ After)$, respectively. Finally, how much the influencer values her target's action may comprise not only her valuation of their action's impact, but also the affective benefits or costs of knowing that they are involved in the climate movement, $A(Target\ Involved)$.

### 3.2 Identifying efforts to build the climate movement

The full expression for $V(Invite)$ above highlights a key empirical challenge in measuring Americans' attempts to expand the climate movement: participants' choices to pass on invitations may be driven both by image returns and by influence motives per se.[11] Across a series of experimental designs, we empirically isolate influence motives—how much participants value building the climate movement—by varying the timing of when Wave-1 Democrats can pass on profiles saying that they emailed Congress.

**Telling others that one emailed Congress after they've chosen:** Imagine that instead of sending a Wave-2 target an invitation to join in emailing Congress (Figure 1a), a Wave-1 influencer can instead only send the Wave-2 target a profile saying that they emailed Congress **after** that target decides whether to email Congress themself or not; see Figure 1b. Because this profile cannot affect the target's choice to email Congress or not, it does not include a line inviting the Wave-2 target to join in action, as in Figure 1a. We could express how much the Wave-1 participant values passing on this profile as follows:

$$\begin{aligned} V(Tell\ After) = A(Image) + A(Try\ Influence\ After) \\ + \Delta P(Target\ Acts\ After) * [V(Action\ After\ Impact) + A(Target\ Involved)] - C \end{aligned}$$

Here, telling a Wave-2 participant that one emailed Congress would still activate any social- or self-image concerns, $A(Image)$, as well as any benefits of trying to influence their pro-climate action outside of the survey itself. Then, comparing $V(Invite)$ and $V(Tell\ After)$ cleanly isolates how much participants value the opportunity to influence whether Wave-2 targets email Congress during the survey itself:

$$\begin{aligned} V(Try\ Influence\ Email) = V(Invite\ Before) - V(Tell\ After) = \\ = (Try\ Influence\ Email) + \Delta P(Email) * [V(Email\ Impact) + A(Target\ Involved)] \end{aligned} \tag{1}$$

Across a series of experimental designs, we use this logic to empirically identify Wave-1 Democrats' efforts to build the climate movement. In particular, we identify influence motives by randomly varying whether participants can tell Wave-2 targets that they emailed Congress before versus after those targets decide whether to do the same.

**Predicted heterogeneity in influence motives:** The expression for influence motives in Equation 1 yields several intuitive heterogeneity predictions. First, participants who believe that invitations have stronger impacts on others—higher "influence beliefs," or $\Delta P(Email)$—will more highly value opportunities to influence others. Second, if participants who are more concerned about climate change value others' emails to Congress more highly, those with stronger climate worry should also value attempts to influence others more highly. We will help corroborate that our experimental methods correctly isolate influence motives throughout the project by verifying these heterogeneity patterns.

[11] Influence motives encompass all benefits of trying to change targets' action during or after the survey itself. In the expression for $V(Invite)$ above, this includes all terms except for $A(Image)$ and $C$.

### 3.3 Affective polarization and differential influence motives by party

In this framework, what could underlie differential efforts by Democrats to reach out to conservatives or other liberals?

**Neoclassical drivers of influence gaps:** First, any such gap could arise from impact-based factors alone: Democrats may believe that their invitations would have differential impact on whether liberals versus conservatives email Congress ($\Delta P(Letter|D) \neq \Delta P(Letter|R)$), or they may believe that emails from liberals or conservatives would be differentially impactful in achieving short- or long-term climate policy goals ($V(Email\ Impact|D) \neq V(Email\ Impact|R)$).

**Preference-based drivers of influence gaps:** On the other hand, Democrats may differentially try to mobilize co- versus counter-partisans due to differential *preferences* for engaging liberals versus conservatives, captured in the affective terms in Equation 1 above. We can think of any such differential preferences as arising from affective polarization. First, Democrats may derive differential emotional payoffs from knowing that a liberal versus a conservative is engaged in the climate movement: $A(Target\ Involved|D) \neq A(Target\ Involved|R)$. Second, Democrats may derive differential emotional payoffs from trying to influence a liberal versus a conservative, independent of their probability of success: $A(Try\ Influence\ During|D) \neq A(Try\ Influence\ During|R)$

**Role of affective polarization in neoclassical drivers:** While affective polarization is captured directly in the gaps in $A(Try\ Influence\ During)$ and $A(Target\ Involved)$ above, it may also indirectly shape the "neoclassical" drivers of influence motives. First, Democrats with strong partisan sentiment may over-estimate the gap between $\Delta P(Letter|D)$ and $\Delta P(Letter|R)$ through motivated reasoning. On the other hand, participants may expect $\Delta P(Letter|R)$ to be low precisely because they expect counter-partisans to be affectively polarized against them.

## 4 Wave 2: Do action invitations work?

Before examining Democrats' attempts to build the climate movement, we first establish a key feature of the experimental context: Democrats can actually influence Wave-2 participants' action. In an experiment among Wave-2 participants, we find that seeing an invitation from a Wave-1 Democrat makes Wave-2 participants substantially more likely to email Congress. However, this true influence effect is only about half as large among conservatives as among liberals. Section 4.1 describes our Wave-2 experimental design and sample, Section 4.2 presents our empirical specifications, and Section 4.3 presents the Wave-2 results.

### 4.1 Experimental design, recruitment, and fidelity

#### 4.1.1 Experimental structure

Our experimental approach in Wave-2 is simple: we randomly assign half of a large sample of liberals and conservatives to see invitations from Wave-1 participants to join them in emailing Congress. We then test the impacts of these invitations on Wave-2 participants' action during our survey.

**See basic demographic profile of Wave-1 match:** Eligible Wave-2 participants begin their survey by building a basic demographic profile (Appendix Figure A6). We next show them the comparable profile

for a Wave-1 participant who has been randomly paired with them; we say that we are showing them this profile to give them a sense of who else is involved in our study (Panel A of Appendix Figure A7). Note that we show all participants the demographics of an earlier participant at this stage, regardless of their later treatment status, to ensure that any impacts of seeing an action invitation do not arise just from seeing details about an earlier participant. To encourage Wave-2 participants to pay attention to their match's demographics, we then ask them to rate how similar they are to this recent participant and to explain why.

**Treatment: Show participants action invitations from Wave-1 match:** After a series of questions about their climate beliefs, we show all participants a preview of the upcoming opportunity to email Congress. (See Appendix B.1.2 for details of the preview.) Next, we randomly implement the invitation treatment. In particular, participants in the Wave-2 treatment group see an invitation like that in Figure 1a from the same Wave-1 participant whose demographic profile they saw earlier; alongside, they see text saying that when their Wave-1 match took our survey, they chose to contact Congress via our form and to pass on this profile (Panel B of Appendix Figure A7).[12] To eliminate the risk that Wave-2 participants assigned to see invitations infer that their own action would be shown to others, treatment participants next see a slide stating that we will not tell any other participants whether they contact Congress during our survey or not.

**Choose whether to email Congress or not:** All Wave-2 participants then decide whether to email Congress or not over several stages: interested participants initially opt into the email process, choose whether to continue the process after being informed that the email form will require an address, and then can write and send their emails. Appendix Section B.1.2 lays out the survey flow for the full process of emailing Congress.

#### 4.1.2 Wave-2 sample recruitment and attrition

We recruit the Wave-2 sample of liberals and conservatives from a combination of social media media and online panels. Appendix Section C.1 describes Wave-2 recruitment in more detail.

**Social-media recruitment:** We recruited most liberals in the Wave-2 sample via ads on Facebook, Twitter, and Instagram. We also redirected qualifying conservatives or Democratic-leaning Independents recruited via social media who started but were ineligible for the Wave-1 survey. A total of 5,032 social-media participants were randomized to a Wave-2 treatment arm. This sample is highly skewed towards liberals: only 210 participants recruited via social media and randomized to a Wave-2 treatment lean towards the Republican party.

**Qualtrics recruitment:** We recruited nearly all conservatives in the Wave-2 sample from Qualtrics, which aggregates respondents from partnering market-research panels and online samples. Participants recruited via Qualtrics were subject to identical screening criteria on demographics, political affiliation, and climate beliefs as participants recruited from social media (Section 2.2). In total, 3,653 participants recruited from

[12] To ensure that this statement is truthful, we only pass on invitations from Wave-1 participants who emailed Congress and knew when they did so that an invitation would be passed on to Wave-2 participants in the Wave-2 participant's demographic group; Section 5 describes the Wave-1 treatment variations under which some participants knew these invitations would be sent. To facilitate this truthful matching, Wave-2 participants are randomly assigned to treatment status—whether they will later see an invitation or not—before being paired with a Wave-1 match and seeing their basic demographic profile. After randomly assigning Wave-2 participants' treatment status, we then pair them with a Wave-1 participant in their paired demographic cell (footnote 10) for whom we can truthfully pass on an invitation or not. Appendix Section C.2.1 describes this matching process in detail.

Qualtrics continued through Wave-2 treatment randomization. By construction, the Qualtrics sample is heavily conservative: 2,791 participants randomized to a treatment arm lean towards the Republican party, while 862 lean towards the Democratic party.

**Attrition:** In total, 8,685 participants are randomized to a Wave-2 treatment arm, comprising 5,683 liberals and 3,002 conservatives. Of these, about 92% advance through sending an email to Congress or not, with no differential attrition by treatment status (column 1, Appendix Table A2). We define our main Wave-2 experimental sample as the 7,981 participants for whom we observe email choices.

#### 4.1.3 Wave-2 sample description and balance

Columns 1 and 2 of Table 1 present summary statistics for liberals and conservatives in the Wave-2 experimental sample, respectively.

**Comparing Wave-2 liberals and conservatives:** Liberals and conservatives in the Wave-2 sample differ notably both in demographics and in political engagement and beliefs. On demographics, liberals are older, more likely to live in state groups that we classify as blue or purple, more educated, and wealthier than conservative Wave-2 participants. On politics, liberals report higher baseline engagement in activities like contacting politicians and signing petitions,[13] worry about climate change, desire for additional government action on climate change, and perceived local climate impacts than do conservatives. (See Appendix Figure A8 for the distributions of these variables separately among liberals and conservatives). Finally, Wave-2 liberals report a stronger preference for friends of their own party than do Wave-2 conservatives, so they may be more affectively polarized. (See Appendix Section C.2.3 for more detail on this measure.)

While some of these gaps, such as in educational attainment, mirror demographic gaps across parties in national samples (ANES 2020), others diverge. For example, Democratic- and Republican-leaners report about equal political engagement in national samples (Oliphant, 2018). These differential gaps in our survey in part reflect the differential selection induced among liberals and conservatives when restricting to those who believe climate change is mostly human-caused—88% of Democrats and only 35% of Republicans say so in national samples (Fuong and Skelley, 2022)—a form of differential selection that is also at work in the real-world climate movement. More artificially, the differences between liberals and conservatives in the Wave-2 sample also arise in part from the fact that we largely recruited liberals from ads about climate change on social media and recruited conservatives from Qualtrics. Appendix Table A1 compares baseline traits among liberals and conservatives separately by recruitment source. While liberals recruited from Qualtrics more closely match Wave-2 conservatives on income, education, and baseline political engagement, they remain more highly educated, wealthy, concerned about climate change, politically engaged, and affectively polarized. Section 8.1 shows that the differential impacts of Democrats' invitations on Wave-2 liberals and conservatives cannot be explained by basic demographic gaps between these groups.

[13] Even while liberals in the Wave-2 sample are more politically engaged than conservatives, they are much less likely to identify as members of the Democratic party than conservatives are to identify as members of the Republican party. This gap arises because 46% of Wave-2 liberals were redirected to the Wave-2 survey from Wave-1 recruitment because they identified as Independents rather than members of the Democratic party. 72% of liberal participants recruited through a mechanism other than Wave-1 redirection identify as members of the Democratic party, compared to the 74% of conservatives overall who identify as members of the Republican party.

**Balance:** Columns 3 through 8 of Table 1 show that the Wave-2 sample is balanced across treatment arms both among liberals and conservatives.

#### 4.1.4 Survey attention

Wave-2 participants' attention to the experimental set-up is generally high (Appendix Figure A9). We elicited all comprehension measures at the end of the survey among a randomly-chosen half of participants.[14] See Appendix Section C.2.2 for detail on these measures.

First, treatment participants are much more likely to believe that their Wave-1 match emailed Congress when they took our survey. Participants who saw an invitation are 60pp more likely to state that their paired Wave-1 participant emailed Congress (Panel A, Appendix Figure A9). Even when treatment participants state that they don't know whether their paired Wave-1 participant emailed Congress, they guess that their match did so with higher probability than do control participants (Panel B, Appendix Figure A9).[15] Second, Wave-2 participants also attend to their paired matches' demographics. In a series of multiple-choice questions at the end of the survey, participants correctly recall matches' political leanings, gender, age, and state group of residence at least 85% of the time (Panel A, Appendix Figure A10). Then, Wave-2 participants may react differentially to invitations by Wave-1 inviters' traits.

### 4.2 Empirical specifications

We estimate the impacts of seeing an invitation from a Wave-1 participant on whether Wave-2 participants email Congress in the following simple specification:

$$Contact_i = \alpha + \beta Treatment_i + \Phi X_i + \varepsilon_i \tag{2}$$

where $Contact_i$ indicates that participant $i$ emails Congress, $Treatment_i$ is an indicator for being assigned to see a Wave-1 invitation, and $X_i$ is a vector of control variables. Here, $\beta$ is our primary coefficient of interest.

**Primary outcome variables for emailing Congress:** Our primary Wave-2 outcome variable is whether participants match to an email record to Congress sent via the form embedded in our experimental survey. We match participants to email records by first name and email address, their treatment status—since we embedded separate forms by treatment arms—and the timing of their survey completion. Note that we often cannot simply match on email alone because participants may use different emails when consenting to the survey and emailing Congress. In total, we successfully merge 91% of all recorded emails sent via the embedded Wave-2 forms to Wave-2 participants, with no differential success by treatment group. Appendix Section B.1.3 describes the match process in detail.

[14] Note that participants are only included in our final sample if they successfully pass a simple attention check in which we explicitly ask them to choose a particular option in a multiple-choice question. Participants recruited via Qualtrics answered this attention check before randomization and were excluded from randomization if they failed it. In contrast, participants recruited via social media answered the attention check after randomization and were excluded at that point if they failed it. See Appendix Section C.1 for detail on how this attention check affected Wave-2 recruitment.

[15] One concern with the Wave-2 design is that participants assigned to see an invitation from an earlier participant might infer that their own action will be shown to future survey participants and act accordingly. However, treatment participants are actually more likely to correctly respond that future participants will not be told if they email Congress (Panel C, Appendix Figure A9). This pattern makes sense, since we explicitly stated after the invitation from a Wave-1 participant that their own action would not be shown to others.

In addition to our main treatment effects on whether participants match to an email record, we also estimate effects on whether participants initially opt in to the process of emailing Congress via our form. Across the full Wave-2 sample, only 54% of those who initially opt into the email process ultimately match to an email record.[16]

**Wave-2 control variables:** Our main Wave-2 specifications control for participants' recruitment source, demographics, baseline beliefs about politics and climate change, and baseline political engagement.

In particular, the demographic controls include participants' gender, age in 5-year bins, state of residence, income bin, educational attainment, and whether they identify as Hispanic or not. Our controls for baseline climate and politics beliefs include standardized measures of participants' stated worry about climate change, desire for additional government action, perception of the impacts of climate change in their local area, an index of beliefs about the extent to which the government responds to citizen advocacy, and indicators that participants identify as members of the Republican party, Republican-leaning Independents, Democratic-leaning Independents, or members of the Democratic party. Finally, we control for participants' baseline political engagement with a standardized index summing over whether participants have undertaken a series of political actions in the last two years, such as emailing, writing letters to, or phoning their political representatives or phone-banking for a political or social issue.

Appendix Section B.3 describes each of these control variables in detail, and Section 8.1 shows that our main Wave-2 results are fully robust to these choices.

## 4.3 Results: Invitations increase Wave-2 action

Invitations from Wave-1 participants have large effects on both Wave-2 liberals' and conservatives' political action (Table 2). Pooling across political affiliations, seeing an invitation makes Wave-2 participants 5.7pp (16%) more likely to opt into the email process and 4.4pp (24%) more likely to match to an email record.

### 4.3.1 Invitations have larger absolute impacts on action by liberals

While invitations increase action both among Wave-2 liberals and conservatives, their absolute impacts are larger among Wave-2 liberals. About 43% of liberals in the Wave-2 control group start the process of emailing Congress and 25% ultimately match to email records (columns 2 and 5); seeing an invitation increases these rate by 6.9 pp (16%) and 5.8pp (23%), respectively. Conservatives in the Wave-2 sample have substantially lower baseline rates of action—about 24% start the process of emailing Congress and only 8% ultimately match to an email record—but also react strongly to invitations from Wave-1 participants. Invitations make conservatives 4.4 pp (18%) and 2.1 pp (25%) more likely to start the process of emailing Congress and match to an email record, respectively (columns 3 and 6).[17]

[16] Participants who opt into the email process may drop out at several stages. First, 16% of those who initially express interest in emailing Congress leave the process after we warn them that the form will ask for their address. Second, participants who expressed interest in emailing Congress at both elicitations may simply not follow through with sending an email once they reach the form itself, or they may not match to an email record because their letter used a different name or email address: 36% of those who say they want to email Congress at the second elicitation do not match to an email record. Appendix Section B.1.2 lays out the survey flow for the full process of emailing Congress.

[17] Appendix Table A3 show that the treatment effects on Democrats are statistically and economically similar among those recruited from Qualtrics and social media. While the effects of invitations on whether conservatives start the email process is substantially larger among those recruited from social media (N = 163) than those recruited from Qualtrics (N = 2791), we do not interpret this small-sample result. All future Wave-2 analysis will pool respondents recruited from the two sources.

Proportionally, then, Wave-1 Democrats' invitations have very similar impacts on climate advocacy by liberal and conservative recipients. However, our conceptual framework suggests that the differential value of inviting a liberal versus conservative to join in political action hinges on the absolute impacts on targets' political action, rather than this proportional impact. We can reject ($p = 0.023$) that invitations have the same absolute impact on whether liberals and conservatives match to email records.[18] Section 8.1 below shows that these and the following Wave-2 results are highly robust to changes in sample restrictions, controls, and corrections for experimenter demand effects.

#### 4.3.2 The role of affective polarization in partisan gaps

We find suggestive evidence that more polarized conservatives respond less to invitations from Wave-1 Democrats. We measure affective polarization in several ways: how strongly participants say they would prefer to be friends with someone from their own political party, the residuals of this variable when regressed on a suite of demographic traits, and how warmly participants rate their own party versus the opposing party on a "feelings thermometer" commonly used in national samples; we only collect thermometer values for a random half of Wave-2 participants, but we also test for heterogeneity by OLS- and lasso-predicted values of this measure in the full sample. Appendix Section C.2.3 describes these measures in detail. Appendix Tables A4 and A5 estimate how invitations' impacts on opting into the email process and email records, respectively, vary across liberals and conservatives with above- or below-median polarization by each metric.

Among liberals (columns 1-5), there is no consistent heterogeneity in invitations' impacts by polarization. Indeed, we would not expect heterogeneity among Wave-2 liberals by polarization, since they are interacting only with co-partisans. On the other hand, our point estimates suggest that invitations have substantially lower impacts among more polarized conservatives (columns 6-10), though we are under-powered to statistically reject homogeneous effects. The only exception to this pattern is when we measure affective polarization using participants' simple Likert-scale reports of how much they would prefer to be friends with a member of their own party; due to heavy bunching on the scale, only 112 of 2,960 conservative Wave-2 participants report a value below the median.

#### 4.3.3 Similarity on other dimensions matters, but by less than politics

We find suggestive evidence that sender-recipient similarity on non-political traits also makes invitations more effective, though these traits may have less impact than sharing politics. Figure 2 plots the interactions between $Treatment_i$ and indicators that Wave-1 and Wave-2 participants match on each of the traits shown in the invitations: political leanings, gender, age group, educational attainment, and state group of residence; Appendix Table A7 reports these regression results in full.[19]

[18] In Appendix Section C.3, we explore the mechanisms by which invitations increase action among liberals and conservatives. Seeing an invitation from an earlier participant increases participants' reports of how worthwhile it is to email Congress and shifts up their beliefs about the share of other study participants who emailed Congress, with similar patterns among liberals and conservatives. Simple mediation analysis controlling in turn for each of these intermediate outcomes suggests that invitations' impacts on action can be primarily explained by their impacts on beliefs about others' action, consistent with the idea that invitations work by changing participants' perceived norms of political action in our context.

[19] Appendix Tables A8 and A9 also test for heterogeneous impacts of invitations by Wave-1 sender traits and Wave-2 recipients traits, respectively. We find suggestive evidence that invitations from Wave-1 participants in red states, rather than blue or purple, may have larger effects overall, consistent with the idea that invitations from non-stereotypical climate activists could provide stronger signals to recipients. Turning to Wave-2 recipients' traits (Appendix Table A9), we find that invitations may have larger impacts on woman and on older targets.

Matching on politics increases invitations' impacts by 3.8pp in these joint regressions.[20] However, non-political similarity matters as well. While each of these interactions is imprecisely estimated, invitations increase Wave-2 participants' likelihood of matching to email records by an additional 1.5pp when senders and recipients share educational attainment, by 1.8pp when they live in the same state group, and by 3.5pp when they share an age group. Pooling across these dimensions, invitations' impact on email records rises by about 1.4pp for each shared non-political trait (column 8, Appendix Table A7), though this coefficient is statistically indistinguishable from zero. Increasing a continuous measure of Wave-2 participants' perceived non-political similarity to their Wave-1 match by one standard deviation increases the invitations' impacts by a statistically-significant 1.8pp (column 9).[21] Altogether, these results suggest that while Wave-2 participants respond more to invitations from senders who are like them in non-political ways, the impact of sharing these other traits is smaller than that of shared politics. Consistent with these patterns, shared politics is by far the largest contributor to how similar Wave-2 participants feel to their Wave-1 matches, even as matching on non-political traits matters as well (Appendix Figure A11).

# 5 Wave-1 action experiment: Do liberals act to build the climate movement?

Given that Wave-1 Democrats can influence Wave-2 participants' political action, do they internalize these spillovers and seek to build the climate movement? In the "Wave-1 action experiment," we start by establishing a simple fact: liberals indeed are motivated to engage others in climate action. In particular, they are more likely to advocate for policy change when doing so can inspire others to join them. Section 5.1 describes our Wave-1 experimental design and sample, Section 5.2 describes our empirical specifications, and Section 5.3 presents results.

## 5.1 Experimental design, recruitment, and fidelity

### 5.1.1 Experimental structure

The Wave-1 action experiment tests whether Democrats are more likely to email Congress when doing so allows them to invite others to join them. In particular, we randomly assign a subset of Wave-1 Democrats to know that if they email Congress, we will pass on invitations like those in Figure 1a to Wave-2 participants on their behalf. To separate influence motives from any social- or self-image concerns from simply telling others that one took action, we randomize other Wave-1 participants to know that if they email Congress, we will tell Wave-2 participants that they did so *after* those targets decide themselves whether to email Congress or not. This design maps closely to the conceptual logic laid out in Section 3.2.

While our design creates substantial variation in the demographics of Wave-2 targets whom Wave-1 participants can try to influence, this portion of the experiment is not powered to examine differential

[20] An important caveat here is that sharing political leanings is collinear with the Wave-2 recipient being a liberal, so we cannot separate the role of political similarity from liberals responding differentially to invitations overall.

[21] We elicit Wave-2 participants' perceptions of how similar they are to their paired Wave-1 match on a scale from 1 (Not at all similar) to 7 (Extremely similar) after first showing them this match's basic demographic profile. We regress this Likert-scale measure on indicators for matching one's paired Wave-1 participant on each demographic trait (Appendix Figure A11). We then estimate perceived non-political similarity by predicting perceived overall similarity from these estimates, subtracting the predicted contribution of shared party from Democrat-liberal matches, and then standardizing this measure in the Wave-2 sample.

influence motives by targets' party or other demographics. We explore suggestive evidence on differential influence motives here, but we primarily examine these gaps in a nested experiment described in Section 6.

**Told that basic demographics will be shown to Wave-2 matches:** Wave-1 participants begin their survey by building a basic demographic profile (Appendix Figure A6). After showing each Wave-1 participants their own profile, we say that this profile will be shown to up to 10 future Wave-2 participants in a particular demographic group in order to give those future participants a sense of who else is participating in the study. We show each Wave-1 participant a simple demographic profile of the Wave-2 participants to whom their own profile may be shown (Appendix Figure A17). As we describe in Section 2.2, Wave-1 participants in a given demographic cell are randomized to be paired with either liberal or conservative Wave-2 participants from another demographic cell.

This set-up holds fixed across all Wave-1 participants that their basic demographic profile may be shown to future Wave-2 participants, no matter their treatment group or whether they choose to email Congress; doing so ensures that no treatment effects of passing on an invitation (Figure 1a) operate through participants' concerns about showing their demographics to Wave-2 participants. As in Wave 2, we ensure that participants attend to the demographics of their paired Wave-2 participants by asking them to report how similar they feel to these participants, from 1 (Not at all similar) to 7 (Extremely similar), and why.

**Treatment variations to identify influence motives:** Next, we show all Wave-1 participants a preview of the upcoming opportunity to email Congress. (See Appendix B.1.2 for details.) We then randomize participants to treatment variations that cleanly identify attempts to influence others' action.

First, we randomize some participants into a pure control group (denoted A0): these participants are told nothing about whether their choice to email Congress will be reported to any future participants. Next, we randomize other participants into two treatment groups in which they're told that the same future Wave-2 participants who may see their demographic profile will also be told if they contact Congress. Participants assigned to the "Invitation" group (denoted A2) are told that these Wave-2 participants will see an invitation from them (Figure 1a) to join in emailing Congress, and targets will see this invitation **before** deciding whether to email Congress or not. In contrast, participants assigned to the "Tell after" group (denoted A1) are told that these Wave-2 participants will see a profile saying that they emailed Congress (Figure 1b) **after** deciding whether to do so themselves. Note that the extra profiles passed on from A1 participants do not include a statement explicitly inviting the Wave-2 participants to join them in action. In both groups A1 and A2, participants are told that if they do not email Congress, their paired Wave-2 participants will see nothing about whether they or any other participants emailed Congress. Appendix Section D.2.1 lays out the text of these treatment variations.

By the logic of the framework in Section 3, comparing the A0 (Pure control), A1 (Tell after), and A2 (Invitation) treatment arms isolates influence motives from social- or self-image concerns. Any gaps between the rates of emailing Congress in the A1 (Tell after) versus A0 (Pure control) groups could arise from self- or social-image concerns activated by knowing others will be told that you emailed Congress, as well as any efforts to affect Wave-2 participants' behavior after the experiment. In contrast, the only difference between the A1 (Tell after) and A2 (Invitation) groups is that A2 participants' choice to email

Congress or not can affect whether their paired Wave-2 participants do so as well.[22] Thus, this variation allows us to cleanly estimate whether Wave-1 Democrats act to inspire others.

**Ensuring treatment comprehension**: To ensure that Wave-1 participants in the A1 and A2 groups understand the experimental set-up, we show participants flowcharts indicating when their paired Wave-2 participants would see their extra profile (Appendix Section D.2.1). Finally, we ask and correct participants' answers to incentivized comprehension questions on what their paired participants will see if they email Congress, when these participants will see this extra profile, and what the future participants will see if they do not email Congress. Appendix Section D.2.2 details these comprehension questions. We make these questions as subtle as possible by emphasizing that we ask them to ensure that participants know how their basic information will be used in future surveys.

**Choose whether to email Congress or not:** All Wave-1 participants then decide whether to email Congress or not, following the same process as Wave-2 participants (Section 4.1), described in Appendix Section B.1.2.

### 5.1.2 Wave-1 sample recruitment and summary

**Recruitment:** We recruit Wave-1 participants using ads on Facebook, Instagram, and Twitter. In total, 29,596 unique participants consented to the survey and provided an email address, which we required of all participants in order to link them with records of emailing Congress. Of these, 12,755 participants met our full set of screening criteria based on demographics, climate beliefs, and identifying as members of the Democratic party. We randomize 8,937 participants into the Wave-1 experimental sample and randomize an additional 2,004 participants into parallel experimental samples described in Sections 6 and 7 below. Appendix Section D.1 describes this recruitment process in detail.

**Attrition:** While the A0 control group advances immediately from randomization to answering whether they want to email Congress, the A1 and A2 treatment groups go through several pages of explanation and comprehension questions between randomization and answering whether they want to email Congress or not. Thus, while we can directly observe whether 99.2% of the A0 group emails Congress, we are about 12pp less likely to observe whether participants in the A1 and A2 groups email Congress (Appendix Table A11, Panel A column 1). On the other hand, there is no differential attrition between the A1 and A2 groups.

Our main analysis makes the conservative assumption that participants who leave the survey between learning about the upcoming opportunity to email Congress and deciding whether to do so or not would not have emailed Congress had they continued.[23] Participants see the email preview before randomization, so this assumption allows us to define whether all randomized participants emailed Congress. All of our main Wave-1 conclusions are robust to restricting the sample to participants for whom we can directly observe choices to email Congress or not (Section 8.2).

[22] Contemporaneous work by Esguerra et al. (2023) and earlier work by Karlan and McConnell (2014) use similar experimental variation to isolate influence motives.

[23] The attrition patterns support this assumption. A1 and A2 participants who drop out of the survey before deciding to email Congress or not are more likely to identify as men, are older, are more likely to live in a purple state, are less likely to have a 4-year college degree, and report substantially lower past political participation than those who remain (Appendix Table A11, Panel B). Nearly all of these baseline traits, especially past political participation, predict lower likelihood of emailing Congress in the A0 control group (Appendix Table A12).

**Sample description:** Column 1 of Table 3 presents summary statistics for the 8,937 participants randomized into the Wave-1 experimental sample. This sample is highly educated and wealthy: over 80% have a 4-year college degree or more, and 51% and 15% have total pre-tax household income over $100,000 and $200,000, respectively. Recall, in addition, that the Wave-1 sample is fully white by construction (Section 2.2). While our sample therefore cannot represent the full mainstream environmental movement, it does reflect a movement that remains predominately white, affluent, and educated.[24] The Wave-1 sample is also quite politically engaged: 73% of participants report that they've contacted elected representatives about a political or social issue in the last two years, while in 2018 only 40% of Americans reported having contacted an elected official in the last 5 (Oliphant, 2018) years.

**Balance:** Columns 2 through 6 of Table 3 show that the sample is largely balanced on baseline characteristics across the A0, A1, and A2 treatment groups. There are several small imbalances on age categories and some forms of political engagement, so we control for baseline traits in our main specifications. Appendix Table A10 shows that the sample is also largely balanced across treatment arms when split by whether Wave-1 participants are paired with liberal or conservative members of their matched Wave-2 demographic group.

### 5.1.3 Set-up comprehension and attention

Most Wave-1 participants understand the survey set-up—in particular, whether their action can influence others (Appendix Figures A18 and A19). We elicited A1 and A2 participants' comprehension at two points in the survey: just after describing the treatments, after which we correct their answers, and again at the end of the survey. (See Appendix Section D.2.2 for details.)

Before correction, over 85% of respondents in both the A1 and A2 groups correctly state that their matched Wave-2 participants would see that they emailed Congress if they do so, and over 71% correctly say that these matches would see only their basic demographic profile if they do not (Appendix Figure A18). Moreover, participants generally attend to *when* paired future participants would be told that they emailed Congress (Panel A of Appendix Figure A19). Before correcting their answers, participants in the A2 group are about 56 pp more likely than A1 participants to say that future participants will see that they emailed Congress before deciding whether to do the same. At the end of the survey, after we correct their initial answers, this gap rises to 76 pp. Finally, A2 participants are 51 pp more likely than A1 participants to

[24] In recent years, commentators on the mainstream US environmental movement have increasingly focused on its longstanding dominance by white and educated members of the middle-class (Jones 2020, Ortiz 2021), even as evidence overwhelming shows that racial minorities and lower-income communities disproportionately bear the burdens of environmental harms (e.g. Jbaily et al. 2022, EPAP 2021) and may report higher environmental concern than whites (Pearson et al. 2018). The mainstream environmental movement's whiteness and affluence took root early, growing out of a focus on conserving wilderness areas largely accessible only to the white and wealthy and from, in some cases, overtly racist leadership (Purdy 2015). Beginning in the late 1960s, several studies document that environmental activists were overwhelming white and disproportionately likely to have a college degree (Taylor 2014). This socio-economic skew has persisted strongly over time, even as an environmental-justice movement led predominantly by people of color rose to prominence in the 1980s (Mohai et al. 2009). The Bureau of Labor Statistics found in 2015 that 3% of white Americans reported volunteering for an environmental or animal-care organization in 2015, compared to 1% of Blacks, 1.2% of Asian Americans, and 1.6% of Hispanics. In the 2014 People's Climate March, one of the largest climate demonstrations in history, whites and those in the top two quintiles of 2013 income were substantially over-represented (Beer 2017). This racial imbalance extends strongly into the leadership of the mainstream environmental movement; in 2014, ethnic minorities filled only 16% of general staff and 12% of leadership positions in government environmental agencies and mainstream environmental organizations (Taylor 2014).

answer at the end of the survey that their choice to email Congress or not could in theory affect whether their paired Wave-2 participants do so during the survey (Panel B of Appendix Figure A19). This gap provides a sizable implicit first stage on influence beliefs.

**Attention to Wave-2 participants' demographics:** Wave-1 participants also attend to their paired Wave-2 matches' demographics, so they may differentially seek to influence Wave-2 participants with certain traits. In multiple-choice questions at the end of the Wave-1 survey, about 75% or more of both A1 and A2 participants correctly identify their Wave-2 matches' political leanings, gender, age, education, and state group (Panel B of Appendix Figure A10).

## 5.2 Empirical specifications

Our simplest analysis restricts to the A0 (Pure control) and A2 (Invitation) groups and estimates the following regression:

$$Contact_i = \alpha + \beta_1 TellBefore_i + \Phi X_i + \varepsilon_i$$

where $Contact_i$ indicates that participant $i$ emails Congress about climate change, $TellBefore_i$ is an indicator for being in treatment group A2, and $X_i$ is a vector of control variables. Here, $\beta_1$ captures the policy-relevant effect of having opportunities to invite others: are Democrats more or less likely to email Congress when doing so lets them invite 10 future participants to join them?

However, this simple regression above does not distinguish influence motives from social- or self-image concerns that are activated by others knowing you emailed Congress. To do so, we incorporate participants assigned to the A1 (Tell after) treatment group in the following specification:

$$Contact_i = \alpha + \beta_2 Tell_i + \beta_3 Invitation_i + \Phi X_i + \varepsilon_i \tag{3}$$

where $Tell_i$ indicates that participant $i$ is in group A1 or A2, $Invitation_i$ indicates that participant $i$ is in group A2, and $X_i$ is a vector of control variables. Here, $\beta_2$ captures the effects of knowing that up to 10 future participants will be told that one emailed Congress, while $\beta_3$ captures influence motives: whether participants are more or less likely to email Congress when doing so can affect whether others join in action during our survey.

**Primary outcome variables for emailing Congress:** As in Wave 2, the primary outcome variable throughout our Wave-1 analysis is whether each participant matches to a record for an email sent via our form. In total, we successfully merge 94% of all recorded emails sent by Wave-1 participants to individuals. We also present impacts on whether participants initially opt in to the process of emailing their Senators and House Representatives via a form in our survey. Recall, as described in Section 5.1.2, that we make the conservative assumption of filling in zero for both of these outcomes among participants who left the survey between being randomized to a Wave-1 treatment arm and explicitly choosing whether to email Congress or not.

**Wave-1 control variables:** Our main regression specifications include all of the same control variables for participants' demographic traits, baseline beliefs about politics and climate change, and baseline political

engagement as in Wave 2 (see Section 4.2), with several exceptions.[25] Appendix Section B.3 describes each of these control variables in detail, and Section 8.2 shows that our Wave-1 results are entirely robust to varying the controls we include.

## 5.3 Results: Democrats act to mobilize others

### 5.3.1 Impacts of opportunities to invite others

First comparing just the A2 and A0 groups, we find that having opportunities to invite others to join in emailing Congress substantially increases rates of climate advocacy (column 1, Table 4). Being assigned to the A2 Invitation group makes Democrats 16pp more likely to initially opt into the email process and 16pp more likely to ultimately match to an email record, increases of 34% and 48% over the A0 control means.

**Separating out influence motives:** About one-fifth of the total effect of knowing that future participants will see your action *before* deciding whether to act (column 1) can be attributed to participants' efforts to influence whether those participants email Congress during the survey itself. To isolate these motives, Column 2 of Table 4 incorporates the A1 treatment arm to estimate equation 3. Participants who know that future participants will be told if they email Congress *before* deciding whether to do the same rather than *after* (i.e. assigned to the A2 group relative to A1) are 3.3 pp more likely to start the email process (Table 4 Panel A, column 2) and 2.7 pp more likely to have an email record (Table 4 Panel B, column 2). These "influence motive" effects amount to 7.2% and 8.7% of the relevant A0 control means, respectively.[26]

The remaining four-fifths of the total A2 effect arise from a combination of self- and social-image concerns and attempts to influence what paired participants do after the survey. Being assigned to the A1 treatment arm, where others will know that you emailed Congress during the survey but it cannot affect whether they do the same, makes Democrats 12pp (26%) more likely to opt into the email process and 13pp (43%) more likely to match to an email record (Table 4 Panels A and B, column 2).

Though we cannot decompose this effect into image effects and attempts to influence targets' post-survey behavior, our evidence suggests that these influence motives may play a role in explaining the differences between participants in groups A0 and A1. About 54% and 48% of participants in the A1 and A2 groups correctly answer that their decision to email Congress or not could in theory affect paired future participants' behavior after the survey, with nearly all other participants answering that they do not know if it could (Appendix Figure A20, Panel A). Furthermore, when asked how likely it is that their decision to email Congress or not would affect what paired participants do after the survey, about 60% of A1 and A2 participants report at least 4 on a scale from 1 (Not at all likely) to 7 (Extremely likely) (Appendix Figure A20 Panel B). Of

[25] The only exceptions are that the Wave-1 regressions we do not control for recruitment method, since we recruited all Wave-1 participants directly via social media, and or for political affiliation, since all Wave-1 participants identify as members of the Democratic party.

[26] The A2 versus A1 variation could affect action through a mechanism other than influence beliefs if participants expect many or all other survey-takers to face the same information structure—i.e. to also have opportunities to influence others' action—and revise their beliefs about aggregate participation in emailing Congress via our survey in response. As we describe in Appendix C.3, the impacts on action of any such belief updating are theoretically ambiguous (Miller and Prentice 2016; Rogers et al. 2018; Gerber et al. 2018; Cantoni et al. 2019, Hager et al.). However, A2 has no differential effect relative to A1 on participants' incentivized beliefs for how many out of 100 people who took the survey chose to email Congress via our form (column 2, Appendix Table A19).

course, self- or social-image concerns are also likely to be strong drivers in this setting, as in other political and non-political domains (DellaVigne et al. 2017, Bursztyn and Jensen 2017).

### 5.3.2 Democrats may act to influence liberals more than conservatives

While the Wave-1 action experiment is not powered to test whether Democrats differentially try to mobilize co- versus counter-partisans, we find suggestive evidence that Democrats differentially act to mobilize liberal Wave-2 participants. Columns 3 and 4 of Table 4 estimate our Wave-1 regressions among participants paired with liberal Wave-2 matches, while columns 5 and 6 do the same among those paired with Wave-2 conservatives.

**Partisan gaps in overall A2 effects:** Turning first to columns 3 and 5 of Table 4, Democrats respond less overall to opportunities to invite Wave-2 conservatives than to opportunities to invite Wave-2 liberals. Among Democrats paired with Wave-2 liberals (column 3), A2 participants are 18pp (40%) more likely to opt into the email process and 18pp (56%) more likely to match to an email record than A0 participants. In contrast, the A2 treatment only makes Democrats 12pp (26%) more likely to opt into the email process and 14pp (44%) more likely to match to an email record when they are paired with Wave-2 conservatives (column 5). We can reject with 95% confidence that the overall impacts of A2 on opting into the email process are equal across those paired with liberals versus conservatives.

**Partisan gaps in influence motives and image returns:** Our point estimates imply that the partisan gaps in the overall impacts of A2 arise both from differential self- or social-image concerns and from differential efforts to engage liberals and conservatives in climate action (columns 4 and 6). In both panels A and B of Table 4, the impacts of A1 versus A0 are somewhat larger in magnitude when Wave-1 Democrats are paired with liberal Wave-2 participants. While this gap could arise from differential efforts to influence targets' behavior after the survey, it is also consistent with differential self- or social-image returns to telling co- versus counter-partisans that one emailed Congress about climate change.

Moreover, liberals in our sample may differentially act to motivate other liberals, rather than conservatives. Participants are 4.9 pp (10.8%) more likely to opt into the email process and 3.5 pp (11.5%) more likely to match an email record when doing so could influence whether up to 10 liberals do the same, while being only 1.7 pp (3.6%) and 2.0 pp (6.3%) more likely to start to or ultimately email Congress, respectively, when doing so could influence conservatives. We cannot reject that these coefficients are equal in either Panel A or Panel B, but they suggest that liberals may differentially act to build the climate movement within, rather than across, party lines.[27]

[27] In contrast to these patterns by partisanship, we do not find evidence that Democrats differentially act to influence Wave-2 targets who match them in non-political ways (Appendix Tables A14 and A15). Pooling across all non-political demographic dimensions, we find zero heterogeneity in either the A1 versus A0 gap or the A2 versus A1 gap when Wave-1 participants and their Wave-2 matches share an additional non-political demographic trait or have higher predicted non-political similarity. (We estimate Wave-1 participants' perceived non-political similarity with their Wave-2 matches via exactly the same strategy as in Wave 2. See footnote 21.) Next, Appendix Table A16 tests whether Wave-1 Democrats differentially respond to opportunities to invite Wave-2 participants with particular traits overall. These effects are all imprecise, but our point estimates suggest that Democrats may differentially act to mobilize Wave-2 participants with a 4-year college degree, and in blue states.

#### 5.3.3 Evidence for influence motives: Theory-predicted heterogeneity

While the experimental design cleanly isolates influence motives, several additional pieces of evidence help to confirm that the A2 versus A1 effects we estimate indeed capture participants' efforts to mobilize others. In particular, the effects of being assigned to A2 correlate with several traits of Wave-1 participants that the framework in Section 3 suggests should correlate with stronger attempts to influence others.

**Heterogeneity by climate worry:** Our framework predicts that influence motives are stronger among those who care more about climate change and thus more highly value future participants' emails to Congress. Early in the Wave-1 survey, we ask participants how worried they are about climate change, on a scale from 1 (Not at all worried) through 7 (Extremely worried). In columns 2 and 6 of Appendix Table A17, we find that our point estimates for influence motives are substantially larger among Democrats with above-median climate worry. We reject at the 10% level that the A2 versus A1 treatment effects on opting into the email process are equal across these groups.

**Suggestive heterogeneity by influence beliefs:** The framework in Section 3 also predicts that influence motives are stronger among participants who estimate that $\Delta P$, the impacts of invitations on Wave-2 participants' action, is larger. At the end of the survey, we randomize one-third of A1 and A2 participants to estimate a fictional future participants' likelihood of emailing Congress during our survey if they do or do not see an invitation from the Wave-1 participant.[28] Columns 3 and 7 of Appendix Table A17 test for heterogeneous treatment effects on starting the email process and having an email record, respectively, by whether participants report above- or below-median numeric influence beliefs. While our estimates are imprecise, they suggest that influence motives are stronger among those with stronger influence beliefs.

## 6 The WTP experiment: Who do inframarginal activists invite?

While the Wave-1 action experiment shows that Democrats act to build the climate movement, it is not designed to look in detail at *who* these citizens seek to recruit to the climate coalition. This section describe a sub-experiment embedded in the Wave-1 A0 pure control group that we refer to as the Willingness-to-pay (WTP) experiment.

This sub-experiment serves two goals. First, while the Wave-1 action experiment tests whether influence opportunities can induce Democrats to act, the WTP experiment identifies influence motives among inframarginal climate activists—those who email Congress when they have no opportunity to tell others. Second, and most importantly, the WTP experiment tests *whom* Democrats invite into the climate movement. We find that Democrats differentially seek to mobilize other liberals, and that these partisan influence gaps dwarf attempts to mobilize political action by state or any other match traits.

### 6.1 Experimental design

We start the WTP experiment by telling participants that we will soon show them a list of 20 past study participants who we will be recontacting to take a second short survey.[29] Each WTP participant will be

[28] One concern with this measure is that it may itself be affected by treatment status through motivated reasoning. However, we find no evidence for such effects (Appendix Table A19, column 3).

[29] We recruited this sample of possible matches prior to starting the Wave-1 experiment and from participants who started but were ineligible to complete the Wave-1 experiment. We do not in any way analyze the behavior of these potential matches.

randomly paired with one of these 20 participants, who will see a profile of the WTP participant when they return for the follow-up survey.

**Randomization of profile timing:** We randomize whether each possible match would see the WTP participants' profile before versus after themselves choosing whether to email Congress or not. We motivate this variation simply by telling WTP participants that matches would see their profiles at different times based on the "structure of the survey they're signed up to complete."

**Choosing whether to say that one emailed Congress:** For each of the 20 possible matches to whom each WTP participant could be paired, we ask them to make a costly choice for whether the profile that match would see should include that the WTP participant chose to email Congress. In particular, we ask participants to choose between the following two options:

*Option 1. Basic profile and carbon offsets:* We show the returning match the WTP participant's basic demographic profile and add a fixed amount, randomized at $3, $4, $5, or $6 across participants, to a carbon offset purchase that our research team would make.[30] WTP participants constructed this basic demographic profile (Appendix Figure A6) at the beginning of the Wave-1 survey.

*Option 2. Extended profile:* We show the returning match an extended profile saying that the WTP participant emailed Congress and, in some cases, inviting them to join in action. A matched participant seeing an extended profile *before* deciding to email Congress would see the profile shown in Figure 1a, while one assigned to see the extended profile *after* deciding would see the profile shown in Figure 1b, which does not include an invitation to join in action.[31]

**Identifying influence motives:** As in the Wave-1 action experiment, we identify influence motives using the variation in when possible matches would see that each WTP participant emailed Congress. In particular, we test whether participants are more likely to make the costly choice to tell possible matches that they emailed Congress when doing so can affect whether those matches email Congress themselves.

**What WTP participants see about possible matches:** Participants see demographic profiles for each of the 20 matches to whom they could be paired (Appendix Figures A25 and A26). These profiles differ from the basic demographic profiles used throughout this project in two key respects. First, the profiles include an additional attribute, denoted "Status," where those who would see one's profile before versus after deciding whether to email Congress are labeled as "Hasn't been asked" and "Already decided," respectively. Second, we randomize WTP participants to either see profiles of possible matches that state that they believe climate change is human-caused or that include no information about their climate beliefs. This variation allows us

[30] Since WTP participants may be unfamiliar with carbon offsets, we benchmark these dollar amounts as offsetting the approximate emissions from driving a new 2WD SUV between cities located near the participants' state of residence. We calculate these driving distances using the conversions published by Clear (https://clear.eco/), the company from which we purchase the carbon offsets. For a participant from Iowa, for example, we would benchmark a $4 carbon offset purchase as offsetting approximately the emissions from driving 430 miles, the rough distance from St. Louis to Omaha, Nebraska or to Hunstville, Alabama.

[31] One concern in comparing options 1 and 2 is that option 2 can only be implemented if the possible match in fact returns for a follow-up survey. If WTP participants expect certain possible matches to be more likely to return than others, these beliefs could drive spurious heterogeneity in participants' binary choices across possible matches. To eliminate this risk, we explicitly tell WTP participants that we will only make the carbon-offset donation if their paired match returns for a follow-up survey and sees their basic demographic profile.

to test the degree to which establishing a simple common-ground belief about climate change mitigates any gap in Democrats' attempts to invite other liberals versus conservatives into the climate movement.

### 6.2 WTP sample recruitment and experimental fidelity

**Recruitment:** We recruit WTP participants from those in the Wave-1 A0 pure control group who report that they emailed Congress during our survey. Thus, these are inframarginal climate advocates with respect to influence motives: they email Congress when no one will be told that they did so.[32] In total, 1,350 participants begin the main WTP survey. Appendix Section E.2 details WTP recruitment.

**Attrition:** Just before participants begin making choices for each of their 20 possible matches, we randomize the 1,109 participants who remain to either see profiles that include matches' climate beliefs or not. A total of 1,023 participants (76% of those who start the experiment and 92% of those randomized) complete profile choices for all 20 possible returning participants, with no differential attrition by treatment arm (column 2, Appendix Table A20). These 1,023 participants compose our sample for analysis.

**Sample description:** Column 1 of Appendix Table A21 summarizes baseline traits among the Wave-1 A0 control group, while column 2 summarizes the same traits in the WTP sample. Comparing these columns, participants who ultimately email Congress and complete the WTP survey are more likely to be women, are somewhat more concerned about climate change, and have higher baseline political engagement.

**Balance:** The WTP sample is largely balanced across those see information on matches' climate beliefs and those who do not (columns 3 through 5, Appendix Table A21). Our main analysis includes participant fixed effects, controlling for any remaining baseline differences.

**Set-up comprehension:** We invest heavily in making sure that participants understand the WTP experimental set-up. After describing the set-up once, we go through it again while asking a series of 8 incentivized comprehension questions and correcting participants' answers. Participants answer an average of 5.8 comprehension questions correctly, and Appendix Figure A27 plots the distribution of number of questions that participants initially answered correctly; note that we reiterate the correct answers immediately after each question. We show later that all of our main results are robust to participants who initially answered at least half of the 8 comprehension questions correctly.

### 6.3 Empirical specifications

We estimate partisan gaps in WTP participants' attempts to influence others in the following specification:

$$Extended_{ij} = \alpha_i + \beta_1 Before_{ij} + \beta_2 Repub_{ij} + \beta_3 Before_{ij} * Repub_{ij} + \Theta P_{ij} + \varepsilon_{ij} \quad (4)$$

where $Extended_{ij}$ indicates that WTP participant $i$ chooses to show possible match $j$ an extended profile saying that they emailed Congress, $Before_{ij}$ indicates that possible match $j$ would see participant $i$'s profile before deciding whether to email Congress or not, and $Repub_{ij}$ indicates that possible match $j$ leans towards the Republican party.

[32] We also recruited some participants to take a survey equivalent to that completed by A0 participants, but who were not included in Wave-1 and instead funneled directly into the WTP experiment.

Here, $\beta_1$ captures whether Democrats in the WTP sample try to influence other liberals: whether they are more or less likely to tell a liberal match that they emailed Congress when it can affect whether that participant does the same. $\beta_2$ captures whether Democrats are more or less likely to show a conservative, rather than a liberal, an extended profile when it cannot affect their in-survey action. Finally, $\beta_3$ captures whether WTP participants differentially try to influence matches who are conservative, rather than liberal.

**Control variables:** Because each WTP participant makes 20 binary choices, our main regression specifications will include participant fixed effects, $\alpha_i$. We also control for $P_{ij}$, a vector of controls for other features of possible match $j$. While $P_{ij}$ only includes choice number in our main specifications, our results are fully robust to controlling for other features of match $j$'s profile (Section 8.3).

### 6.4 WTP Results: Democrats differentially try to mobilize other liberals

**Overall influence motives in the WTP set-up:** We find that WTP participants strongly value opportunities to invite others to join in emailing Congress. Overall, participants choose to pass on extended profiles about 23% of the time when matches would see them after deciding whether to email Congress or not. When matches would see their profile *before* deciding whether to email Congress or not, on the other hand, WTP participants are 42 pp and 48 pp more likely to choose to show they emailed Congress, without and with climate-belief information, respectively (columns 1 and 3, Table 5).[33]

**The partisan influence gap:** WTP participants are much less likely to try to influence conservatives, however. When participants don't know that all possible matches believe climate change is human-caused (column 2), WTP participants are 54 pp more likely to show a liberal an extended profile when it can affect their in-survey action; this "influence effect" is 24 pp (45%) lower when the Before match is conservative. The partisan gap in influence effects falls when WTP participants know that all possible matches believe climate change is human-caused, but the influence effect remains 11.5 pp (21%) smaller for conservative than liberal matches (column 4).

Alongside these gaps in attempts to influence liberals versus conservatives, Democrats are less likely to tell conservative matches that they emailed Congress even when the profiles cannot affect those matches' action. When WTP participants do not know that all matches believe climate change is mostly human-caused, they are 13.5 pp (45%) less likely to choose an extended profile for an After match (whose in-survey action they cannot effect) when that match is conservative, rather than liberal. This partisan gap falls to 3.7pp (15%) when climate beliefs are revealed. These partisan gaps could arise from differential social-image concerns, efforts to affect matches' action after the survey, or affective benefits of interaction.

**Politics dwarfs heterogeneity by state and other match traits:** WTP participants much more strongly target extended profiles by partisanship than by any other traits shown in possible matches' profiles. Here,

[33] One key question in interpreting this result is whether WTP participants value carbon offsets. To explore this concern, we randomize 132 participants whom we randomized to choose between passing on a basic demographic profile plus a carbon-offset donation, passing on an extended profile, and passing on a basic demographic profile plus receiving a take-home gift card that could be redeemed at hundreds of stores. Appendix Figure A27a plots the shares of finished participants in the Money group who choose offsets or gift cards in 0 through 20 choices. While participants chose the basic profile and offset donation in an average of 10.5 choices, they chose the basic profile and gift card in an average of 1.2 choices; about 80% of participants never chose the take-home gift card.

we estimate versions of Equation 4 in which $P_{ij}$ includes indicators that possible match $j$ falls in a range of demographic categories and their interactions with $Before_{ij}$, separately in samples with and without information on possible matches' climate beliefs. Figure 3 and Appendix Tables A24 and A25 present the interaction coefficients estimated in samples without and with climate information, respectively.

When WTP participants lack information on possible matches' climate beliefs, they more strongly try to influence participants who are women or live in blue or purple states, and our point estimates suggest that they may differentially try to mobilize those with 4-year college degrees. However, these gaps are much smaller in magnitude than participants' differential efforts to mobilize liberals. Informing participants that all possible matches believe climate change is mostly human-caused eliminates or attenuates all of these differentials, consistent with the idea that participants may use possible targets' demographics as signals of their likely stances on climate change. Indeed, each of these traits is associated across with US with stronger beliefs in anthropogenic climate change (Hornsey et al. 2016, Marlon et al. 2022). Notably, there is no evidence that Democrats differentially try to mobilize climate advocacy in red or purple states, where this advocacy might be more impactful.

**Politics dwarfs heterogeneity by non-political similarity:** Just as WTP participants more strongly target invitations by targets' political leanings than by any other traits, they also respond much more to political similarity than to similarity on any other observable measure (Figure 4). Again, we estimate versions of Equation 4 in which $P_{ij}$ includes indicators that WTP participant $i$ and possible match $j$ share an age decade, whether they have a 4-year college degree, gender, state group of residence, and political leanings, as well as the interactions of each indicator with $Before_{ij}$. Figure 4 and Appendix Tables A26 and A27 present the interaction coefficients estimated in samples without and with climate information, respectively.

When WTP participants lack information on matches' climate beliefs, they are 6-7 pp more likely to try to influence matches with whom they share state or gender. While notable, these effects are only about 25% of the differential influence attempts when participants match on political party. Moreover, these differential influence attempts by gender and state matches fall in magnitude and become statistically insignificant when participants have information about possible matches' climate beliefs, while differential influence attempts by partisan leaning strongly persist.[34]

## 7 Follow-up experiments: Decomposing partisan influence gaps

Why do Democrats differentially try to mobilize other liberals for climate action, rather than conservatives, even as they agree that a bipartisan climate movement would be more effective? In this section, we return to Section 3's conceptual framework to decompose the beliefs and preferences that underlie liberals' reluctance to reach across party lines. We find no evidence that these gaps arise from Democrats' distaste for engaging with counter-partisans. Rather, these gaps may be fully explained by Democrats' beliefs that their invitations

[34] We find similar patterns of heterogeneity with respect to a continuous measure of predicted non-political similarity between WTP participants and each possible match (Appendix Tables A26 and A27, columns 6 and 7). We calculate this index by predicting non-political similarity from coefficients on demographic matches estimated in the full Wave-1 sample (Appendix Figure A11), subtracting predicted similarity from partisan match, and then standardizing this variable in the WTP sample. WTP participants' influence attempts significantly increase with overall predicted non-political similarity, but only in the absence of climate-belief information.

more effectively mobilize action by liberals than by conservatives. On the other hand, second-order affective polarization shapes Democrats' influence beliefs: Democrats expect to have more influence on conservatives when their invitations hide their own political leanings.

### 7.1 Accurate partisan gaps in influence beliefs: $\Delta P$

**Eliciting Democrats' influence beliefs:** We first consider participants' beliefs about how much invitations affect action, $\Delta P$. We elicit Democrats' beliefs about the impacts of their invitations on action by liberals or conservatives in a "belief sample" of 521 Democrats recruited with Wave 1. Like all Wave-1 participants, each participant in the belief sample belongs to a demographic cell that has been randomly matched to a parallel demographic cell of Wave-2 participants (Section 2.2). During the belief survey, we ask each participant to estimate how many out of 100 liberal and 100 conservative participants in their matched Wave-2 demographic group would choose to email Congress if they either did or did not see an invitation (Figure 1a) from a member of the belief participant's own demographic cell.[35] In total, 397 participants complete all four belief elicitations, of whom 194 were incentivized for accuracy. We focus throughout this section on belief elicitations from incentivized participants.[36] We then estimate the following regression:

$$ProbContact_{ij} = \alpha_i + \beta_1 Profile_{ij} + \beta_2 Repub_{ij} + \beta_3 Profile_{ij} * Repub_{ij} + \Theta P_{ij} + \varepsilon_{ij} \quad (5)$$

where $ProbContact_{ij}$ gives belief participant $i$'s estimates of how many of 100 participants in group $j$ would email Congress, $Profile_{ij}$ indicates that the belief elicitation is in the case where group $j$ sees invitations from members of participant $i$'s demographic group, and $Repub_{ij}$ indicates that group $j$ leans towards the Republican party. Here, $\beta_1$ captures the impacts of invitations on liberals' action, while $\beta_3$ captures any differential impact on conservatives.

**Democrats accurately perceive lower $\Delta P$ for conservatives:** The left panel of Figure 5 plots participants' estimates of the share of Wave-2 participants who would email Congress, split by political leanings and whether they see an invitation from an earlier participant; the right panel plots the true rates of action among Wave-2 participants. This comparison reveals several key points. First, the belief sample over-estimates rates of action across all groups. On the other hand, their beliefs about the absolute impacts of invitations on Wave-2 liberals and conservatives are approximately correct. The belief sample estimates that seeing an invitation would make Wave-2 liberals and conservatives about 6.2pp and 3.2pp more likely to email Congress, respectively, while we estimate true values in the Wave-2 sample of 5.8pp and 2.1pp, respectively. We cannot reject here that the belief sample's beliefs are accurate on average.[37]

[35] Note that because we will actually pass on invitations from the belief participant's demographic group to conservative and liberal members of their paired Wave-2 demographic group, we can measure these true rates of action and incentivize accurate guesses. In particular, we tell half of the belief sample that we will randomly choose 20 participants, randomly select one of their four guesses, and send them a gift card for $15 if their guess is within 10 of the correct answer.

[36] Appendix Table A28 compares the demographics of this belief sample with the Wave-1 action-experiment sample and the WTP sample. Participants who complete the belief elicitations are younger, wealthier, and more likely to be men than the Wave-1 sample overall, but they match the Wave-1 sample on climate beliefs, political beliefs, and baseline political engagement. Like the main Wave-1 sample, the belief sample is less politically engaged and concerned about climate change than the WTP sample.

[37] In contrast to these accurate beliefs about the role of politics, Democrats underestimate how much non-political similarity between senders and recipients increases invitations' impacts (Appendix Section F.1.1).

## 7.2 No partisan gap in preferences for emails with certainty

While Democrats' believe that their invitations have about twice as much impact on liberals' action as on conservatives' action, their efforts to differentially recruit liberals could reflect underlying preferences in addition to these beliefs. We next isolate preferences from beliefs by measuring Democrats' preference for emails from liberals versus conservatives *when they can be obtained with certainty.* We find no evidence that Democrats either believe emails from individual conservatives or liberals are differentially impactful ($V(Email\ Impact|D,R)$) or have differential preferences for having liberals versus conservatives engaged in the climate movement ($A(Target\ Involved|D,R)$).

### 7.2.1 The certainty experiment: Experimental design

We run two rounds of a "certainty experiment," in which Democrats make incentivized binary choices between carbon-offset donations and emails to Congress, sent with certainty, from a range of demographic groups. While these rounds differ slightly in experimental design, their basic structure remains the same and we pool the data for analysis. In both rounds of the certainty experiment, we show participants a series of profiles for 10 to 14 demographic groups (Appendix Figure A17); these profiles are evenly split between leaning towards the Republican and Democratic parties.[38] For each demographic group, certainty participants choose between two options:

*Option 1. Carbon offset donation:* The research team donates $8 and $16 to carbon offsets. As in the WTP experiment, we benchmark the dollar offset amount to driving distances (see footnote 30).

*Option 2. Enlist an email with certainty:* We enlist a participant like the demographic group shown to email Congress about climate policy through our form. We reiterate to participants that choosing this option means that we would enlist someone to write a letter with certainty, and that it is easy for us to enlist someone like each group shown.[39] (See Appendix F.2.1 for the full text of the survey explanation.)

**Incentivized choices:** We incentivize these choices by telling participants that we will implement one choice for 20 random survey-takers in the first certainty round and for 10 random survey-takers in the second certainty round.

**Recruitment and sample summary:** In total, 459 participants and 574 participants complete all 10 choices in rounds 1 and 2 of the certainty experiment, respectively. See Appendix Section F.2.3 for more detail on the recruitment process. Like the belief sample, participants in the final certainty sample are somewhat younger and wealthier than the main Wave-1 action sample (Appendix Table A28); certainty participants

[38] While participants in the first-round certainty experiment see 10 demographic profiles, those in the second round see 14 profiles. In the first round of the certainty experiment, we constructed these sample profiles from full profiles of past study participants, including politics. In the second round, we instead cross-randomized politics with respect to non-political demographic profiles of past study participants. See Appendix F.2.2 for more details on the process of creating these profiles and for details on the design differences between the two experimental rounds.

[39] Throughout this section, we state only that we will enlist someone "like" each group shown, rather than necessarily enlisting someone who matches all of the demographics shown; doing so could be prohibitively costly for us to implement. For the sake of our experimental design, the only requirement is that participants expect us to enlist someone matching each demographic trait shown with enough certainty to react to those traits.

are also more likely to be women and more likely to have a college degree. However, they match the Wave-1 sample in climate beliefs, political beliefs, and political engagement.[40]

**Specifications:** We run the following simple regressions in the certainty sample:

$$ChooseEmail_{ij} = \alpha_i + \beta_1 \, Repub_{ij} + \beta_2 \, Woman_{ij} + \beta_3 \, College_{ij} + \sum_{a=2}^{4} \gamma_a \, Age_{ija} + \sum_{s=2}^{3} \delta_s \, State_{ijs} + \varepsilon_{ij}$$

where $ChooseEmail_{ij}$ indicates that certainty participant $i$ chooses an email from demographic group $j$ over a carbon offset donation, $\alpha_i$ are participant fixed effects, and $Repub_{ij}$, $Woman_{ij}$, and $College_{ij}$ indicate that members of demographic group $j$ lean towards the Republican party, identify as women, and have a 4-year college degree, respectively. $Age_{ij2}$, $Age_{ij3}$, and $Age_{ij4}$ are 15-year age bins, and $State_{ij2}$ and $State_{ij2}$ indicate that members of demographic group $j$ live in a state group that we classify as blue or purple, respectively (relative to red states). We interpret the set of $\beta$, $\gamma$, and $\delta$ coefficients as capturing Democrats' relative preferences for climate emails to Congress from Americans of varying demographic traits and to legislators in states across the political spectrum.

#### 7.2.2 Democrats choose emails differentially by state, but not by partisanship

Figure 6 plots our our estimated point estimates in the combined certainty sample, and Appendix Table A32 presents them in table form. Democrats show strong preferences over the states in which emails are sent: they are about 11pp less likely to choose an email over offsets for senders in blue states than red states, a decrease of 17% of the mean for possible senders in red states. In turn, they are 5pp more likely to choose an email from senders in purple states than red states, an increase of about 7%. This pattern suggest that when Democrats can choose emails with certainty, they do so strategically: participants target letters to red and purple states, where they could potentially help to increase support for climate policy, relative to blue states, where past survey evidence suggests Democrats think legislators will support climate policy regardless of citizen advocacy (Appendix Section B.2).

On the other hand, Democrats show no preferences for emails by senders' other demographics conditional on state: there are no distinguishable partisan gaps in email choices, either across the full set of possible email-writers or separately in red, blue, or purple states (Appendix Table A32).

#### 7.2.3 No detectable gaps in $V(Email\ Impact)$ or $A(Target\ Involved)$

Our conceptual framework (Section 3) decomposes how much Democrats value an email by a given sender as the sum of how much they value the email's perceived impact—$V(Email\ Impact)$—and the affective value of knowing that the sender is involved in the climate movement—$A(Target\ Involved)$. Then, Democrats may not differentially choose emails from liberal or conservatives either because they both perceive no differential impacts and derive no differential emotional benefits by partisanship, or because partisan gaps in these two measures cancel out.

While our motivating survey suggests that Democrats expect letters from conservatives to be more impactful in achieving climate policy progress than letters from liberals in the aggregate, Democrats in our

[40] Samples in the two rounds of the certainty experiment differ somewhat in demographics, climate beliefs, and political attitudes (Appendix Table A31). For example, participants in the second-round certainty sample are substantially more likely to be women and over age 40 than those in the first round.

sample may not perceive or attend to this differential impact when considering emails from individual Americans across the political spectrum. To clarify these determinants, we ask participants in both rounds of the certainty experiment to rate how impactful they would expect an email to Congress to be from one particular demographic group.[41] In Appendix Table A32, we regress these standardized impact beliefs on the set of email-writer traits.

We find no gap in perceived impact between liberals and conservatives when pooled across state groups (column 5), though we find marginally-significant evidence that Democrats perceive emails from conservatives to be more impactful than those from liberals in red states (column 6). While we interpret these impact measures with caution,[42] they suggest that Democrats do not perceive differential impact from emails by liberals versus conservatives when making choices over individual emails. Combining the null partisan gap in email choices with the null partisan gap in perceived impacts, therefore, we find no evidence that Democrats have differential affective preferences over having a liberal versus conservative engaged in climate action.[43]

### 7.3 But second-order affective polarization shapes $\Delta$ P

Considering the framework in Section 3, our results suggest so far that differential beliefs about $\Delta P(Email|D)$ and $\Delta P(Email|R)$ likely contribute to the gaps in Democrats' efforts to reach out to liberals versus conservatives, while differential beliefs about the impacts of emails, $V(Target\ Email\ Impact|D,R)$, and differential preferences for having a liberal or conservative engaged in climate action, $A(Target\ Involved|D,R)$, do not.

However, affective polarization could still shape the partisan invitation gap in two key ways. First, Democrats' beliefs about $\Delta P$ could themselves be shaped by affective polarization: Democrats may expect to have less impact on conservatives' action precisely because they expect those conservatives to be affectively polarized against them. Moreover, even if Democrats have no differential preference for emails from liberals versus conservatives when enlisted by us, the research team, they may still have differential preferences over directly trying to engage liberals versus conservatives in action during the survey, $A(Try\ Influence\ During|D,R)$.

**Revised WTP experiment:** To explore these remaining questions, we run a revised WTP experiment that introduces one key variation: we randomize some participants to have the option to hide their party leanings

[41] We phrase this question differently in the two rounds of the certainty experiment. In the first round, we simply ask participants to rate how impactful a personalized email from someone in that group to their national representatives would be in helping to enact climate policy, on a scale from 1 (Not impactful at all) to 7 (Extremely impactful.) In the second round, we ask participants to imagine that a climate bill were introduced to Congress in November 2023, and that 20 people in the given demographic group sent personalized emails to their national representatives in Congress about the bill via our form. Asking participants to assume that their national representatives read the personalized emails, we then ask them to report how these emails affect whether those representatives support the climate bill, on a scale from 1 (Make much less likely to support) to 11 (Make much more likely to support). We standardize these variables separately in the two rounds of the certainty experiment and combine them in our main analysis.

[42] According to these measures, Democrats actually rate emails from Americans in blue and purple states as more effective than from those in red states, even while they are less likely to choose emails from Americans in blue states. In our simple model of how much Democrats value emails from certain types of Americans (Section 3), this pattern would suggest the unlikely conclusion that Democrats have a strong dislike for engaging those from blue states in the climate movement. More likely, we believe, is that our measure of perceived email impact captures a heuristic of impact—for example, that Republican legislators will categorically ignore constituents advocating for climate change—that differs from what participants have in mind as they make choices between emails and carbon-offset donations.

[43] Moreover, we do not find that Democrats' choices over emails from liberals or conservatives vary systematically with a range of measures of affective polarization (Appendix Figure A34).

from the profiles they pass on to each possible match. This variation allows us to test the role of second-order affective polarization by measuring WTP participants' beliefs about the impacts of their invitations on conservatives' and liberals' actions when those invitations include or hide their own political leanings. In theory, this treatment variation could also effectively instrument for the impact of participants' beliefs about $\Delta P(Email|D) - \Delta P(Email|R)$ on the partisan invitation gap; we could then estimate whether closing the partisan belief gap would close the gap in Democrats' efforts to influence liberals versus conservatives. However, we find that allowing participants to hide their politics generates only a very small first stage effect on influence beliefs, ruling out this strategy.

#### 7.3.1 Experimental design, recruitment, and fidelity

**Experimental structure:** The round-two WTP experiment follows the same procedure as the main WTP experiment described in Section 6.1, with two key alterations. First, while the original WTP experiment randomized some participants to see profiles of possible matches without information about their climate beliefs, all participants in the round-two WTP experiment see profiles that include matches' climate beliefs. Next, we randomize half of round-two WTP participants to have the option to hide their own political leanings from the basic or extended profiles that they pass on to each possible match. (See Appendix Section F.3.1 for the explanation to participants). Participants with the option to hide politics choose whether to do so or not separately for each possible match on the same screen on which they choose whether to pass on a basic or extended profile (Appendix Section F.3.2).

**Recruitment:** We recruit participants for the round-two WTP experiment using ads on Facebook. We invite participants to take the additional WTP survey section if they report that they emailed Congress, and 1,420 participants start the round-two WTP survey in total. Appendix Section F.3.4 details recruitment.

**Attrition:** Midway through explaining the WTP experimental set-up, we randomize the 1,278 participants who remain to either have or not have the option to hide politics. A total of 995 participants (70% of those who start the experiment and 78% of those randomized) complete all 20 binary choices, with no differential attrition by treatment arm (column 3, Appendix Table A20).

**Sample description and balance:** Columns 9 through 12 of Appendix Table A28 summarize the demographics of the round-2 WTP sample. Like the main WTP sample, these participants are more concerned about climate change and politically engaged than the main Wave-1 sample (column 10). The round-two WTP sample is older and somewhat less likely to identify as women than the main WTP sample, but it matches this sample in climate beliefs, political beliefs, and political engagement (column 11). The sample is fully balanced across treatment arms (Appendix Table A33).

#### 7.3.2 The role of second-order affective polarization

Both WTP participants' choices to hide or show their own political leanings and their stated beliefs about the influence of invitations on conservative and liberal matches suggest that second-order affective polarization plays a key role in efforts to reach across the political aisle.

**Choices to hide politics:** WTP participants are much less likely to show conservative matches their own political leanings (Figure 7). While participants show their politics to about 91% of liberal matches, regardless of when those matches would see their profile, they show their politics to only 34% and 44% of

conservatives matches who would see their profiles before and after deciding whether to email Congress, respectively. These patterns suggest that Democrats may differentially hide their politics from conservatives both due to social-image concerns and to increase their impacts on conservatives' action.

**Influence beliefs:** Indeed, Democrats believe that their invitations will have mcuh higher impact on conservatives' action if they hide their own political leanings. At the end of the Round-2 WTP survey, we randomize half of participants to make unincentivized guesses for the probability that two of their possible matches would email Congress if they did or did not see an invitation (Figure 1a); we randomize whether participants make these predictions for invitations that do or do not include their own political leanings. Figure 8 plots participants' estimates for the probability that conservative and liberal matches would email Congress with and without seeing a profile that did or did not show influencers' political leanings.

Democrats with the option to hide their politics estimate that an invitation without their own political leanings would make conservatives 11.4pp more likely to email Congress, while an invitation identifying them as a liberal would increase conservatives' action by only 3.4pp. On the other hand, they estimate that hiding their political leanings would somewhat decrease the impact of invitations on liberals' action from 13.3pp to 9.8pp. These patterns suggest that second-order affective polarization plays an important role in Democrats' beliefs about their ability to spread the climate movement across the political aisle: they expect conservatives to respond less to their invitations precisely because they are liberals.[44]

### 7.3.3 Can we instrument for the partisan belief gap with the option to hide politics?

Under reasonable assumptions, the ability to hide one's politics should only impact the partisan influence gap in the WTP experimental set-up (coefficient $\beta_3$ in equation 4) through its effects on $\Delta P(Email|D) - \Delta P(Email|R)$, the gap in Democrats' beliefs about their ability to influence conservatives versus liberals. Then, we could in theory use the round-two WTP experiment to benchmark the role of this partisan belief gap in Democrats' differential efforts to reach out to liberal versus conservative matches. Unfortunately, we find that allowing WTP participants to hide their politics in each profile does not substantially affect the partisan belief gap; thus, we cannot use this strategy.

**The effective first stage on beliefs:** Even while participants with the option to hide their politics predict that invitations with and without their politics would have substantially different impacts, we do not find that giving people the option to hide politics shrinks the partisan belief gap. This null effect arises because participants who cannot hide their politics are more optimistic about the impacts of baseline profiles on conservatives: while participants with the option to hide their politics estimate that an invitation showing their politics would make a conservative only 3.2pp more likely to email Congress (Figure 8), participants who cannot hide their politics estimate that the same invitation would make conservatives 8.5pp more likely to email Congress (Appendix Figure A36).[45] Taking these belief elicitations at face value and assuming that they are homogeneous within treatment groups, we would estimate that the partisan belief gap only falls from

[44] These belief gaps could in theory be explained by WTP participants expecting conservatives to rationally update less about the value of emailing Congress when they see a signal (invitation) from someone less similar to themselves. However, ... ?

[45] This divergence could arise for a range of reasons. For example, participants with the option to hide their politics may formulate more precise and differentiated beliefs about the impacts of profiles with and without political leanings over the course of the binary WTP choices, or those without the option to hide politics may overstate the impacts of baseline invitations via motivated reasoning.

6.1pp among those without the option to hide politics to 4.5pp among those who can hide politics. Moreover, the large divergence in beliefs about baseline profiles' impacts across the treatment groups suggests that it may be unreasonable to directly compare the elicited beliefs across these groups.

**No impact on influence attempts:** Consistent with the null first-stage effects on participants' beliefs about their relative impact on liberal versus conservative matches, the option to hide one's politics does not significantly reduce the gap in WTP participants' efforts to mobilize liberal versus conservative matches (Figure 9). With or without the option to hide one's politics, Democrats are about 19% less likely to try to influence conservatives than liberals. We cannot distinguish whether this null effect arises from a true null effect on how much influence participants believe they can have on liberals versus conservatives, or because Democrats have differential preferences over directly trying to engage liberals versus conservatives in action during the survey, $A(Try\ Influence\ During|D,R)$. Nevertheless, our results here are fully consistent with Democrats' beliefs about their impacts on liberals versus conservatives driving their reluctance to reach across the aisle.

### 7.4 Summing up: Drivers of the partisan influence gap

Altogether, we find no evidence that Democrats' own affective polarization holds back their efforts to try mobilizing conservatives for political action; when emails can be obtained with certainty from different groups, Democrats target action to more strategically-valuable states, with no preference for letters from liberals or conservatives per se. Rather, Democrats differentially try to mobilize other liberals for action because they believe that they can more strongly affect their action. These influence beliefs are shaped by second-order affective polarization: Democrats believe that they can influence conservatives less because of their own partisan affiliation.

## 8 Robustness

Finally, this section shows that the main results across the Wave-2, Wave-1, and WTP analysis are highly robust to changes in sample definitions, control-variable specifications, and adjustments for experimenter demand effects.

### 8.1 Robustness of the Wave-2 results

**Sample definitions:** Appendix Figures A13 and A14 show the robustness of the main Wave-2 results. Our main Wave-2 estimates restrict the experimental sample to the roughly 92% of those randomized for whom we observe explicit choices to email Congress or not (Section 4.1.3). In our Wave-1 analysis, however, we will deal with differential attrition by assuming that all participants who leave the experimental survey after seeing a preview of the upcoming email opportunity would not have emailed Congress had they continued. To make the Wave-1 and Wave-2 results as comparable as possible, we show in the specification charts that our Wave-2 results are fully robust to expanding the experimental sample via this assumption.[46]

[46] As we will argue for the Wave-1 sample, the assumption that those who leave the experimental survey after the email preview would not have emailed Congress appears to be reasonable in Wave 2. Appendix Section C.4 discusses the validity of this assumption in detail.

Next, while we pre-registered a total Wave-2 sample size of about 4,250 liberals, our recruitment on social media yielded a total of 5,200 liberals who saw the email preview, and thus for whom we can construct a filled-in email outcome, and 5,027 for whom we observe explicit choices to email Congress or not. Our results are fully robust to restricting the liberal sample to the first 4,250 participants either for whom we observe direct email choices or who saw the email preview. While we pre-registered that we would recruit 3,250 conservatives, our budget ultimately allowed us to recruit only 2,960.

Finally, our results are largely robust to restricting to the participants who correctly answer whether they know if their paired Wave-1 match emailed Congress when they took our survey (Section 4.1.4). We only observe this comprehension question for a random half of Wave-2 participants and restrict to the roughly 74% and 78% of control and treatment participants who answer it correctly, respectively.

**Control variables:** Our Wave-2 estimates are also fully robust the following control specifications: no controls, only demographic controls, the full set of controls for demographics, baseline beliefs, and baseline political engagement that we include in our main regressions, and a set of controls selected by double-post Lasso regressions following Chernozhukov et al. (2018).[47]

**Experimenter demand effects:** Next, our Wave-2 estimates are robust to accounting for experimenter demand effects. Strong demand effects are organically entwined in the Wave-2 intervention, which is a direct invitation to join in emailing Congress. Indeed, participants assigned to see a Wave-1 invitation report 0.2sd higher demand on a 6-point Likert-style measure of how strongly they think we (the researchers) wanted them to email Congress during the survey. While we take pains in the question statement to ask specifically about the researchers' demand, participants are unlikely to fully separate social pressure applied by us from social pressure applied by the Wave-1 inviter. Any demand effects at play inly threaten the validity of our estimate if they are differentially activated when participants know they are participating in an academic project.

While we cannot cleanly separate the demand effects that would be induced by an NGO's implementation versus our own implementation of the action invitations, we take several steps to ensure that any such experimenter demand effects do not drive our Wave-2 estimates. First, experimenter demand effects can only be at play among participants who recognize our research question or hypothesis. About 7% of the Wave-2 experimental sample guesses that our research question relates to peer effects or the invitations' impacts, comprising about 11% of the treatment group and 4% of the control group. Our results are fully robust to excluding these participants (Appendix Figures A13 and A14). Next, explicitly stating the research hypothesis to participants may shift real-stakes behavior by between 0.1sd and 0.3sd (Mummolo and Peterson 2019, de Quidt et al. 2018). Because we estimate impacts on a relatively costly form of action, we would expect participants to be even less responsive in our setting. In Appendix Figure A15, we show that our estimates change only slightly when we assume that recognizing the study purpose would either increase or decrease our main outcomes by 0.2sd.

**Robustness of the partisan gap:** Finally, we test that our results for the partisan gap in invitations' effects

[47] The Lasso procedure selected controls from the full set of controls for participants' demographics and baseline beliefs, as well as from separate dummy variables for each of the forms of past political engagement that we elicit, separately by political party and each sample restriction.

are robust to key potential confounders that are correlated with party affiliation in our sample (Section 4.1.3). Our goal here is not to identify the differential impact of invitations on liberals or conservatives who are otherwise identical; indeed, gaps in concern about climate change and education would be at play in real-world attempts to mobilize liberals versus conservatives for climate action. Rather, Appendix Figure A16 shows that the partisan gap we estimate is largely robust to controlling for the interaction of treatment status with key traits associated with our differential recruitment strategies for liberals and conservatives: whether a participant is recruited from Qualtrics, political engagement, and income.

## 8.2 Robustness of the Wave-1 results

**Sample definitions:** Appendix Figures A21 and A22 show the robustness of the main Wave-1 results. While we pre-registered a total Wave-1 sample size of about 8,200 participants, we did not specify whether this total would include or exclude participants who left the survey before explicitly choosing to email Congress or not. In practice, we terminated Wave-1 recruitment when 8,269 participants had explicitly chosen whether to email Congress or not. However, our main regression specifications include all of the 8,937 participants who were randomized into a Wave-1 treatment arm, assuming that those who attrit between randomization and the decision to email Congress would not have emailed Congress had they completed the survey (Section 5.1.2). Appendix Figures A21 and A22 plot Wave-1 estimates in a range of samples: the first 8,200 participants randomized into a Wave-1 treatment arm (filling in zeros for outcomes among those who attrit), all of the 8,269 participants for whom we observe an explicit choice to email Congress or not, and the first-recruited 8,200 participants for whom we observe an explicit choice to email Congress.

As expected, restricting the sample to those for whom we observe an explicit choice to email Congress substantially increases our estimates for the impacts of the A1 arm (Tell After) relative to A0, the pure control. This effect is largely mechanical, since participants in the A1 group are substantially less likely to finish the Wave-1 survey (Section 5.1.2) and thus were more likely to have outcome variables set to zero. However, this shift simply means that our main estimates for the impacts of A1 versus A0 are conservative. None of the sample redefinitions appreciably changes our estimates for the effect of A2 versus A1, our estimate of Democrats' efforts to influence others.

Next, we show that our results for the A2 versus A1 gap are robust to restricting to participants who correctly understand the experimental set-up. In particular, we re-estimate our main specifications among participants who correctly reported when Wave-2 participants would see that they emailed Congress, if they did so, and separately, among participants who correctly reported whether their choice to email Congress could affect whether their Wave-2 matches do the same.[48] Imposing these comprehension restrictions has only minimal effects on our main estimates.

**Control variables:** Next, our Wave-1 results are fully robust to the set of control variables we include. Appendix Figures A21 and A22 plot our main Wave-1 estimates with no controls, with only demographic

[48]Note that we asked these comprehension questions at the end of the Wave-1 action survey and only among A1 and A2 participants. Thus, we cannot test that our estimates for the effect of A1 relative to A0 are robust to this restriction. Restricting to those who understood profile timing keeps 67% of A1 participants and 75% of A2 participants, while restricting to those who understood the influence structure keeps 46% of A1 participants and 64% of A2 participants. Given this substantial differential selection, this robustness check is only suggestive.

controls, with the full set of controls for demographics, baseline beliefs, and baseline political engagement that we use in our main regressions, and with a set of controls selected by double-Lasso regressions. The Lasso procedure selects controls from the full set of participants' demographics, baseline beliefs, and set of separate dummy variables for a range of forms of past political engagement.

**Experimenter demand effects:** In the real world, NGOs trying to mobilize political advocacy would intentionally create strong demand effects. Nonetheless, we try to minimize demand effects throughout the Wave-1 action experiment by repeatedly telling participants that whether they email Congress does not hurt or help our research. Moreover, we do not explicitly tell participants in the A2 group that their action could affect what their paired Wave-2 participants do. We thus test whether participants try to influence others without encouragement to do so; as such, our estimates may under-estimate the degree to which Americans might act to mobilize others in a real-world NGO context.

For completeness, we show that our estimates of influence motives are robust to eliminating any differential demand effects that remain. On the same Likert-style demand measure we describe in Section 8.1, perceived experimenter demand is 0.06sd higher among A2 participants than A1 participants (columns 1 and 2 of Appendix Table A19). Our Wave-1 results are largely robust to adjusting for differential experimenter demand between A1 and A2. In 500 simulations, we randomly drop A2 participants who report experimenter demand at 6 on the 6-point scale until there is zero differential demand between the A1 and A2 groups; we then re-estimate our main Wave-1 regressions in each adjusted sample. Appendix Figure A23 plots the distribution of our point estimates for A2 versus A1 relative to the point estimate in our full sample. In each case, our point estimates fall on average by only a small amount when we correct for differential demand.

### 8.3 Robustness of the WTP results

**Sample definitions:** Our WTP results are fully robust to alternate sample definitions. In particular, Appendix Figures A31 through A33 plot our results both with and without information on possible matches' climate beliefs when we estimate our main regressions in the following adjusted samples: among participants who finished all 20 WTP choices in either the Basic or Money groups; excluding the 166 participants in our main analysis sample who answered fewer than 5 comprehension questions correctly; and excluding the 7 participants in our main sample who explicitly mentioned being concerned that conservatives would email Congress opposing climate policy. None of these restrictions appreciably changes our results.

**Control variables:** Next, our WTP results are fully robust to the set of control variables we include. In Appendix Figures A31 through A33, we first show that our main estimates are robust to including only individual fixed effects and controlling for choice number (matching columns 1 and 3 of Table 5), adding in controls for basic match traits and their interactions with $Before_{ij}$, and adding in a richer set of controls and interactions for possible targets' age groups and states. Next, we also show that our main results are robust to including Lasso-selected controls drawn from the full set of possible controls for possible match profiles, including dummy variables for avatar traits like hairstyle and color. We show in particular that the coefficients on $Repub_{ij}$ and $Before_{ij} * Repub_{ij}$ are robust to controlling for all traits selected by Lasso to predict $Repub_{ij}$, as well as for their interactions with $Before_{ij}$. Finally, we show that our results are robust to

excluding participant fixed effects and including the individual-level controls included in our main Wave-1 specifications.

# 9 Conclusion

In a large online experiment with about 25,000 participants recruited from social media and online panels, we create opportunities for Democrats to invite Americans across the country, demographic groups, and the political aisle to join them in political climate advocacy. Democrats are motivated to get others engaged in climate action, but they differentially reach out to other liberals rather than building a more bipartisan political coalition. Even as affective polarization reaches record highs, however, we find no evidence that this gap arises from a distaste for engaging with counter-partisans. Rather, Democrats' reluctance to reach across party lines can be fully explained by their accurate beliefs that their invitations have less impact on action by conservatives, rather than liberals. These beliefs themselves reflect second-order affective polarization: Democrats believe that they have higher impacts on conservatives when they hide their own political leanings. If beliefs are more malleable than preferences, our beliefs-based account of Democrats' influence gaps suggests that cross-party cooperation may be easier to achieve than the dominant narrative on affective polarization would suggest. On the other hand, Democrats' beliefs about their influence on liberals and conservatives are accurate in our setting, so no obvious belief-correction intervention would promote bipartisan outreach. These results suggest that parallel partisan movements, with liberals and conservatives each trying to engage co-partisans, may be efficient.

## References


**Aidt, Toke, Gabriel Leon-Ablan, and Max Satchell**, "The Social Dynamics of Collective Action: Evidence from the Diffusion of the Swing Riots, 1830-1831," *The Journal of Politics*, January 2022, *84* (1), 209–225.

**Allcott, Hunt**, "Social norms and energy conservation," *Journal of Public Economics*, October 2011, *95* (9), 1082–1095.

**Andre, Peter, Teodora Boneva, Felix Chopra, and Armin Falk**, "Fighting Climate Change: The Role of Norms, Preferences, and Moral Values," 2021, p. 59.

**Atske, Sara**, "As Economic Concerns Recede, Environmental Protection Rises on the Public's Policy Agenda," Technical Report, Pew Research Center February 2020.

**Bergan, Daniel E.**, "Does Grassroots Lobbying Work?: A Field Experiment Measuring the Effects of an e-Mail Lobbying Campaign on Legislative Behavior," *American Politics Research*, March 2009, *37* (2), 327–352.

_ **and Richard T. Cole**, "Call Your Legislator: A Field Experimental Study of the Impact of a Constituency Mobilization Campaign on Legislative Voting," *Political Behavior*, March 2015, *37* (1), 27–42.

**Bernard, René, Panagiota Tzamourani, and Michael Weber**, "Climate Change and Individual Behavior," September 2023.

**Bilen, David**, "Do carbon labels cause consumers to reduce their emissions? Evidence from a large-scale natural experiment," 2023.

**Bond, Robert M., Christopher J. Fariss, Jason J. Jones, Adam D. I. Kramer, Cameron Marlow, Jaime E. Settle, and James H. Fowler**, "A 61-million-person experiment in social influence and political mobilization," *Nature*, September 2012, *489* (7415), 295–298.

**Bordalo, Pedro, Marco Tabellini, and David Y. Yang**, "Issue Salience and Political Stereotypes," May 2020.

**Boxell, Levi, Matthew Gentzkow, and Jesse M. Shapiro**, "Cross-Country Trends in Affective Polarization," *The Review of Economics and Statistics*, January 2022, pp. 1–60.

**Broockman, David E. and Christopher Skovron**, "Bias in Perceptions of Public Opinion among Political Elites," *American Political Science Review*, August 2018, *112* (3), 542–563.

_ **and Timothy J. Ryan**, "Preaching to the Choir: Americans Prefer Communicating to Copartisan Elected Officials," *American Journal of Political Science*, 2016, *60* (4), 1093–1107.

**Bursztyn, Leonardo and David Y. Yang**, "Misperceptions About Others," *Annual Review of Economics*, 2022, *14* (1), 425–452.

_ , **Davide Cantoni, David Y. Yang, Noam Yuchtman, and Y. Jane Zhang**, "Persistent Political Engagement: Social Interactions and the Dynamics of Protest Movements," *American Economic Review: Insights*, June 2021, *3* (2), 233–250.

**Butler, Daniel M. and David W. Nickerson**, "Can Learning Constituency Opinion Affect How Legislators Vote? Results from a Field Experiment," *Quarterly Journal of Political Science*, August 2011, *6* (1), 55–83.

**Cantoni, Davide, David Y. Yang, Noam Yuchtman, and Y. Jane Zhang**, "Protests as Strategic Games: Experimental Evidence from Hong Kong's Antiauthoritarian Movement," *The Quarterly Journal of Economics*, May 2019, *134* (2), 1021–1077.

**Curry, James M. and Frances E. Lee**, *The limits of party: congress and lawmaking in a polarized era*, Chicago: The University of Chicago Press, 2020.

**Dechezleprêtre, Antoine, Adrien Fabre, Tobias Kruse, Bluebery Planterose, Ana Sanchez Chico, and Stefanie Stantcheva**, "Fighting Climate Change: International Attitudes Toward Climate Policies," July 2022.

**Dellavigna, Stefano, John A. List, Ulrike Malmendier, and Gautam Rao**, "Voting to Tell Others," *The Review of Economic Studies*, January 2017, *84* (1), 143–181.

**Dimant, Eugen**, "Hate Trumps Love: The Impact of Political Polarization on Social Preferences," *Management Science*, February 2023, p. mnsc.2023.4701.

**Drews, Stefan and Jeroen C.J.M. van den Bergh**, "What explains public support for climate policies? A review of empirical and experimental studies," *Climate Policy*, October 2016, *16* (7), 855–876.

**Esguerra, Emilio, Leonhard Vollmer, and Johannes Wimmer**, "Influence Motives in Social Signaling: Evidence from COVID-19 Vaccinations in Germany," *American Economic Review: Insights*, June 2023, *5* (2), 275–291.

**Fuong, Holly and Geoffrey Skelley**, "Do Democrats And Republicans Agree On Anything About Climate Change And Immigration?," Technical Report, FiveThirtyEight September 2022.

**García-Jimeno, Camilo, Angel Iglesias, and Pinar Yildirim**, "Information Networks and Collective Action: Evidence from the Women's Temperance Crusade," *American Economic Review*, January 2022, *112* (1), 41–80.

**Gerber, Alan S. and Donald P. Green**, "The Effects of Canvassing, Telephone Calls, and Direct Mail on Voter Turnout: A Field Experiment," *The American Political Science Review*, 2000, *94* (3), 653–663.

_ , **Gregory A. Huber, Albert H. Fang, and Andrew Gooch**, "The Generalizability of Social Pressure Effects on Turnout Across High-Salience Electoral Contexts: Field Experimental Evidence From 1.96 Million Citizens in 17 States," *American Politics Research*, July 2017, *45* (4), 533–559.

**Gift, Karen and Thomas Gift**, "Does Politics Influence Hiring? Evidence from a Randomized Experiment," *Political Behavior*, September 2015, *37* (3), 653–675.

**Gimpel, James G. and Iris S. Hui**, "Seeking politically compatible neighbors? The role of neighborhood partisan composition in residential sorting," *Political Geography*, September 2015, *48*, 130–142.

**González, Felipe**, "Collective action in networks: Evidence from the Chilean student movement," *Journal of Public Economics*, August 2020, *188*, 104220.

**Gould, Roger V.**, "Multiple Networks and Mobilization in the Paris Commune, 1871," *American Sociological Review*, 1991, *56* (6), 716–729.

_ , "Collective Action and Network Structure," *American Sociological Review*, 1993, *58* (2), 182–196. Publisher: [American Sociological Association, Sage Publications, Inc.].

**Graham, Jesse, Brian A. Nosek, and Jonathan Haidt**, "The Moral Stereotypes of Liberals and Conservatives: Exaggeration of Differences across the Political Spectrum," *PLoS One*, December 2012, *7* (12), e50092.

**Green, Donald P., Mary C. McGrath, and Peter M. Aronow**, "Field Experiments and the Study of Voter Turnout," *Journal of Elections, Public Opinion and Parties*, February 2013, *23* (1), 27–48.

**Hager, Anselm, Lukas Hensel, Johannes Hermle, and Christopher Roth**, "Group Size and Protest Mobilization across Movements and Countermovements," *American Political Science Review*, August 2022, *116* (3), 1051–1066.

**_ , _ , _ , and _** , "Political Activists as Free Riders: Evidence from a Natural Field Experiment," *The Economic Journal*, July 2023, *133* (653), 2068–2084.

**Han, Hahrie**, "The Organizational Roots of Political Activism: Field Experiments on Creating a Relational Context," *American Political Science Review*, May 2016, *110* (2), 296–307.

**_ , Aaron C. Sparks, and Nate Deshmukh Towery**, "Opening up the black box: citizen group strategies for engaging grassroots activism in the twenty-first century," *Interest Groups & Advocacy*, March 2017, *6* (1), 22–43.

**Hernández-Lagos, Pablo and Dylan Minor**, "Political Identity and Trust," *Quarterly Journal of Political Science*, July 2020, *15* (3), 337–367.

**Ho, Lisa and Lucy Page**, "Got Beef with Beef? Evidence from a Large-Scale Carbon Labeling Experiment," Technical Report 2023.

**Huber, Gregory A. and Neil Malhotra**, "Political Homophily in Social Relationships: Evidence from Online Dating Behavior," *The Journal of Politics*, January 2017, *79* (1), 269–283.

**Iyengar, Shanto and Masha Krupenkin**, "The Strengthening of Partisan Affect," *Political Psychology*, 2018, *39* (S1), 201–218.

_ **and Sean J. Westwood**, "Fear and Loathing across Party Lines: New Evidence on Group Polarization," *American Journal of Political Science*, 2015, *59* (3), 690–707.

_ **, Tobias Konitzer, and Kent Tedin**, "The Home as a Political Fortress: Family Agreement in an Era of Polarization," *The Journal of Politics*, October 2018, *80* (4), 1326–1338.

**Jerit, Jennifer and Jason Barabas**, "Partisan Perceptual Bias and the Information Environment," *The Journal of Politics*, 2012, *74* (3), 672–684.

**Jones, Jason J., Robert M. Bond, Eytan Bakshy, Dean Eckles, and James H. Fowler**, "Social influence and political mobilization: Further evidence from a randomized experiment in the 2012 U.S. presidential election," *PLOS ONE*, April 2017, *12* (4), e0173851.

**Jones, Robert, Natalie Jackson, Diana Orcés, Ian Huff, and Maddie Snodgrass**, "American Bubbles: Politics, Race, and Religion in Americans' Core Friendship Networks," Technical Report, Public Religion Research Institute 2022.

**Karlan, Dean and Margaret A. McConnell**, "Hey look at me: The effect of giving circles on giving," *Journal of Economic Behavior & Organization*, October 2014, *106*, 402–412.

**Klofstad, Casey A., Rose McDermott, and Peter K. Hatemi**, "The Dating Preferences of Liberals and Conservatives," *Political Behavior*, 2013, *35* (3), 519–538.

**Leiserowitz, Anthony, Edward Maibach, Seth Rosenthal, John Kotcher, Emily Goddard, Matthew Ballew, Jennifer Marlon, Marija Verner, Sanguk Lee, Jennifer Carman, Teresa Myers, Matthew Goldberg, and Nicholcas Badullovich**, "Climate Change in the American Mind: Politics & Policy, Spring 2023," Technical Report, Yale University and George Mason University, New Haven, CT 2023.

**Maestre-Andrés, Sara, Stefan Drews, and Jeroen van den Bergh**, "Perceived fairness and public acceptability of carbon pricing: a review of the literature," *Climate Policy*, October 2019, *19* (9), 1186–1204.

**Marks, Joseph, Eloise Copland, Eleanor Loh, Cass R. Sunstein, and Tali Sharot**, "Epistemic spillovers: Learning others' political views reduces the ability to assess and use their expertise in nonpolitical domains," *Cognition*, July 2019, *188*, 74–84.

**McAdam, Doug**, "Recruitment to High-Risk Activism: The Case of Freedom Summer," *American Journal of Sociology*, 1986, *92* (1), 64–90. Publisher: University of Chicago Press.

_ **and Ronnelle Paulsen**, "Specifying the Relationship Between Social Ties and Activism," *American Journal of Sociology*, 1993, *99* (3), 640–667. Publisher: University of Chicago Press.

**McConnell, Christopher, Yotam Margalit, Neil Malhotra, and Matthew Levendusky**, "The Economic Consequences of Partisanship in a Polarized Era," *American Journal of Political Science*, 2018, *62* (1), 5–18.

**Mildenberger, Matto and Dustin Tingley**, "Beliefs about Climate Beliefs: The Importance of Second-Order Opinions for Climate Politics," *British Journal of Political Science*, October 2019, *49* (4), 1279–1307.

**Munro, Geoffrey D., Terell P. Lasane, and Scott P. Leary**, "Political Partisan Prejudice: Selective Distortion and Weighting of Evaluative Categories in College Admissions Applications," *Journal of Applied Social Psychology*, 2010, *40* (9), 2434–2462.

**Nickerson, David W.**, "Is Voting Contagious? Evidence from Two Field Experiments," *American Political Science Review*, February 2008, *102* (1), 49–57.

**Oliphant, Baxter**, "10. Political engagement, knowledge and the midterms," Technical Report, Pew Research Center April 2018.

**Opp, Karl-Dieter and Christiane Gern**, "Dissident Groups, Personal Networks, and Spontaneous Cooperation: The East German Revolution of 1989," *American Sociological Review*, 1993, *58* (5), 659–680.

**Page, Lucy, Hannah Ruebeck, and James Walsh**, "The Narrative of Policy Change: Friction Builds Political Efficacy and Climate Action," Technical Report 2023.

**Peterson, Erik and Shanto Iyengar**, "Partisan Gaps in Political Information and Information-Seeking Behavior: Motivated Reasoning or Cheerleading?," *American Journal of Political Science*, January 2021, *65* (1), 133–147.

**_ , Sharad Goel, and Shanto Iyengar**, "Partisan selective exposure in online news consumption: evidence from the 2016 presidential campaign," *Political Science Research and Methods*, April 2021, *9* (2), 242–258.

**Pons, Vincent**, "Will a Five-Minute Discussion Change Your Mind? A Countrywide Experiment on Voter Choice in France," *American Economic Review*, June 2018, *108* (6), 1322–1363.

**Qin, Bei, David Strömberg, and Yanhui Wu**, "Social Media, Information Networks, and Protests in China," September 2019.

**Reinstein, David and Gerhard Riener**, "Reputation and influence in charitable giving: an experiment," *Theory and Decision*, February 2012, *72* (2), 221–243.

**Robbett, Andrea and Peter Hans Matthews**, "Polarization and Group Cooperation," *Quarterly Journal of Political Science*, April 2023, *18* (2), 215–241.

**Shafranek, Richard M.**, "Political Considerations in Nonpolitical Decisions: A Conjoint Analysis of Roommate Choice," *Political Behavior*, March 2021, *43* (1), 271–300.

**Turnbull-Dugarte, Stuart J., Joshua Townsley, Florian Foos, and Denise Baron**, "Mobilising support when the stakes are high: Mass emails affect constituent-to-legislator lobbying," *European Journal of Political Research*, 2022, *61* (2), 601–619.

**Tyson, Alec**, "On climate change, Republicans are open to some policy approaches, even as they assign the issue low priority," Technical Report, Pew Research Center 2021.

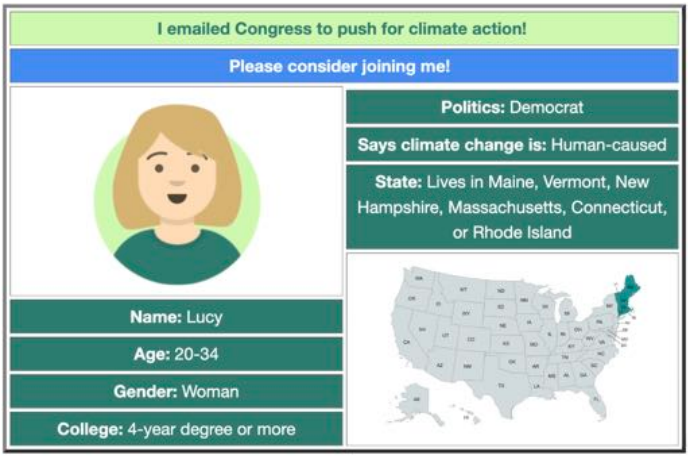
**Wintoki, M. Babajide and Yaoyi Xi**, "Partisan Bias in Fund Portfolios," *Journal of Financial and Quantitative Analysis*, August 2020, *55* (5), 1717–1754.

**Zhang, Yunhao and David G. Rand**, "Sincere or motivated? Partisan bias in advice-taking," *Judgment and Decision Making*, 2023, *18*, e29.

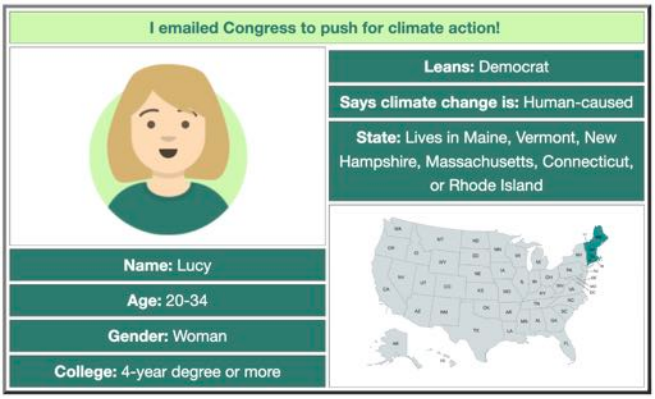
# 10 Main-text figures

Figure 1: Demographic profiles and invitations to act

(a) Sample invitation for Wave-2 participants

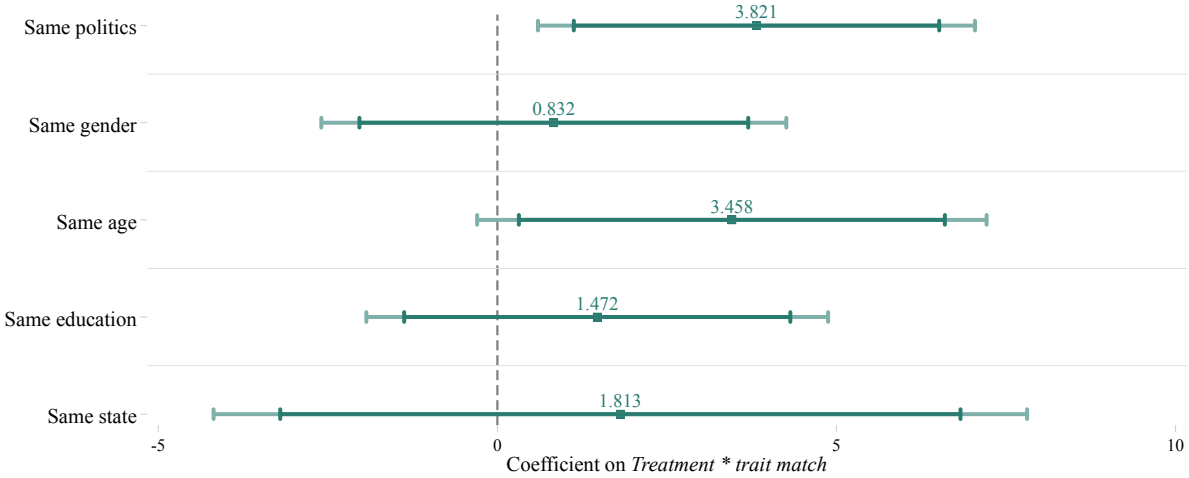



(b) Sample profile saying emailed Congress



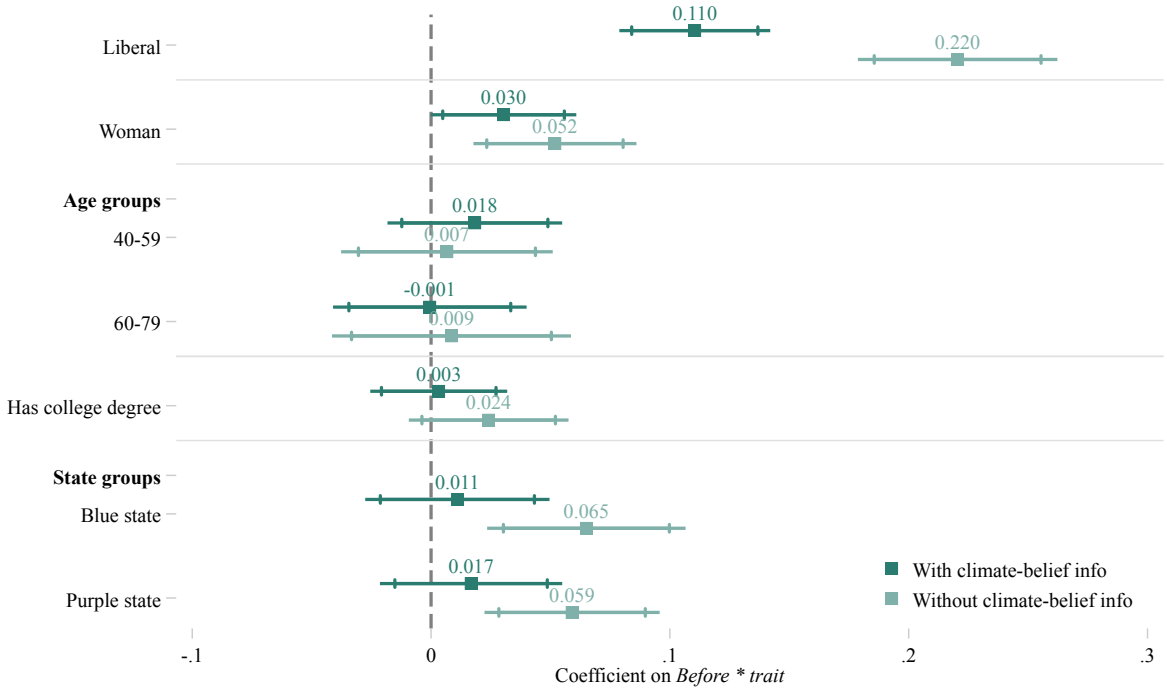

Note: Sub-figure (a) shows a sample invitation that Democratic influencers can send to future participants across a series of experimental designs. Sub-figure (b) shows a profile that Democrats can send to future participants saying that they emailed Congress, but which recipients would see after themselves deciding whether to email Congress or not. Given this timing, the profile in sub-figure (b) does not include a statement inviting the recipient to join in emailing Congress. Across several experimental designs, we isolate influence motives by comparing how often participants pass on invitations, like that in sub-figure (a), to how often they pass on profiles, like that in sub-figure (b), that cannot affect whether recipients' email Congress.

Figure 2: Heterogeneous influence effects by match similarity



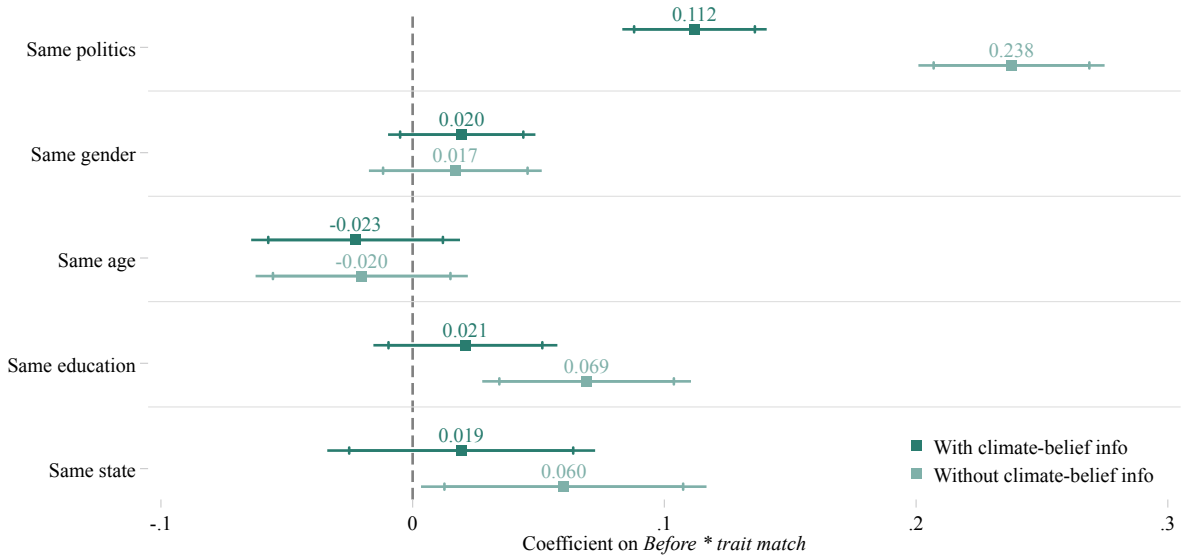

Note: This figure plots the differential impacts of Wave-1 invitations on whether Wave-2 participants match to email records when senders and recipients match on a range of demographic traits. We estimate this heterogeneity in versions of equation 2 where we include indicators that Wave-2 participant $i$ and their paired Wave-1 influencer match on political leanings, gender, age group, educational attainment, and state group of residence, as well as interactions of each dummy with $Treatment_i$. This figure plots the estimated coefficients on these interaction terms when we estimate them jointly in a single regression, which uses the same sample restrictions and control variables as in Table 2. Appendix Table A7 presents the full results for Wave-2 heterogeneity by similarity. The darker and lighter capped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively.

Figure 3: Differential WTP influence attempts by recipient traits



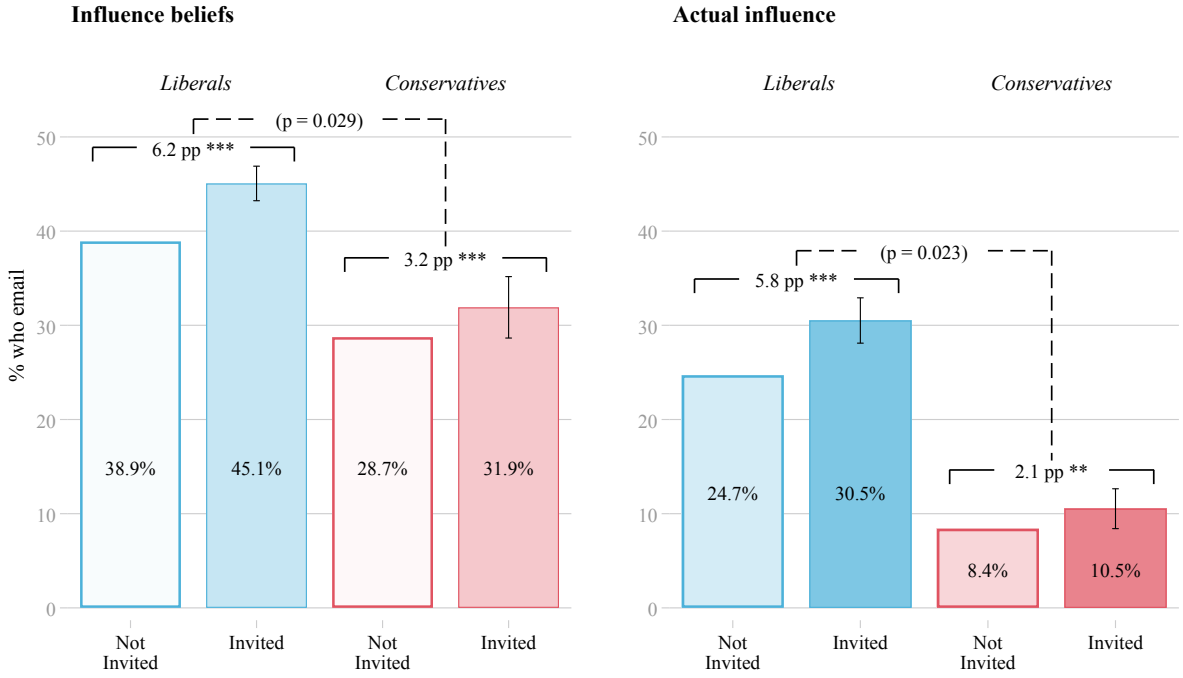

Note: This figure tests whether WTP participants differentially seek to mobilize possible matches with certain demographic traits. In particular, we estimate versions of equation 4 in which we interact $Before_{ij}$ with indicators that possible match $j$ leans towards the Democratic party, identifies as a woman, is ages 40-59 or 60-79 (relative to 25-39), has a college degree, and lives in a blue or purple state group (relative to a red state group), along with main effects for each of these traits. This figure plots the estimated coefficients on these interaction terms when we estimate them jointly in a single regression, with separate estimates for participants with and without information on possible matches' beliefs about climate change. These regressions use the same sample restrictions and control variables as in Table 5. Appendix Tables A24 and A25 present the full results among participants without and with climate-belief information, respectively. The capped and uncapped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively.

Figure 4: Differential WTP influence attempts by influencer-recipient similarity



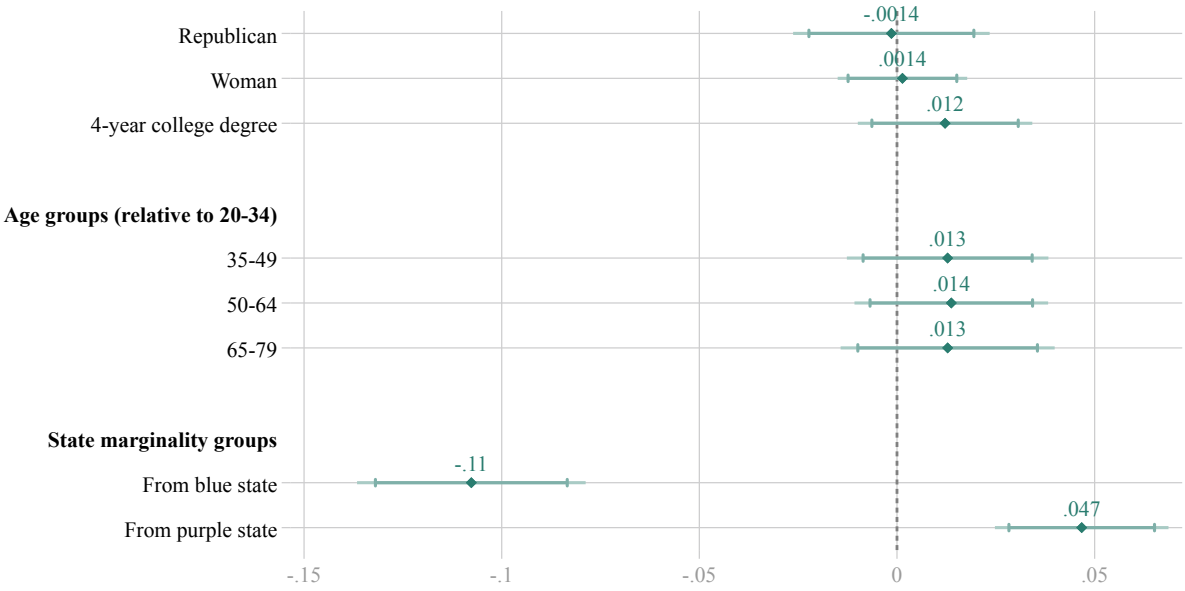

Note: This figure tests whether WTP participants differentially seek to mobilize possible matches with whom they share certain traits. In particular, we estimate versions of equation 4 in which we interact $Before_{ij}$ with indicators that possible match $j$ and WTP participant $i$ have the same political leanings, gender, age group, educational attainment, and state group, along with main effects for each of these traits. This figure plots the estimated coefficients on these interaction terms when we estimate them jointly in a single regression, with separate estimates for participants with and without information on possible matches' beliefs about climate change. These regressions use the same sample restrictions and control variables as in Table 5. Appendix Tables A26 and A27 present the full results among participants without and with climate-belief information, respectively. The capped and uncapped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively.

Figure 5: Overall influence beliefs by party affiliation, compared with the truth



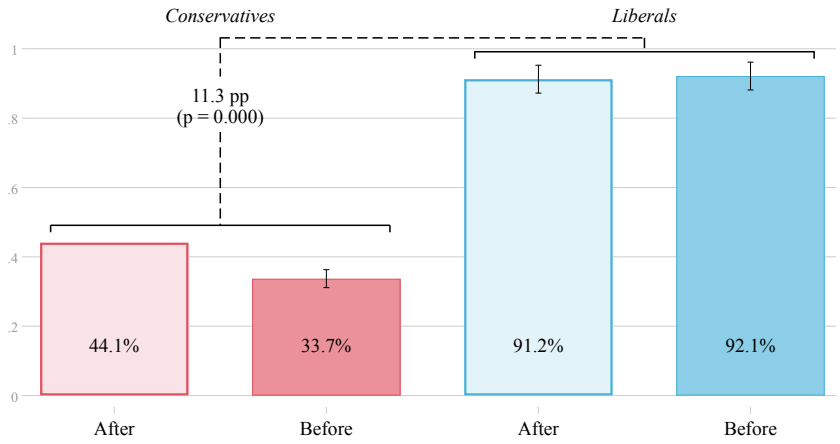

Note: The left panel of this figure plots average incentivized beliefs about the share of liberal and conservative Wave-2 participants who would email Congress if they do or do not see invitations from Wave-1 participants to join in action. We estimate these effects among 194 participants in the "belief sample," each of whom predict the share of liberal and conservative participants in their matched Wave-2 group who would email Congress if they did or did not see a Wave-1 invitation. The figure reports participants' beliefs for the impact of invitations separately among liberals and conservatives, as well as the heteroskedasticity-robust p-value from a test of the null that participants estimate the same imapct across these groups. The right panel of this figure plots the actual impacts of invitations on whether Wave-2 liberals and conservatives match to email records (Table 2). We estimate these impacts among 5,027 liberals and 2,954 conservatives in the Wave-2 experimental sample (Section 4). As for beliefs, the figure reports the heteroskedasticity-robust p-value from a test of the null that invitations have the same true impact on liberals and conservatives. Across the true and esitmated impacts of invitations on action, we indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

Figure 6: Determinants of choosing emails with certainty



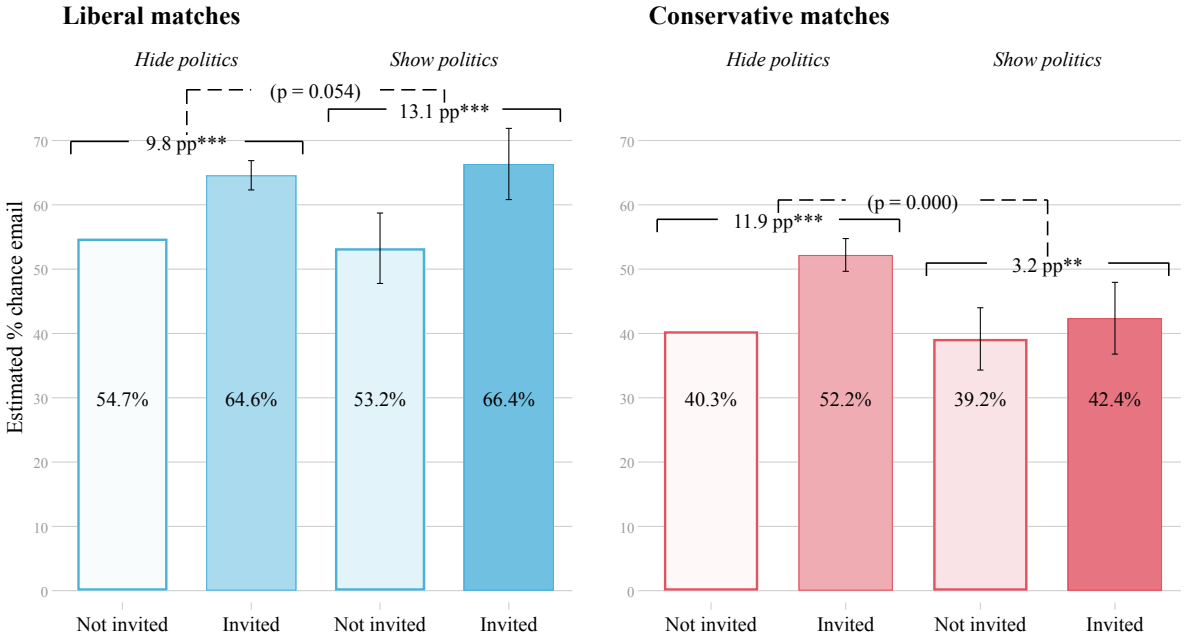

Note: In this figure, we regress whether participants choose an email enlisted with certainty from a given demographic group (instead of a carbon offset donation) on email-letter demographic traits. This regression includes 12,626 choices across 1,033 participants. In addition to estimating the role of the demographics shown above, we also control for individual fixed effects and indicators for choice number. We estimate the role of email-writer age groups relative to being between 20 and 34, and we estimate the role of email-writer state relative to living in a red state group. The capped and uncapped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively.

Figure 7: Choices to show politics in round-two WTP profiles



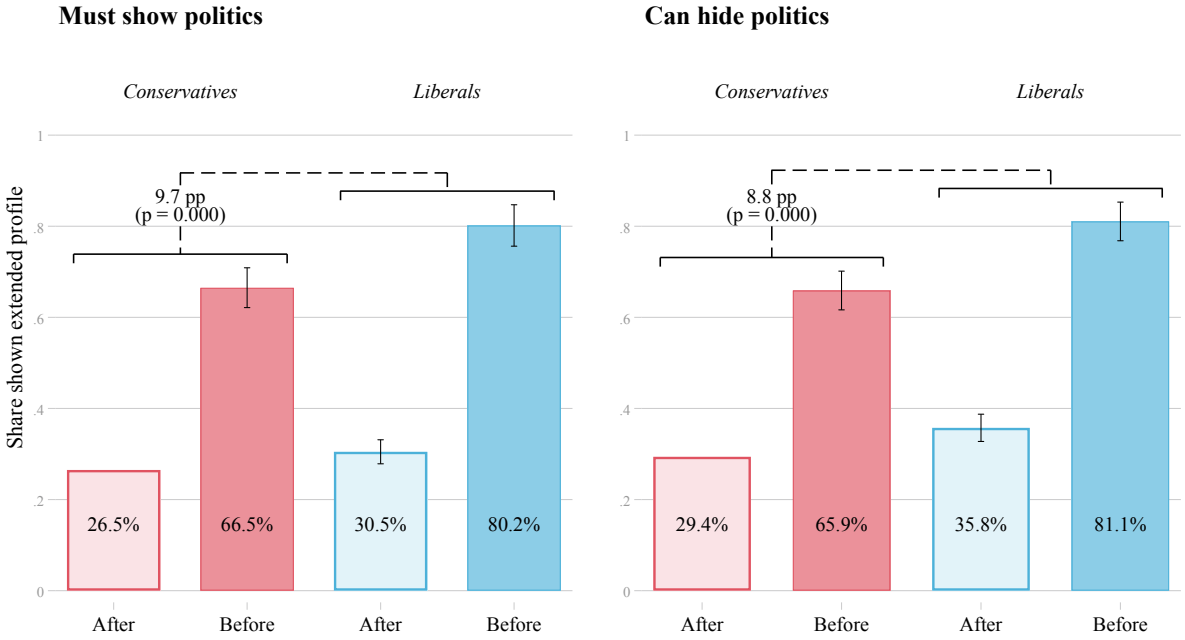

Note: This figure summarizes round-two WTP participants' choices to include their political leanings in profiles shown to possible matches; we restrict here to the 490 participants who are randomized to have the option to hide their politics, each of whom makes 20 choices. We simply regress an indicator that participant *i* chooses to include their political leanings in a profile shown to possible match *j* on interacted indicators that participant *j* is a liberal and would see their profile before deciding whether to email Congress or not, WTP participant fixed effects, and indicators for binary choice number from 1 through 20. We cluster standard errors by WTP participant.

Figure 8: Beliefs about the impacts of profiles with and without own politics in round-two WTP




Note: This figure summarizes participants' beliefs in round two of the WTP experiment about the probability that possible WTP matches would email Congress when they return for a follow-up survey. The figures at left and right are split between potential liberal versus conservative matches. Within each of these sets, we then plot participants' average beliefs for the probability that matches would email Congress if they did not or did see invitations beforehand from a Wave-1 participant, separately for if these invitations included or hid Wave-1 participants' political leanings. We indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

Figure 9: Choices over extended and basic profiles in round-two WTP




Note: This figure summarizes participants' choices to tell possible matches that they emailed Congress in the round-two WTP experiment. The graphs on the left and the right plot the share of the time in whcih participants chose to show possible matches extended profiles saying that they emailed Congress when they did not have or did have the option to hide their own political leanings from their profiles, respectively. Within each of these groups, we then separately plot the share of the time in which participants chose extended profiles for conservative versus liberal matches who would see those profiles after or before deciding whether to email Congress or not. We plot 95% confidence intervals on bars, and we indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

# 11 Main-text tables

Table 1: Wave-2 sample summary and balance

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | **Full means:** | | **Liberals:** | | | **Conservatives:** | | |
| | Liberal | Conserv. | Control | Δ Treat | *p-value* | Control | Δ Treat | *p-value* |
| From Qualtrics sample | 0.171 | 0.945 | 0.177 | -0.011 | (0.317) | 0.944 | 0.001 | (0.901) |
| Woman | 0.565 | 0.521 | 0.560 | 0.011 | (0.432) | 0.522 | -0.002 | (0.912) |
| Hispanic | 1.055 | 1.101 | 1.056 | -0.003 | (0.617) | 1.102 | -0.002 | (0.856) |
| Has $\geq$ 4-year college degree | 0.758 | 0.361 | 0.761 | -0.006 | (0.617) | 0.356 | 0.010 | (0.579) |
| **Age ranges:** | | | | | | | | |
| 20-39 | 0.217 | 0.427 | 0.215 | 0.003 | (0.803) | 0.429 | -0.004 | (0.824) |
| 40-59 | 0.326 | 0.318 | 0.340 | -0.027** | (0.038) | 0.319 | -0.003 | (0.860) |
| 60-79 | 0.457 | 0.255 | 0.445 | 0.024* | (0.087) | 0.251 | 0.006 | (0.708) |
| **Income bins (USD):** | | | | | | | | |
| Less than 50,000 | 0.232 | 0.397 | 0.228 | 0.009 | (0.453) | 0.399 | -0.004 | (0.824) |
| 50,000-99,999 | 0.324 | 0.378 | 0.329 | -0.010 | (0.442) | 0.388 | -0.019 | (0.291) |
| 100,000-149,999 | 0.222 | 0.136 | 0.219 | 0.006 | (0.617) | 0.131 | 0.009 | (0.489) |
| 150,000-199,999 | 0.108 | 0.051 | 0.108 | 0.001 | (0.912) | 0.040 | 0.022*** | (0.006) |
| 200,000 or more | 0.114 | 0.038 | 0.117 | -0.006 | (0.505) | 0.042 | -0.007 | (0.317) |
| **Residence by state marginality:** | | | | | | | | |
| Red state | 0.267 | 0.413 | 0.271 | -0.008 | (0.505) | 0.409 | 0.009 | (0.617) |
| Blue state | 0.419 | 0.257 | 0.415 | 0.008 | (0.568) | 0.260 | -0.006 | (0.708) |
| Purple state | 0.314 | 0.329 | 0.314 | -0.000 | (1.000) | 0.331 | -0.003 | (0.860) |
| **Climate beliefs:** | | | | | | | | |
| Climate worry (1-7) | 6.300 | 4.953 | 6.318 | -0.036 | (0.166) | 4.928 | 0.049 | (0.373) |
| Desire for climate action (1-7) | 6.630 | 5.216 | 6.635 | -0.010 | (0.617) | 5.155 | 0.123** | (0.031) |
| Perceived local impacts (1-7) | 5.483 | 4.836 | 5.466 | 0.035 | (0.243) | 4.873 | -0.075 | (0.157) |
| **Political engagement and beliefs:** | | | | | | | | |
| Member of resp. party | 0.319 | 0.742 | 0.321 | -0.005 | (0.701) | 0.734 | 0.015 | (0.349) |
| Political engage. index (std) | 1.195 | -2.033 | 1.199 | -0.008 | (0.943) | -2.073 | 0.080 | (0.519) |
| Prev. contacted reps | 0.268 | 0.219 | 0.265 | 0.006 | (0.617) | 0.212 | 0.014 | (0.351) |
| Prev. donated | 0.686 | 0.241 | 0.683 | 0.006 | (0.644) | 0.241 | 0.000 | (1.000) |
| Prev. canvassed | 0.064 | 0.040 | 0.063 | 0.003 | (0.668) | 0.038 | 0.004 | (0.568) |
| Prev. signed petition | 0.746 | 0.390 | 0.748 | -0.003 | (0.803) | 0.384 | 0.011 | (0.541) |
| Prev. phonebanked | 0.078 | 0.045 | 0.079 | -0.001 | (0.901) | 0.049 | -0.009 | (0.261) |
| Political efficacy index (std) | -0.109 | 0.186 | -0.120 | 0.021 | (0.162) | 0.193 | -0.014 | (0.505) |
| Prefer friend of own party (1-7) | 5.663 | 4.895 | 5.664 | -0.002 | (0.949) | 4.909 | -0.028 | (0.543) |
| Sample size | 5027 | 2954 | 2517 | 2510 | | 1468 | 1486 | |

Note: This table summarizes and tests for balance within the Wave-2 experimental sample. We define the Wave-2 experimental sample as those who remained in the survey through choosing whether to email Congress or not. Column 1 and 2 present means among liberals (those who belong to or lean towards the Democratic party) and conservatives (those who belong to or lean towards the Republican party), respectively, on a range of baseline traits. Appendix B.3 describes each of these baseline traits in more detail. Variables labeled as "(1-7)" are elicited on Likert scales from 1 through 7, and variables labeled as "(std)" are indices standardized in the full Wave-2 experimental sample. Columns 4 and 6 present control means for each trait, columns 5 and 7 present the difference in means between the treatment and control groups on each baseline trait, and columns 6 and 8 present heteroskedasticity-robust p-values testing the null of equality across treatment and control on each trait, separately among liberals and conservatives. In columns 4 and 6, we indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

Table 2: Impacts of Wave-1 invitations on Wave-2 political action

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Start email process | | | Have email record | | |
| | *All* | *Dem* | *Rep* | *All* | *Dem* | *Rep* |
| Treatment | 0.057*** | 0.069*** | 0.044*** | 0.044*** | 0.058*** | 0.021* |
| | (0.010) | (0.014) | (0.015) | (0.010) | (0.012) | (0.011) |
| Control mean | 0.358 | 0.425 | 0.244 | 0.187 | 0.247 | 0.084 |
| N | 7981 | 5027 | 2954 | 7981 | 5027 | 2954 |
| | *p-value* (2)=(3): 0.212 | | | *p-value* (5)=(6): 0.023 | | |

Note: This table reports impacts of seeing an invitation from a Wave-1 participant on whether Wave-2 participants initially opt into the email process (columns 1 through 3) and match to an email record (columns 5 through 6). Columns 1 and 4 pool across all Wave-2 participants, while columns 2 and 5 restrict to Wave-2 liberals, and columns 3 and 6 restrict to Wave-2 conservatives. Across all columns, we define the experimental sample as those who remained in the survey through choosing whether to email Congress or not. All regressions control for participants' recruitment method, demographics traits (gender, age, state of residence, income category, educational attainment, and whether identify as Hispanic), baseline beliefs about climate change (standardized climate worry, desire for government climate action, and perceived local climate impacts), and political engagement and beliefs (party leanings and membership, political efficacy beliefs, and a standardized index of past political engagement). We present heteroskedasticity-robust standard errors in parentheses and indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively. The last row of the table presents p-values for heteroskedasticity-robust tests of the equality between the treatment effects on liberals and conservatives.

Table 3: Wave-1 sample summary and balance

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | **Full sample** | **Control (A0)** | **Tell after (A1)** | | **Tell before (A2)** | |
| | Mean | Mean | Δ Mean | *p-value* | Δ Mean | *p-value* |
| Woman | 0.638 | 0.632 | 0.006 | (0.617) | 0.012 | (0.317) |
| Hispanic | 1.026 | 1.027 | -0.001 | (0.803) | 0.000 | (1.000) |
| Has $\geq$ 4-year college degree | 0.826 | 0.828 | -0.007 | (0.484) | 0.001 | (0.920) |
| **Age ranges:** | | | | | | |
| 20-39 | 0.114 | 0.119 | -0.019** | (0.018) | -0.001 | (0.901) |
| 40-59 | 0.341 | 0.337 | 0.016 | (0.182) | -0.001 | (0.934) |
| 60-79 | 0.545 | 0.544 | 0.003 | (0.817) | 0.002 | (0.878) |
| **Income bins (USD):** | | | | | | |
| Less than 50,000 | 0.167 | 0.170 | -0.004 | (0.689) | -0.007 | (0.484) |
| 50,000-99,999 | 0.321 | 0.325 | -0.009 | (0.453) | -0.008 | (0.505) |
| 100,000-149,999 | 0.234 | 0.231 | -0.004 | (0.716) | 0.015 | (0.173) |
| 150,000-199,999 | 0.130 | 0.126 | 0.007 | (0.437) | 0.007 | (0.437) |
| 200,000 or more | 0.148 | 0.147 | 0.009 | (0.317) | -0.008 | (0.374) |
| **Residence by state marginality:** | | | | | | |
| Red state | 0.236 | 0.231 | 0.017 | (0.122) | 0.002 | (0.856) |
| Blue state | 0.441 | 0.438 | -0.005 | (0.701) | 0.016 | (0.218) |
| Purple state | 0.322 | 0.331 | -0.012 | (0.317) | -0.017 | (0.157) |
| Climate worry (1-7) | 6.421 | 6.430 | -0.013 | (0.516) | -0.016 | (0.446) |
| Desire for climate action (1-7) | 6.734 | 6.735 | -0.001 | (0.947) | 0.000 | (1.000) |
| Perceived local impacts (1-7) | 5.499 | 5.496 | 0.026 | (0.298) | -0.015 | (0.564) |
| **Political engagement and beliefs:** | | | | | | |
| Political engage. index (std) | -0.017 | -0.029 | 0.021 | (0.840) | 0.021 | (0.838) |
| Prev. contacted reps | 0.731 | 0.733 | -0.009 | (0.413) | 0.004 | (0.716) |
| Prev. donated | 0.819 | 0.816 | 0.007 | (0.484) | 0.004 | (0.689) |
| Prev. canvassed | 0.081 | 0.081 | -0.001 | (0.886) | -0.001 | (0.886) |
| Prev. signed petition | 0.828 | 0.835 | -0.010 | (0.317) | -0.014 | (0.162) |
| Prev. phonebanked | 0.111 | 0.100 | 0.024*** | (0.003) | 0.013 | (0.104) |
| Political efficacy index (std) | -0.000 | 0.004 | -0.003 | (0.830) | -0.012 | (0.424) |
| Degree prefer Dem friends (1-7) | 6.038 | 6.040 | -0.001 | (0.968) | -0.007 | (0.779) |
| Sample size | 8937 | 3616 | 2646 | | 2675 | |

Note: This table summarizes and tests for balance within the Wave-1 experimental sample, which we define as all those randomized to a Wave-1 treatment group. Column 1 presents means in the full sample on a range of baseline traits, and column 2 presents means among participants assigned to the A0 pure control group. Appendix B.3 describes each of these baseline traits in more detail. Variables labeled as "(1-7)" are elicited on Likert scales from 1 through 7, and variables labeled as "(std)" are indices standardized in the full Wave-1 experimental sample. Columns 3 and 4 present the differences in means between the A1 (Tell after) and A0 groups and heteroskedasticity-robust p-values testing the null of equality across these groups, respectively; columns 5 and 6 compare the A2 (Invitation) and A0 groups. In columns 3 and 5, we indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

Table 4: Main Wave-1 results

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Wave-2 match type | | | | | |
| | *All* | | *Liberal* | | *Conservative* | |
| *Panel A: Opted into email process* | | | | | | |
| Tell (A1 or A2) | | 0.121*** | | 0.133*** | | 0.106*** |
| | | (0.012) | | (0.017) | | (0.018) |
| Invite (A2) | 0.155*** | 0.033** | 0.180*** | 0.049*** | 0.123*** | 0.017 |
| | (0.012) | (0.013) | (0.017) | (0.018) | (0.018) | (0.019) |
| Control mean | 0.461 | 0.461 | 0.452 | 0.452 | 0.470 | 0.470 |
| N | 6291 | 8937 | 3152 | 4494 | 3139 | 4443 |
| *p-values:* | *Invite* (3) = (5): | | 0.030 | | | |
| | *Tell* (4) = (6): | | 0.281 | *Invite* (4) = (6): | | 0.220 |
| *Panel B: Has an email record* | | | | | | |
| Tell (A1 or A2) | | 0.134*** | | 0.143*** | | 0.122*** |
| | | (0.012) | | (0.017) | | (0.017) |
| Invite (A2) | 0.160*** | 0.027** | 0.175*** | 0.035* | 0.140*** | 0.020 |
| | (0.012) | (0.013) | (0.017) | (0.019) | (0.017) | (0.019) |
| Control mean | 0.310 | 0.310 | 0.304 | 0.304 | 0.315 | 0.315 |
| N | 6291 | 8937 | 3152 | 4494 | 3139 | 4443 |
| *p-values:* | *Invite* (3) = (5): | | 0.209 | | | |
| | *Tell* (4) = (6): | | 0.389 | *Invite* (4) = (6): | | 0.566 |

Note: This table reports impacts of the Wave-1 treatments on whether Wave-1 participants initially opt into the email process (Panel A) and match to an email record (Panel B). Columns 1 and 2 pool across all Wave-1 participants, columns 3 and 4 restrict to Wave-1 participants paired with liberal Wave-2 matches, and columns 5 and 6 restrict to Wave-1 participants paired with conservative Wave-2 matches. Columns 1, 3, and 5 restrict to participants randomized either to the A0 (Pure control) or A2 (Invitation) groups and test the main effect of knowing that up to 10 matched Wave-2 participants will be invited to join in emailing Congress if you do so. Columns 2, 4, and 6 include the full Wave-1 sample: the "Tell" coefficient captures the effect of being assigned to either A1 or A2, while the "Invitation" coefficient captures the differential effect of being assigned to the A2 (Invitation) group. Across all columns, we define the experimental sample as those who were randomized to a Wave-1 treatment arm, assuming that participants who attrited from the survey before explicitly deciding whether to email or not, but after seeing the email preview, would not have done so. We present control means estimated in the A0 pure control group. All regressions control for participants' recruitment timing, demographics traits (gender, age, state of residence, income category, educational attainment, and whether identify as Hispanic), baseline beliefs about climate change (standardized climate worry, desire for government climate action, and perceived local climate impacts), and political engagement and beliefs (political efficacy beliefs and a standardized index of past political engagement). We present heteroskedasticity-robust standard errors in parentheses and indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively. The last rows of each panel present p-values for heteroskedasticity-robust tests of equality between the treatment effects for Wave-1 participants matched with Wave-2 liberals versus conservatives.

Table 5: Main WTP results

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Showed extended profile | | | |
| | No info | | Has info | |
| Before | 0.416*** | 0.535*** | 0.481*** | 0.538*** |
| | (0.020) | (0.023) | (0.019) | (0.021) |
| Republican | | -0.135*** | | -0.037*** |
| | | (0.016) | | (0.014) |
| Before * Republican | | -0.239*** | | -0.115*** |
| | | (0.019) | | (0.015) |
| Mean: After Dem + Rep | 0.233 | | 0.225 | |
| Mean: After Dem | | 0.300 | | 0.244 |
| Num. participants | 475 | 475 | 548 | 548 |
| Num. choices | 9500 | 9500 | 10960 | 10960 |

Note: This table reports the main results from the WTP experiment. Across columns, the outcome variable is whether participants choose to pass on an extended profile saying that they emailed Congress, rather than passing on a basic demographic profile and delegating a small carbon-offset donation from our research funding, to a returning participant with whom they could be matched. Columns 1 and 2 analyze the sample of WTP participants who are assigned to see profiles of possible matches that do not include information on their beliefs about climate change, while columns 3 and 4 analyze participants who see profiles including that all participants believe climate change is mostly human-caused. In both samples, we restrict to participants who make binary choices for all 20 possible matches. Columns 1 and 3 test whether WTP participants are more or less likely overall to tell a possible match that they emailed Congress when that match would see their profile before deciding whether to email Congress or not; in these columns, the table footer includes the share of cases in which WTP participants choose to show an extended profile to any match who would see their profile after deciding whether to email Congress or not. Columns 2 and 4 then test whether this "Before" effect differs significantly when matches are liberal versus conservative; the table footer in these columns includes the share of cases in which WTP participants choose extended profiles for liberal who would see their profile after deciding whether to email Congress or not. All regressions control for WTP-participant fixed effects and binary choice number, from 1 to 20. We present heteroskedasticity-robust standard errors in parentheses and indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

# A Appendix

## A.1 Figures

Figure A1: Democrats want bipartisanship overall

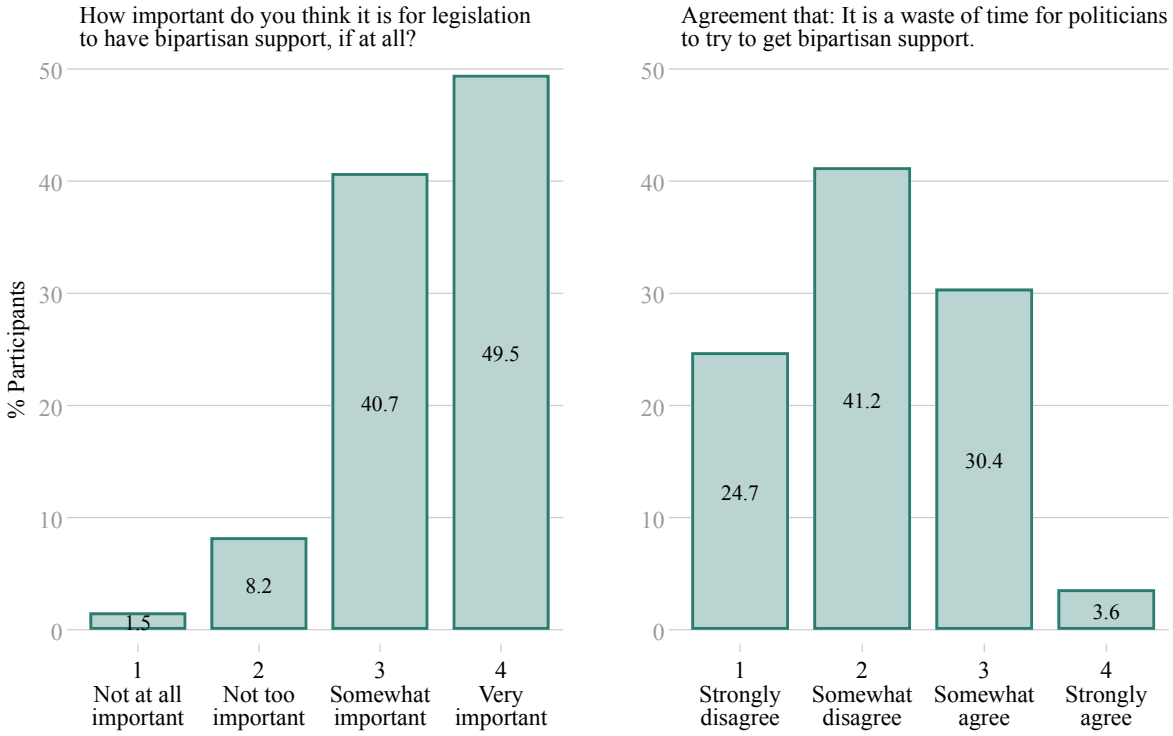


Figure A2: Democrats think increasing Republican lawmakers' support is crucial for climate policy

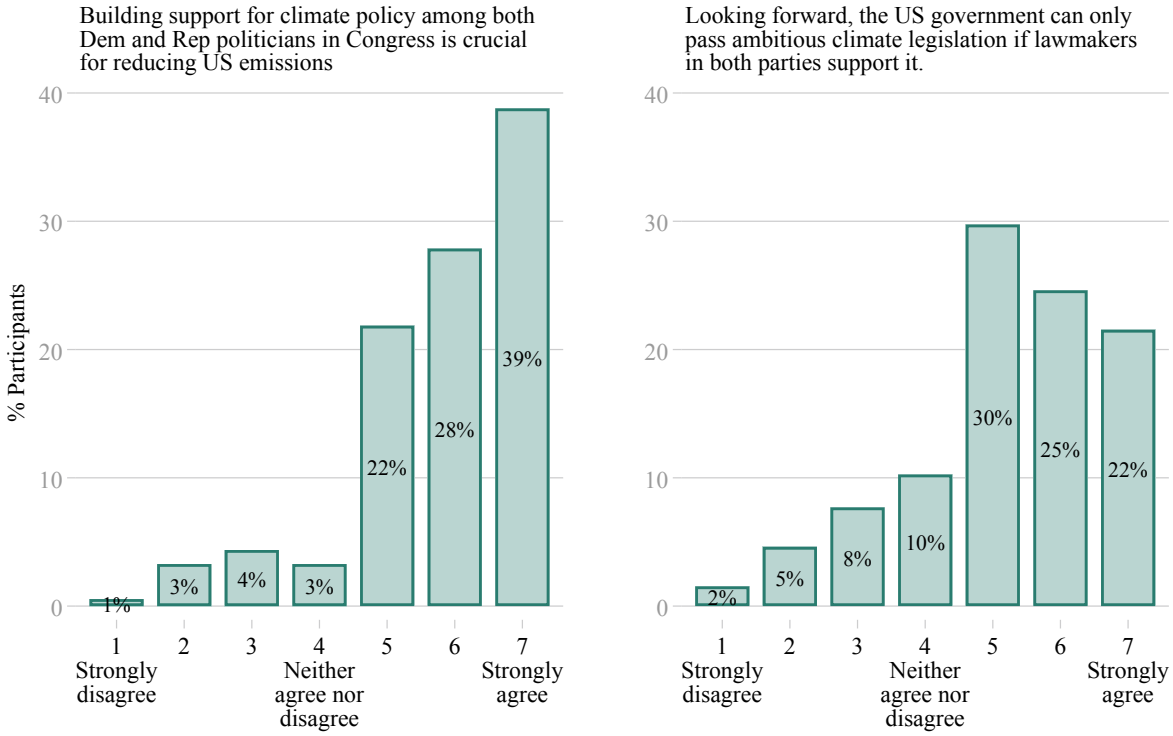
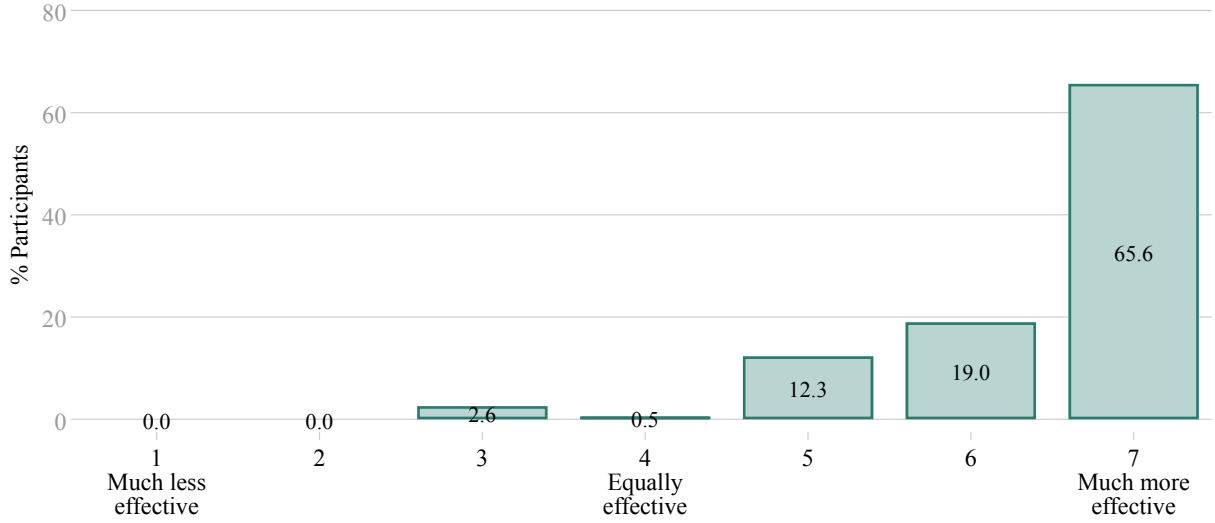


Figure A3: Democrats' beliefs about effectiveness of bipartisan movement

**Panel A.** How much more or less effective would a bipartisan climate movement be than a purely liberal movement in advancing US climate policy?




**Panel B.** Impacts of conservative advocates on legislators and policy goals

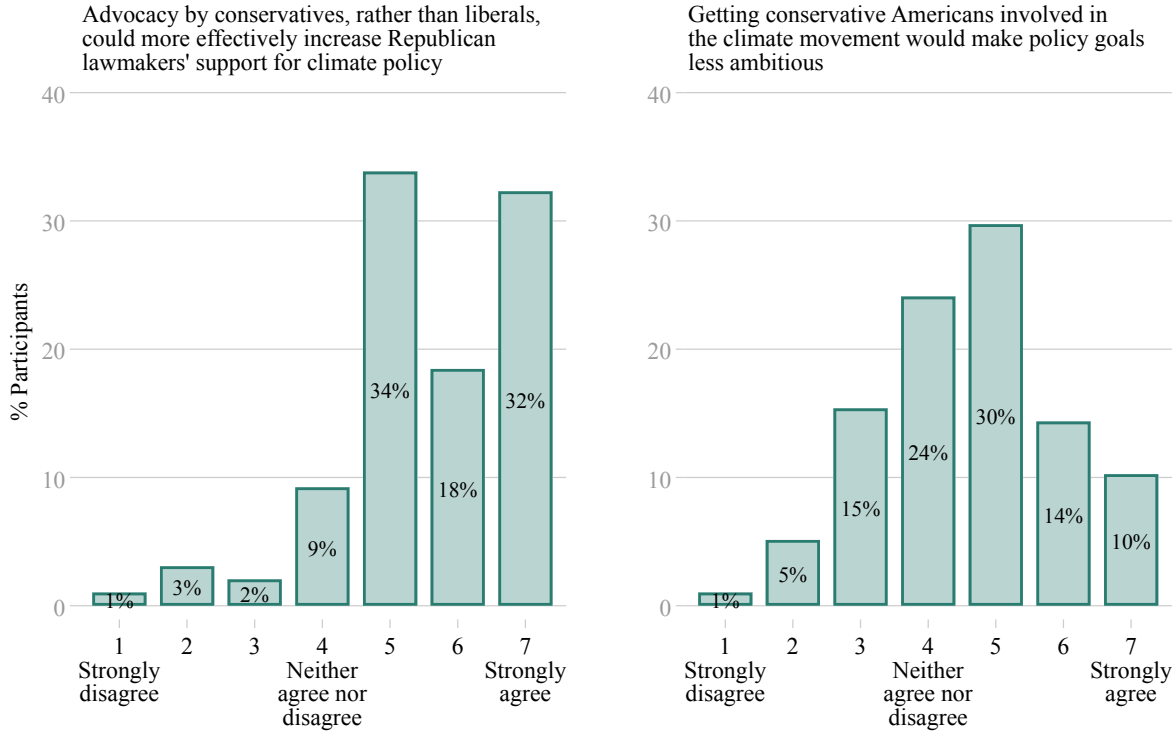


Figure A4: Democrats say citizens and organizations should focus on building Republican support

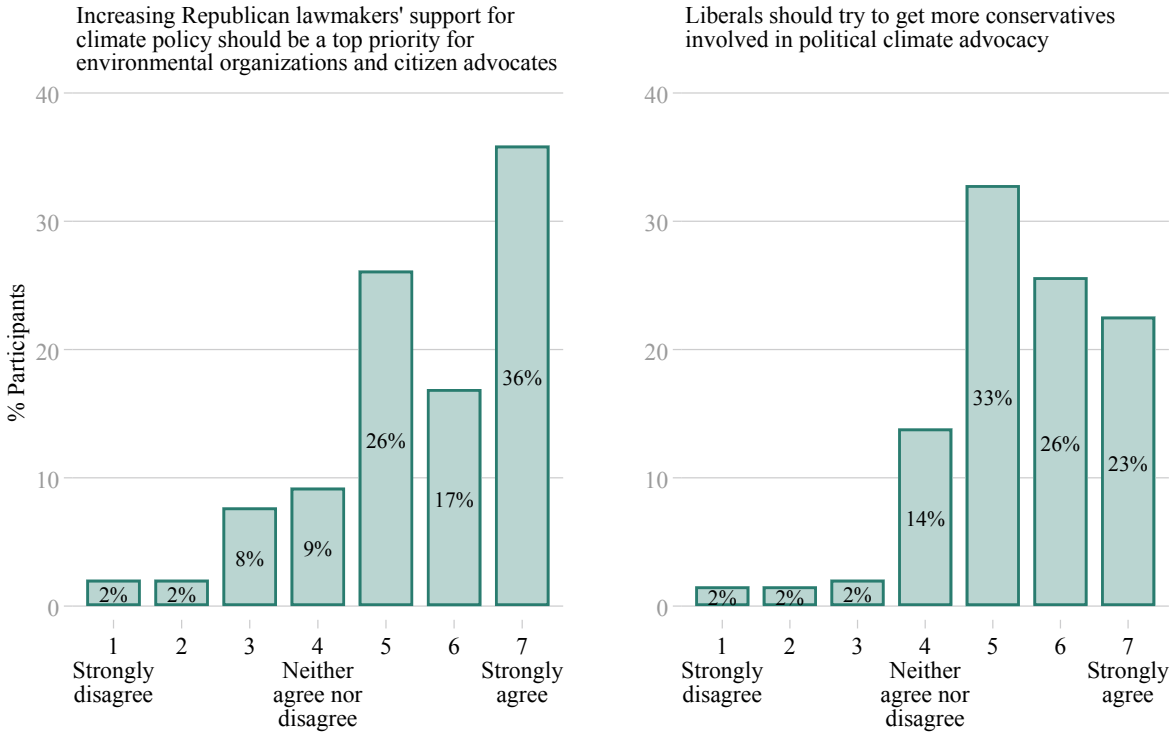


Figure A5: Few Democrats say they've previously invited conservatives

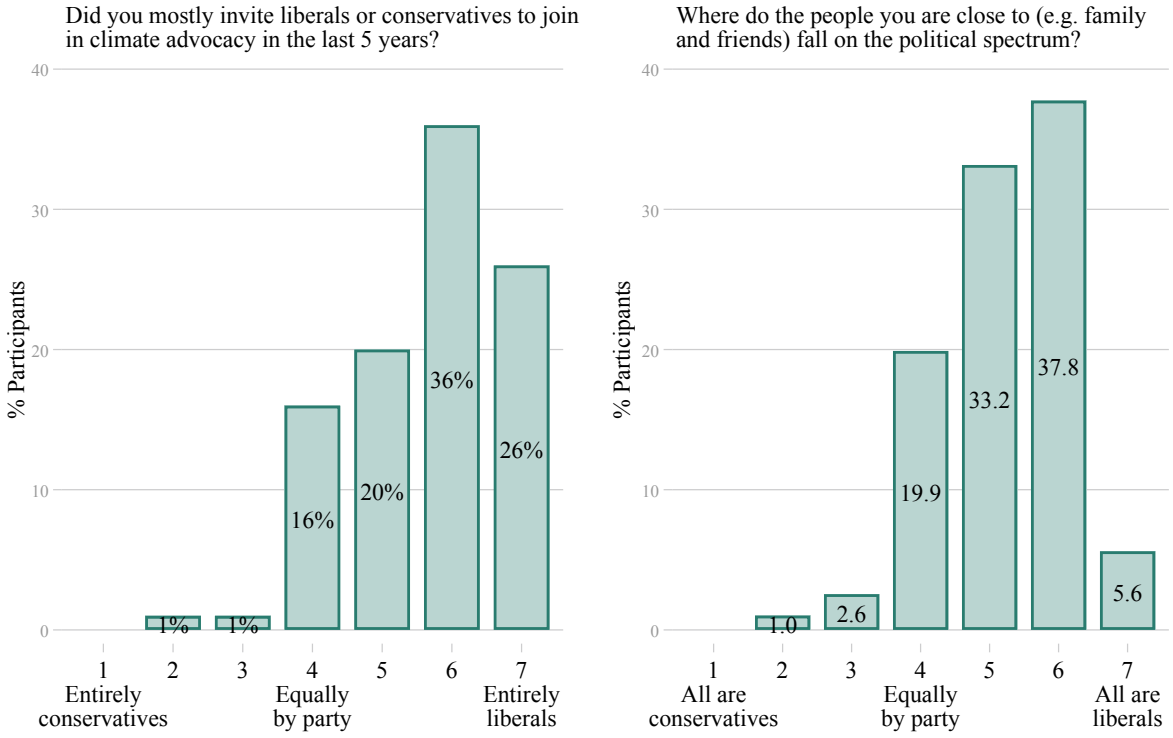
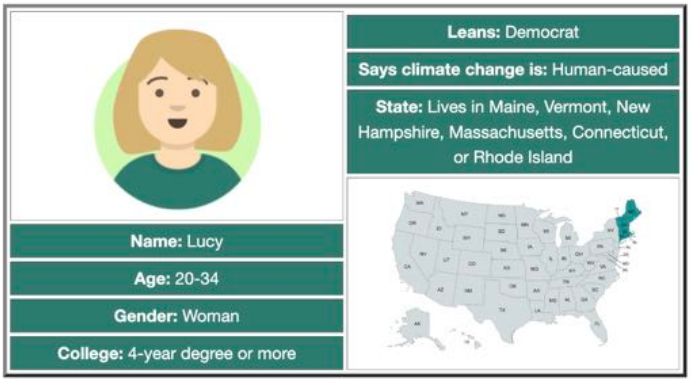


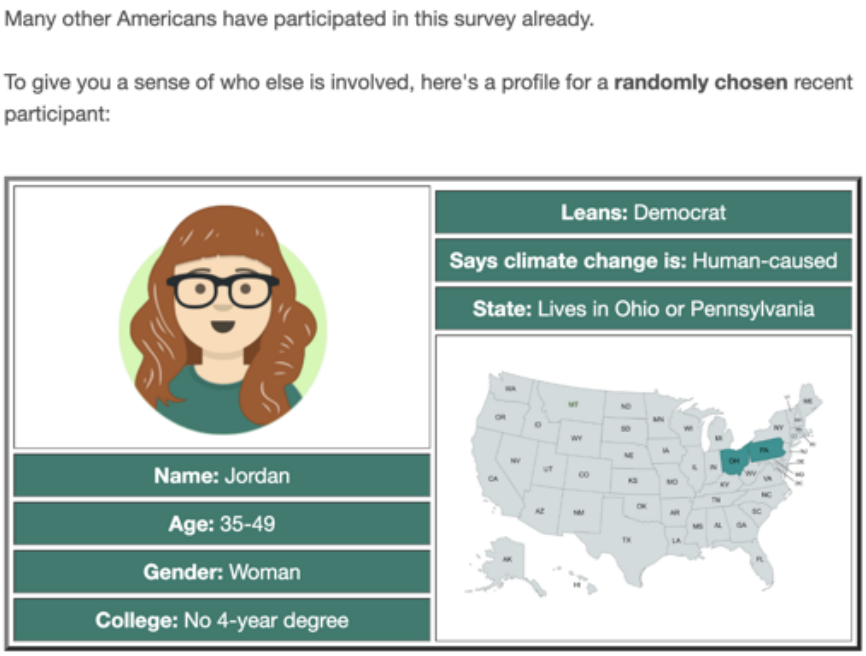

Figure A6: Sample basic demographic profile



### A.1.1 Wave 2

Figure A7: Wave-2 recruitment

**Panel A.** Showing all Wave-2 participants a Wave-1 participant's demographics




**Panel B.** Treatment: Action invitation from Wave-1 match

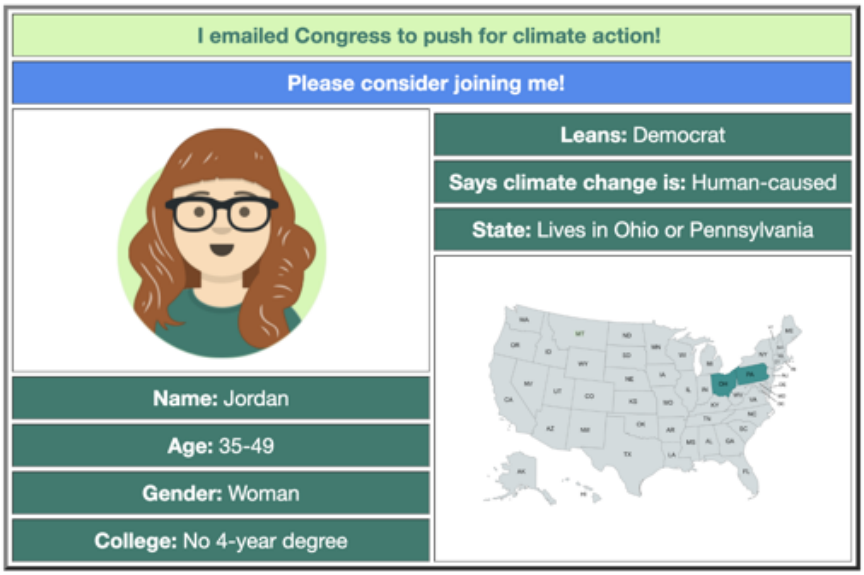
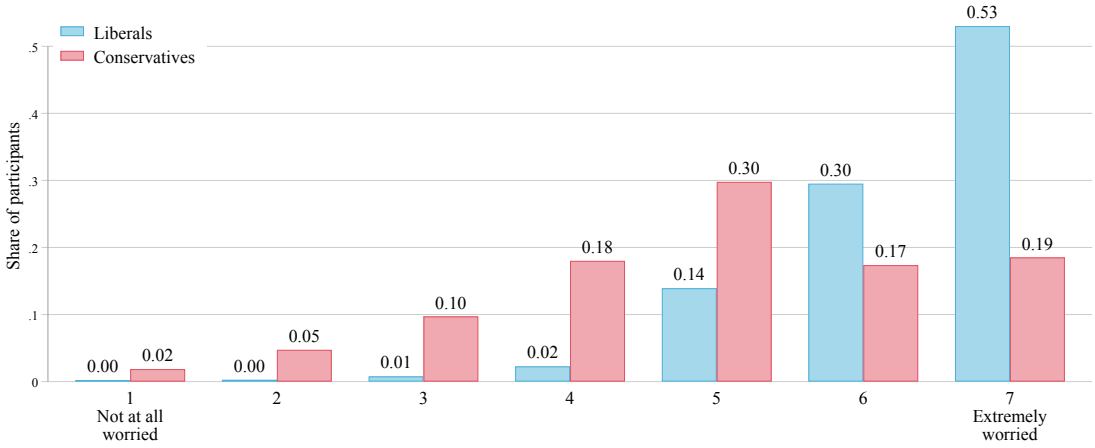
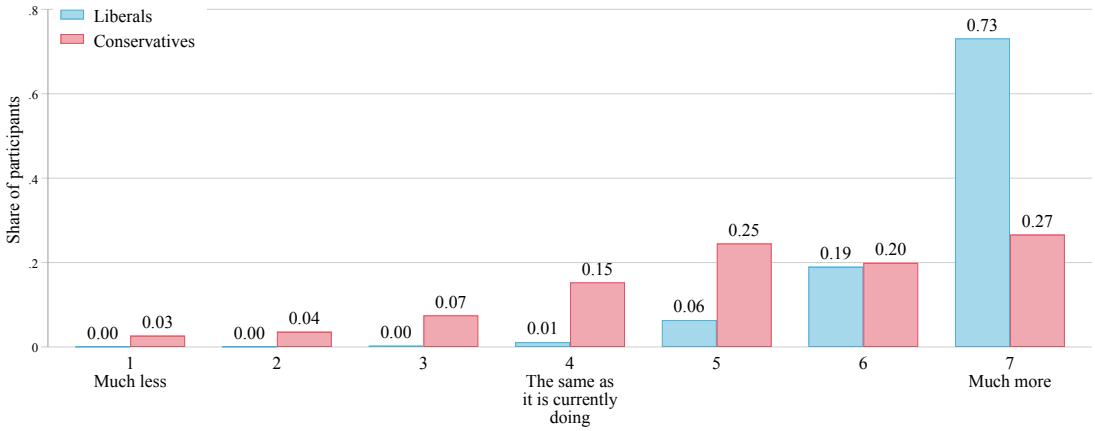


Figure A8: Baseline climate beliefs: Wave 2, split between conservatives and liberals

**Panel A.** How worried are you about climate change?




**Panel B.** How much do you want the federal government to do to slow or stop climate change, relative to what it's currently doing?




**Panel C.** How much would you say you are currently seeing the effects of climate change in your local area, like changes in weather patterns or natural disasters?

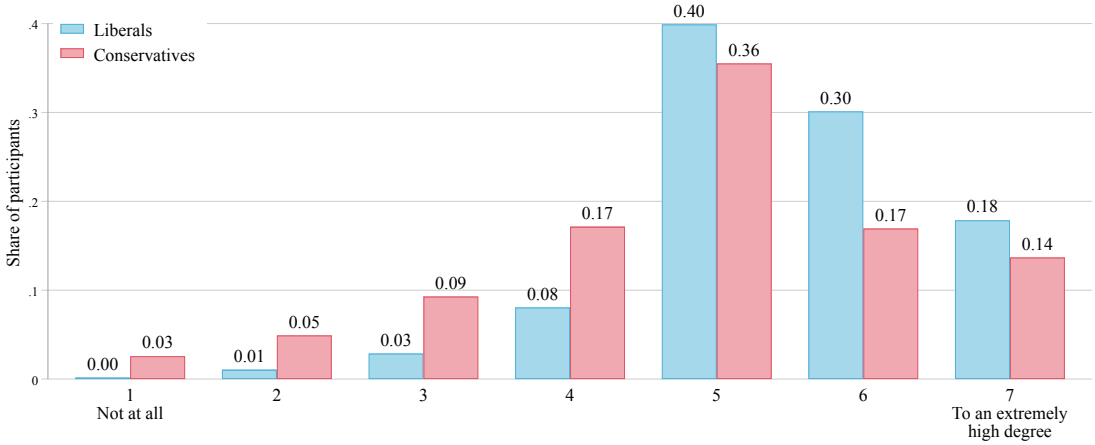
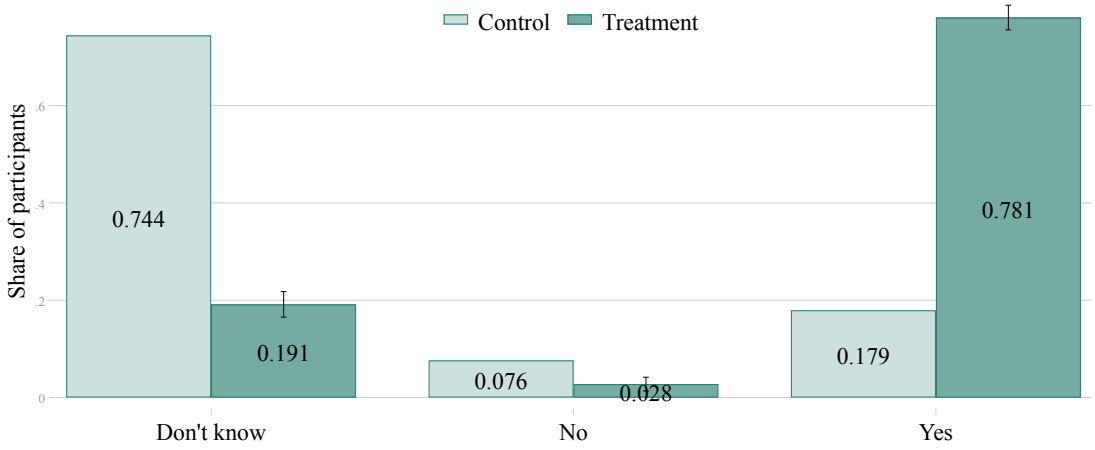
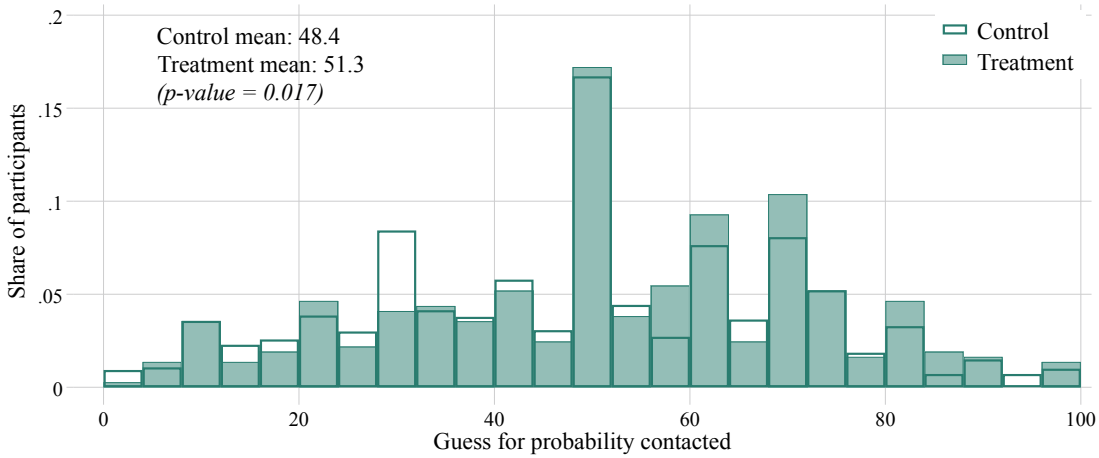


Figure A9: Comprehension among Wave-2 participants

**Panel A.** Do you know whether your paired earlier participant emailed Congress while taking our survey or not?




**Panel B.** If answered they don't know: What would you guess is the probability that the past participant we showed you emailed Congress via our form?




**Panel C.** Will any future participants be told whether or not you contacted Congress?

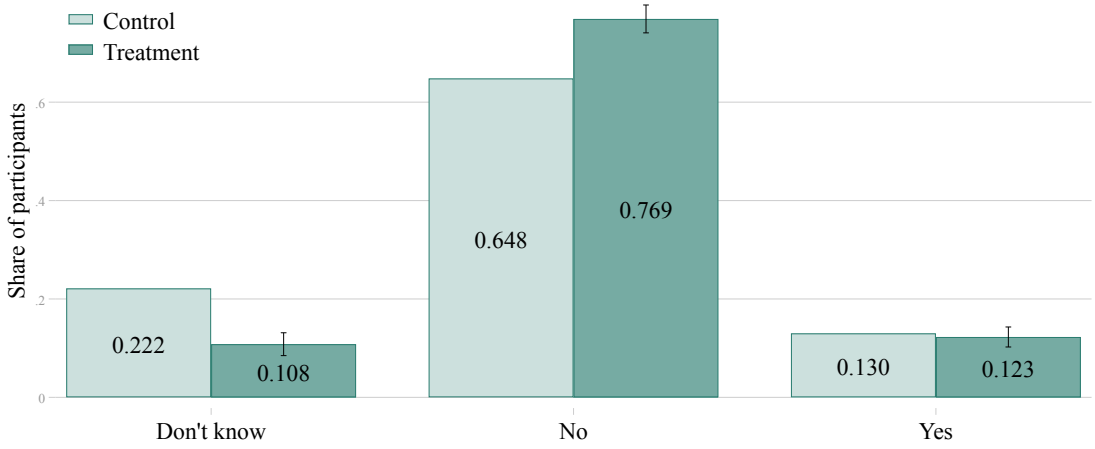
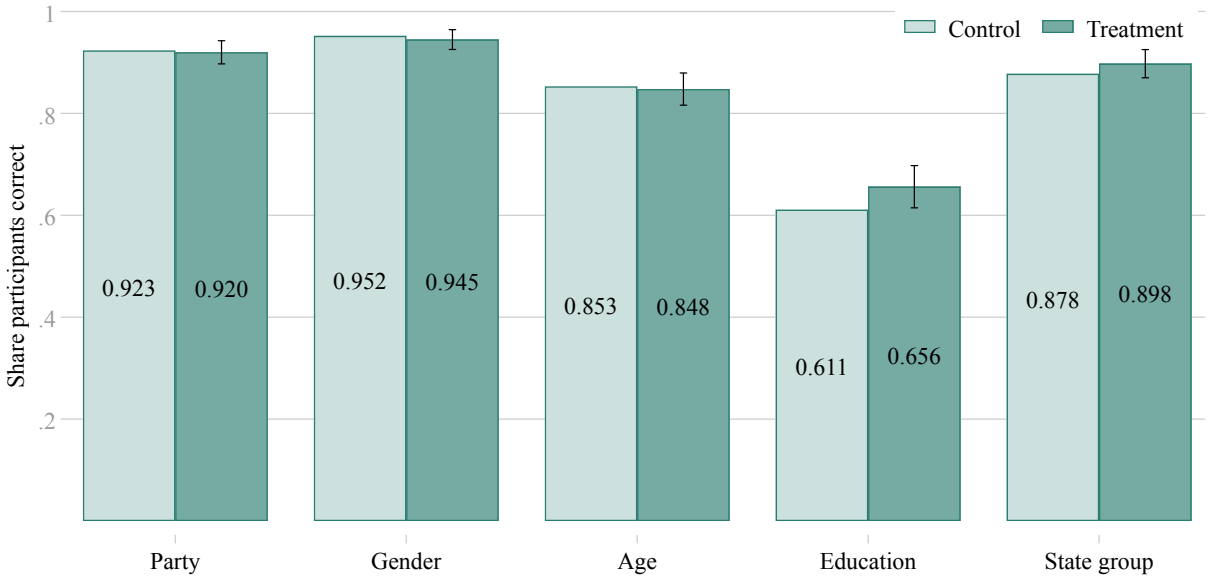


Figure A10: Whether participants correctly remember paired participants' traits

**Panel A.** Share Wave-2 participants who correctly remember Wave-1 matches' traits




**Panel B.** Share Wave-1 participants who correctly remember Wave-2 matches' traits

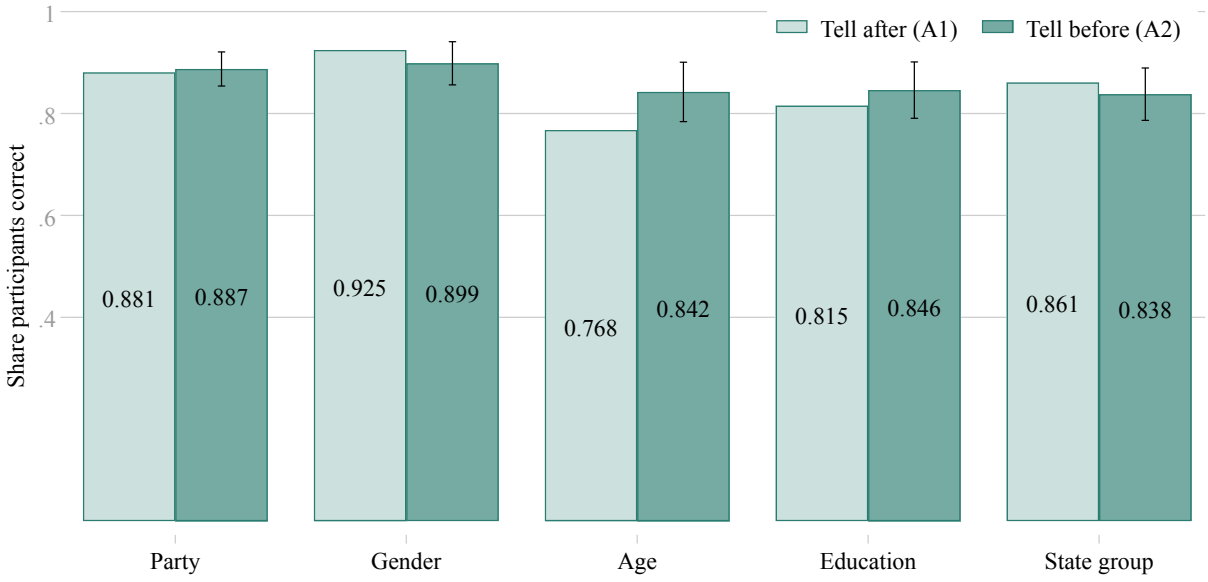


Figure A11: Determinants of perceived similarity

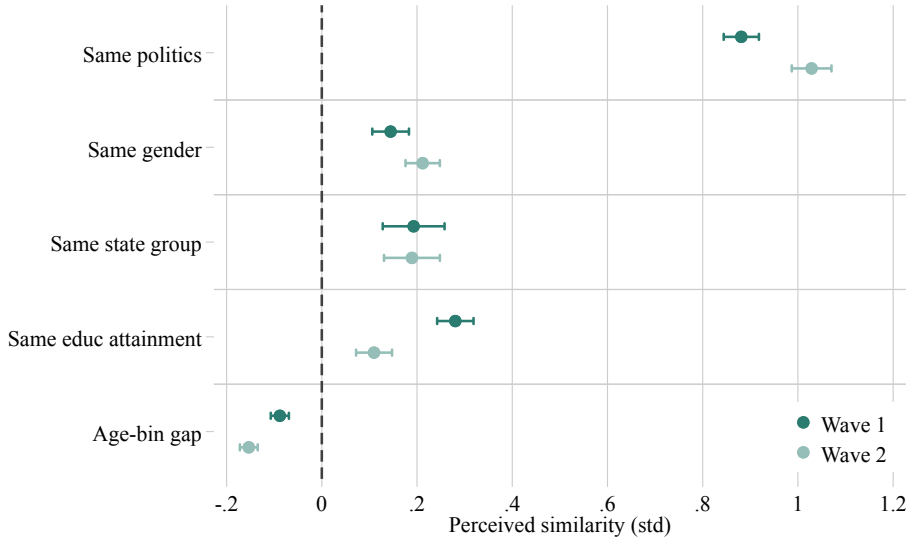
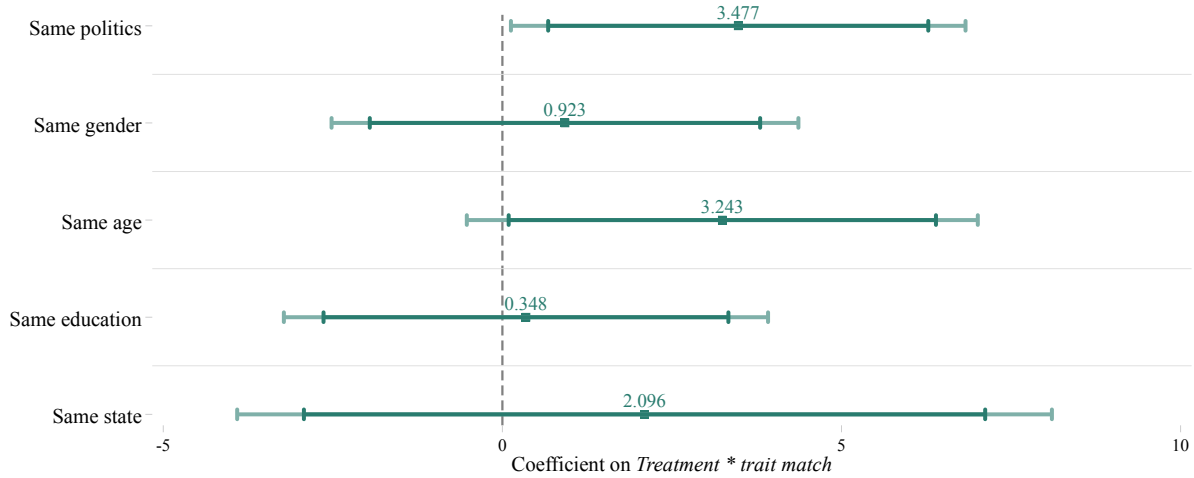



Figure A12: Heterogeneous influence effects by match similarity




Note: This figure plots results from estimating the differential impacts of Wave-1 invitations on Wave-2 participants' action when senders and recipients match on a range of demographic traits. We estimate this heterogeneity in versions of equation 2 where we include indicators that Wave-2 participant $i$ and their paired Wave-1 influencer match on political leanings, gender, age group, educational attainment, and state group of residence and interactions of each dummy with $Treatment_i$. This figure plots the estimated coefficients on the interaction terms in separate regressions for each match trait. Appendix Figure

Figure A13: Specification chart: Wave-2, start the email process

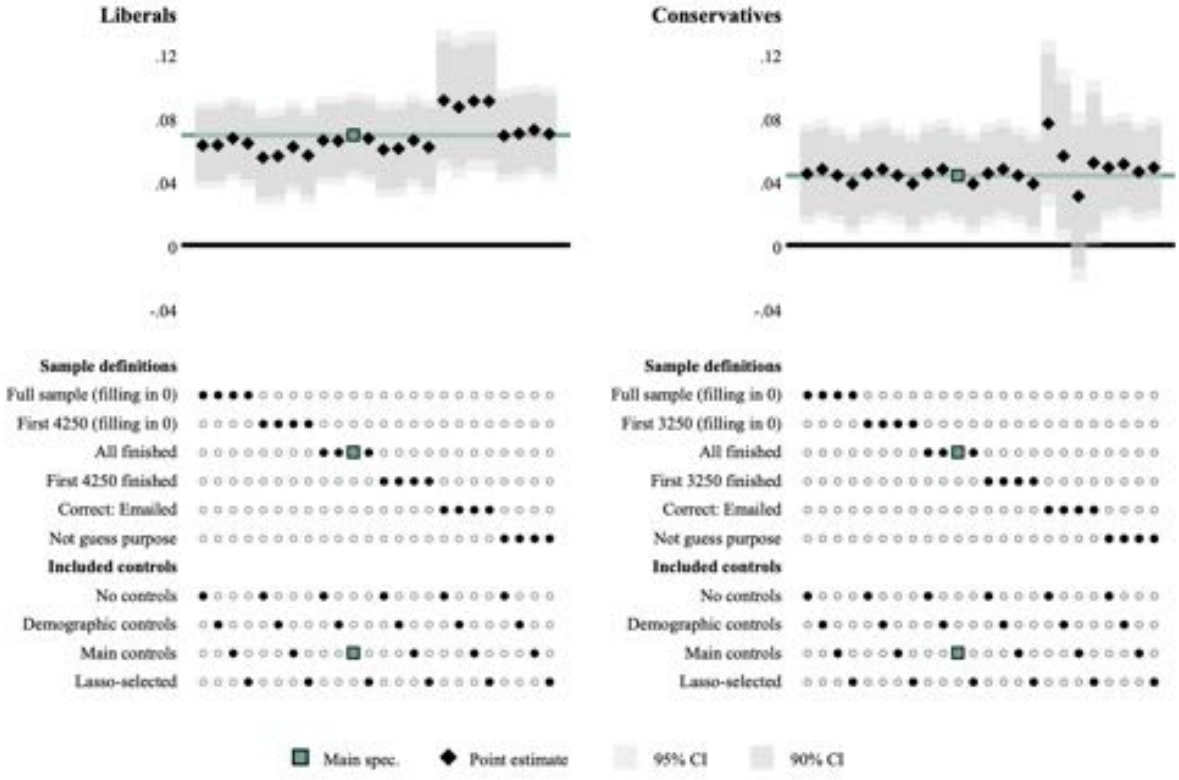


Figure A14: Specification chart: Wave-2, have an email record

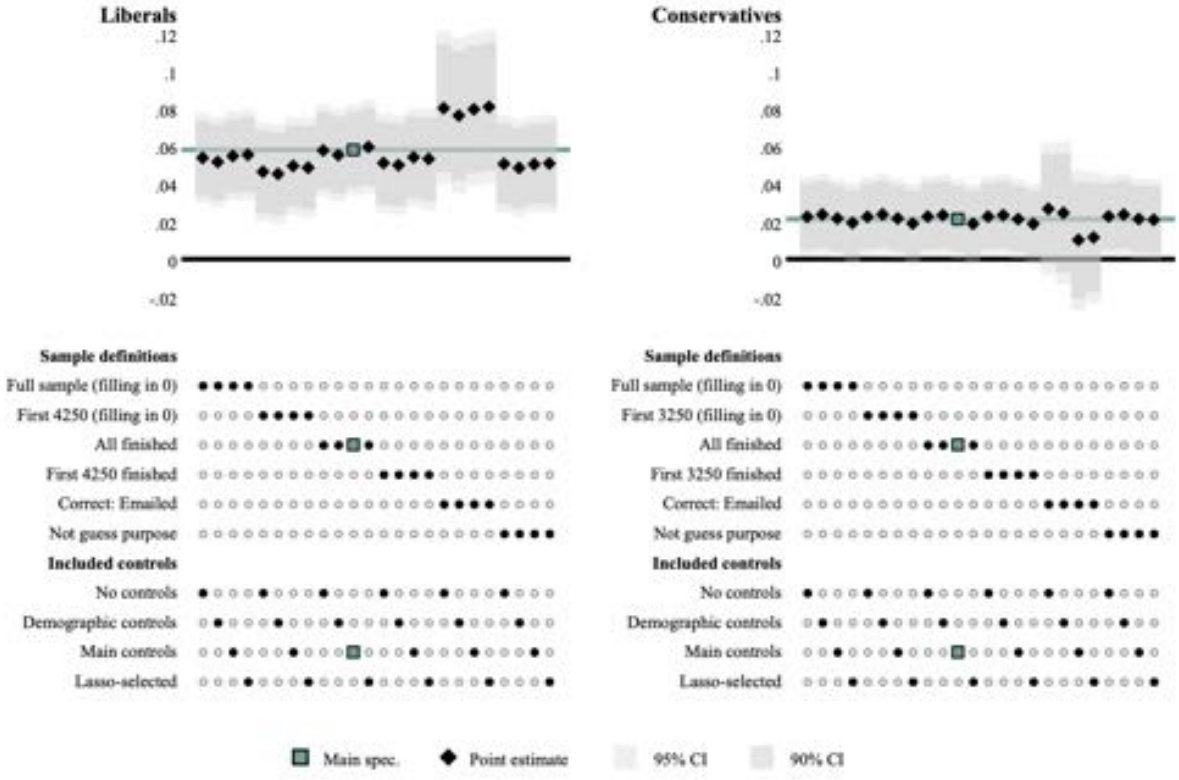
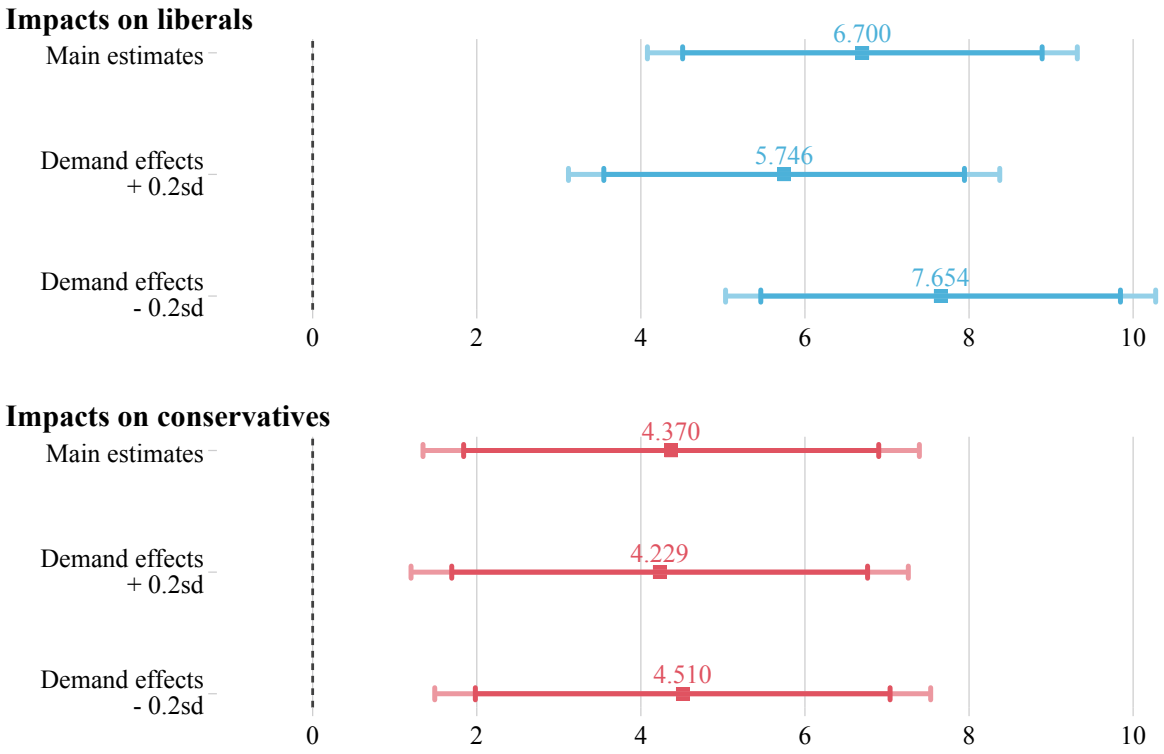


Figure A15: Robustness of Wave-2 estimates to demand effects

**Panel A.** Impacts on opting into the email process




**Panel B.** Impacts on email records

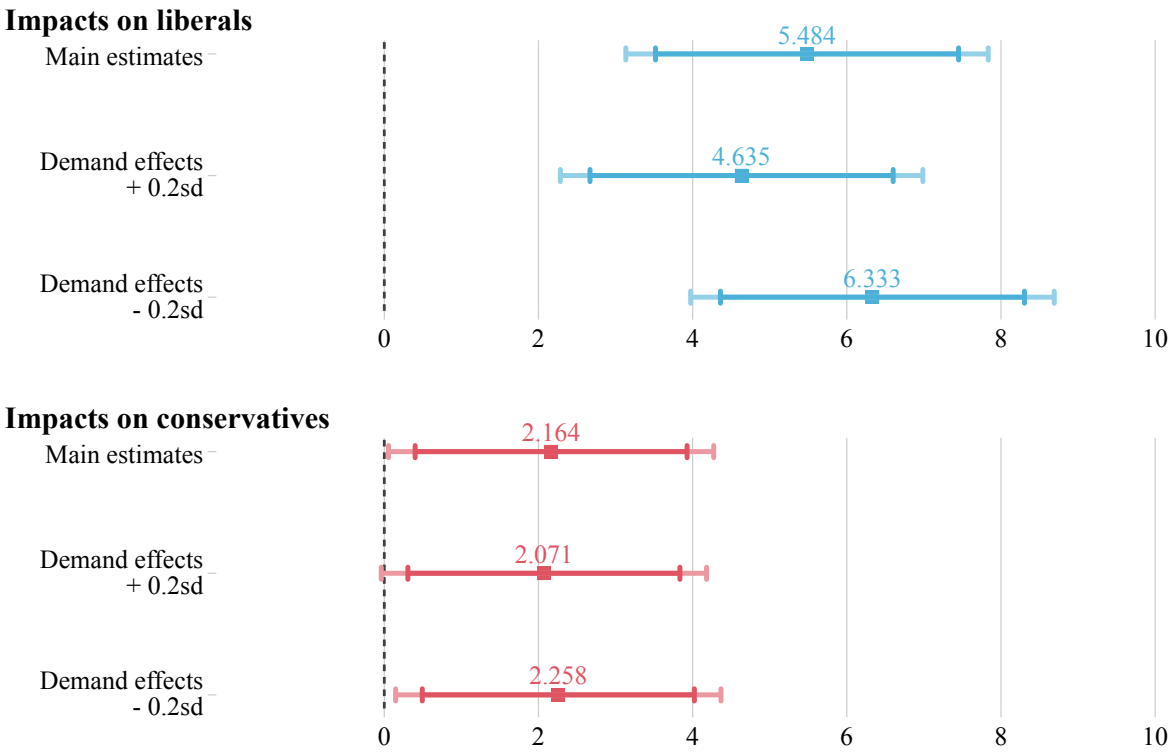


Figure A16: Robustness of Wave-2 partisan gaps

**Panel A.** Impacts on opting into the email process


**Panel B.** Impacts on email records

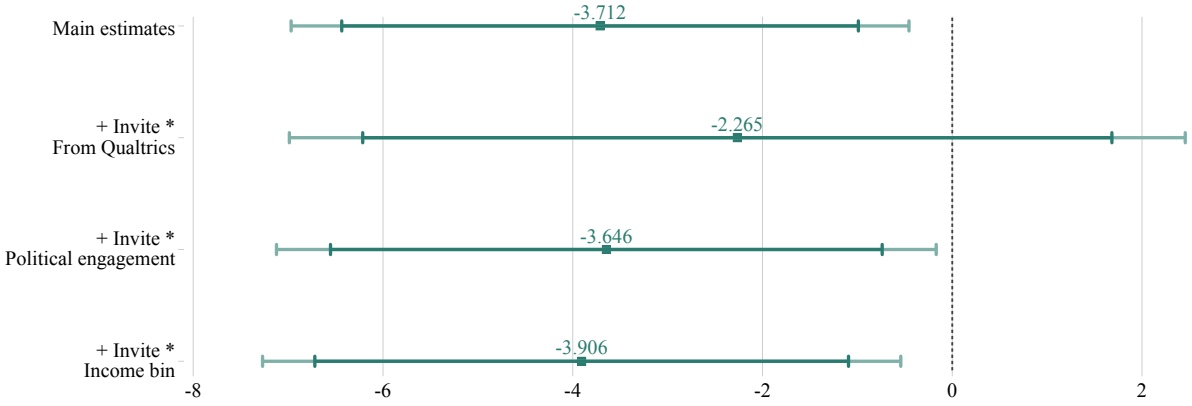



Figure A17: Sample profile showing matched Wave-2 participants

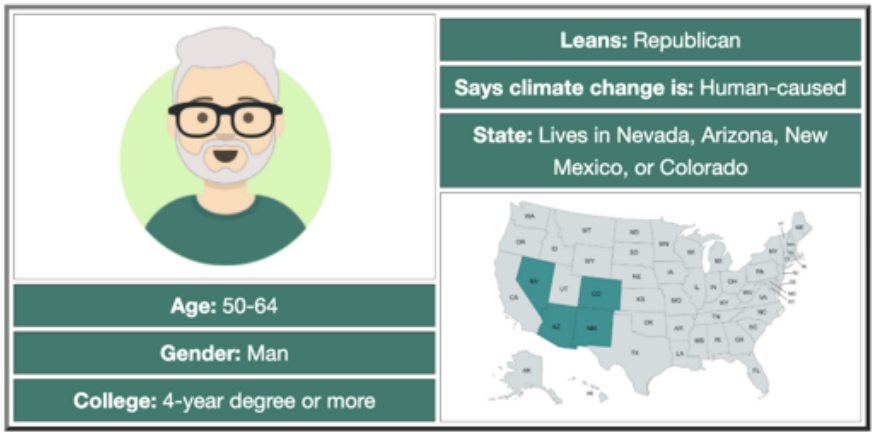


Figure A18: Treatment groups' comprehension of what future participants will see about you

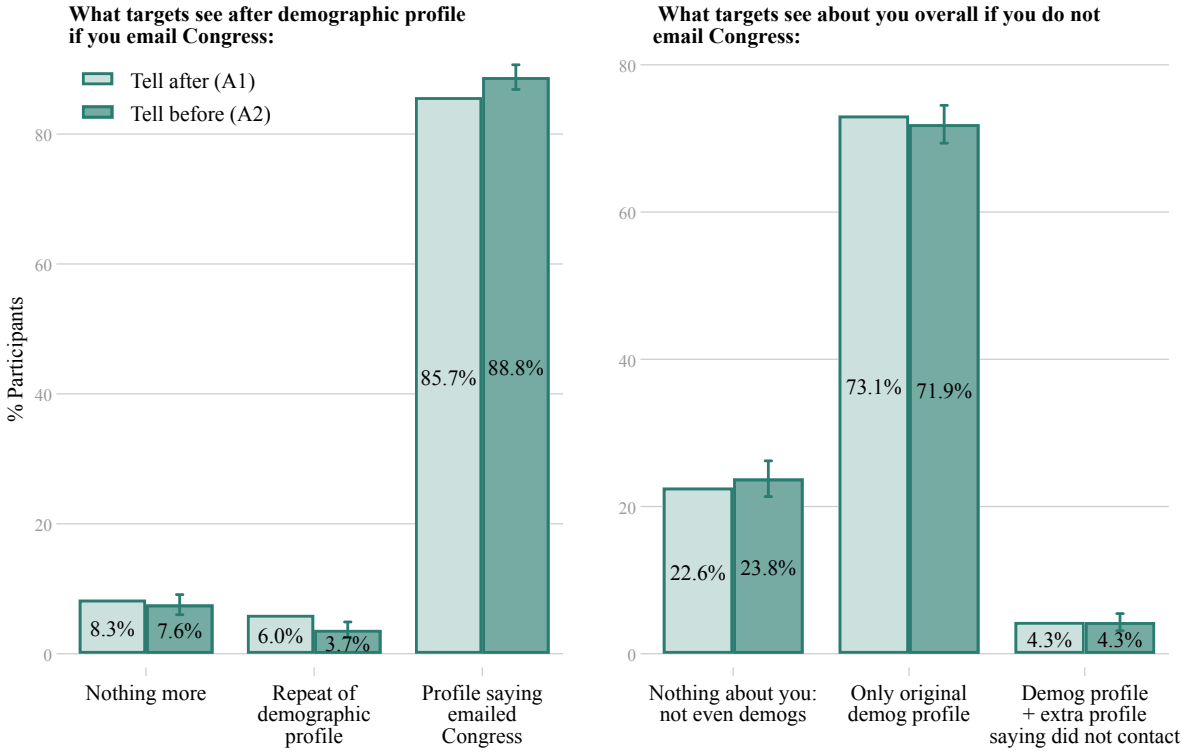
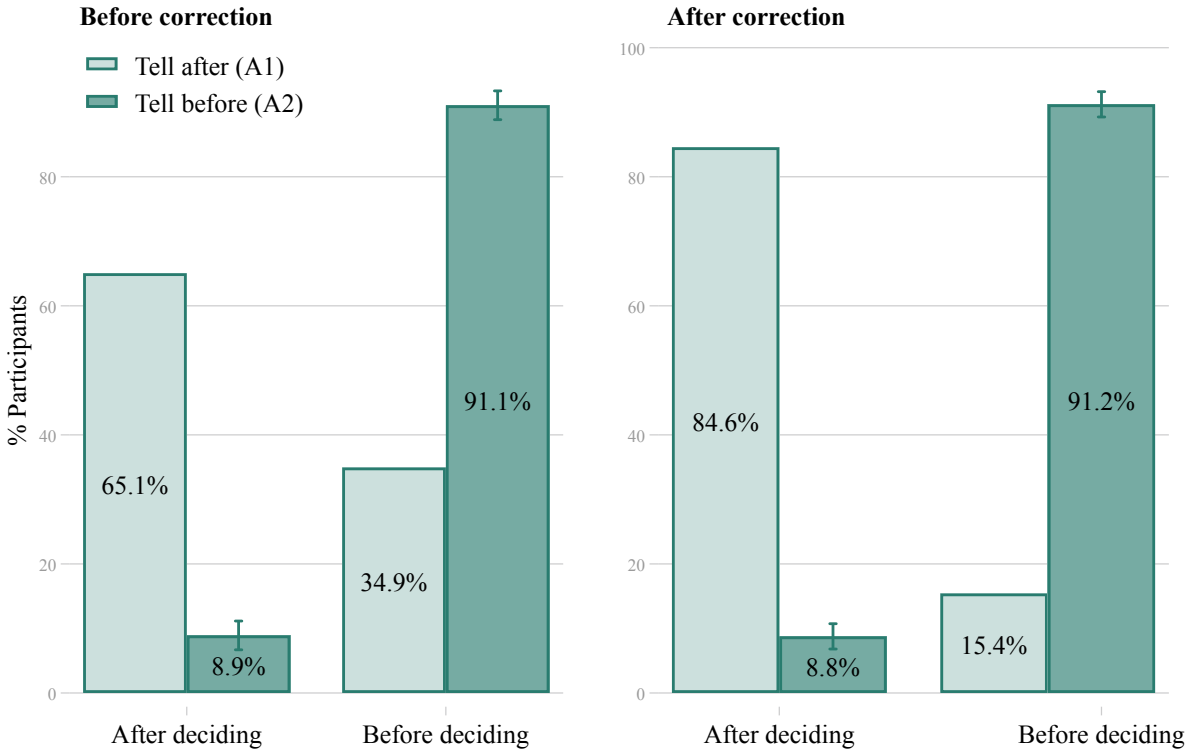


Figure A19: Treatment groups' comprehension of profile timing and opportunities to influence Wave 2

**Panel A.** When would future participants see the extra profile saying you emailed Congress?




**Panel B.** In theory, could your decision to contact Congress or not influence whether paired Wave-2 participants do so in the survey?

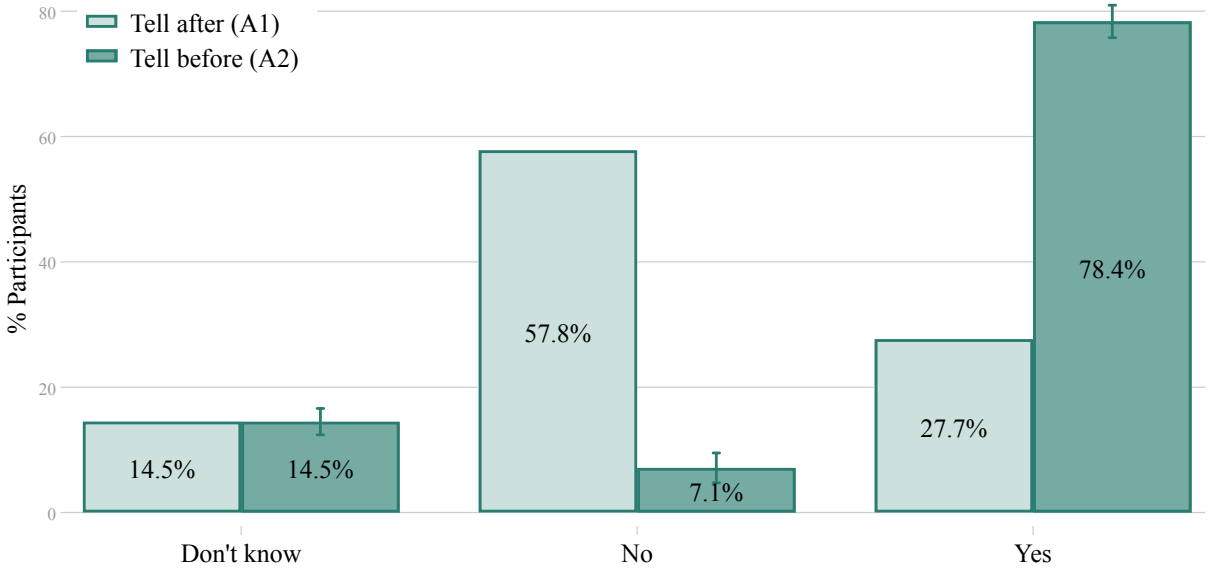
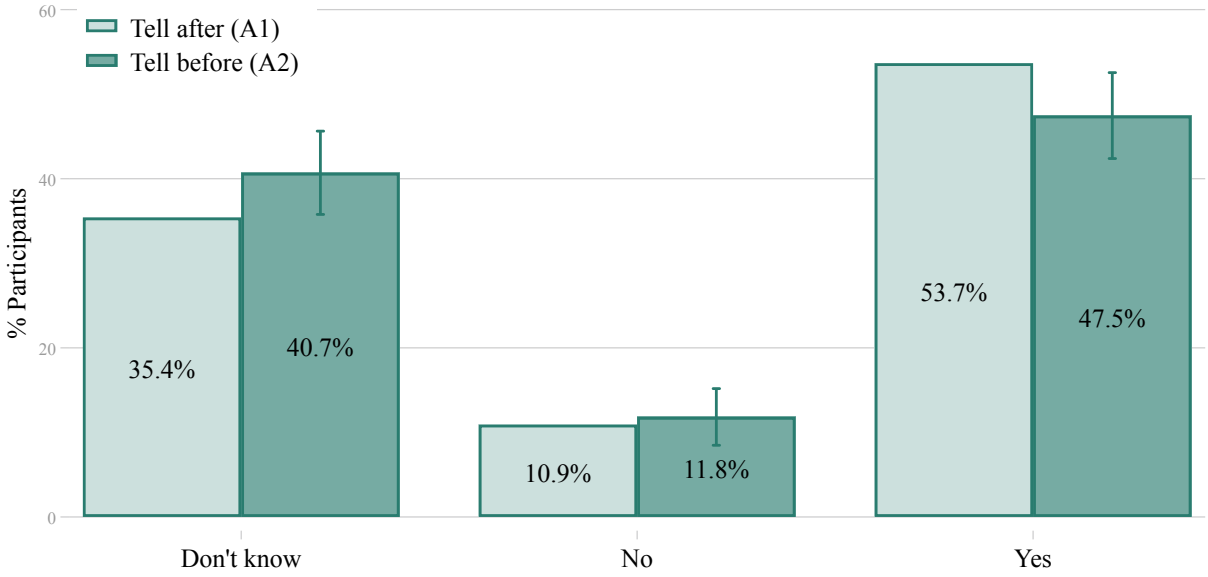


Figure A20: Wave 1 Beliefs on action after the survey

**Panel A.** In theory, could you influence whether targets do environmentally-friendly things after the survey?




**Panel B.** How likely is your decision to affect what targets do after the survey?

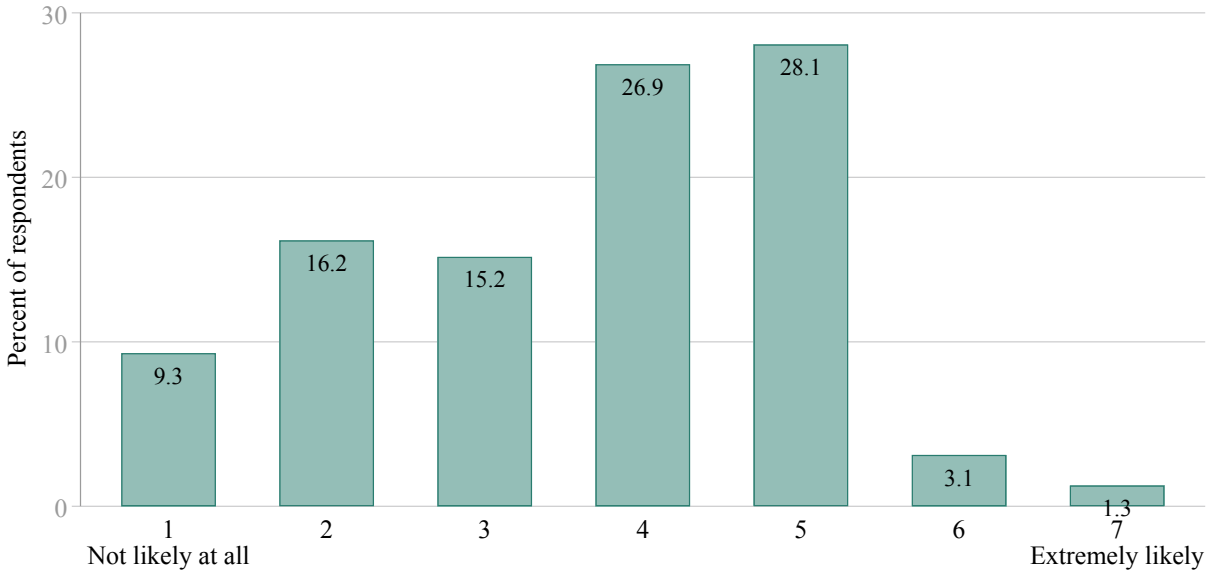


Figure A21: Specification chart: Say want to email Congress

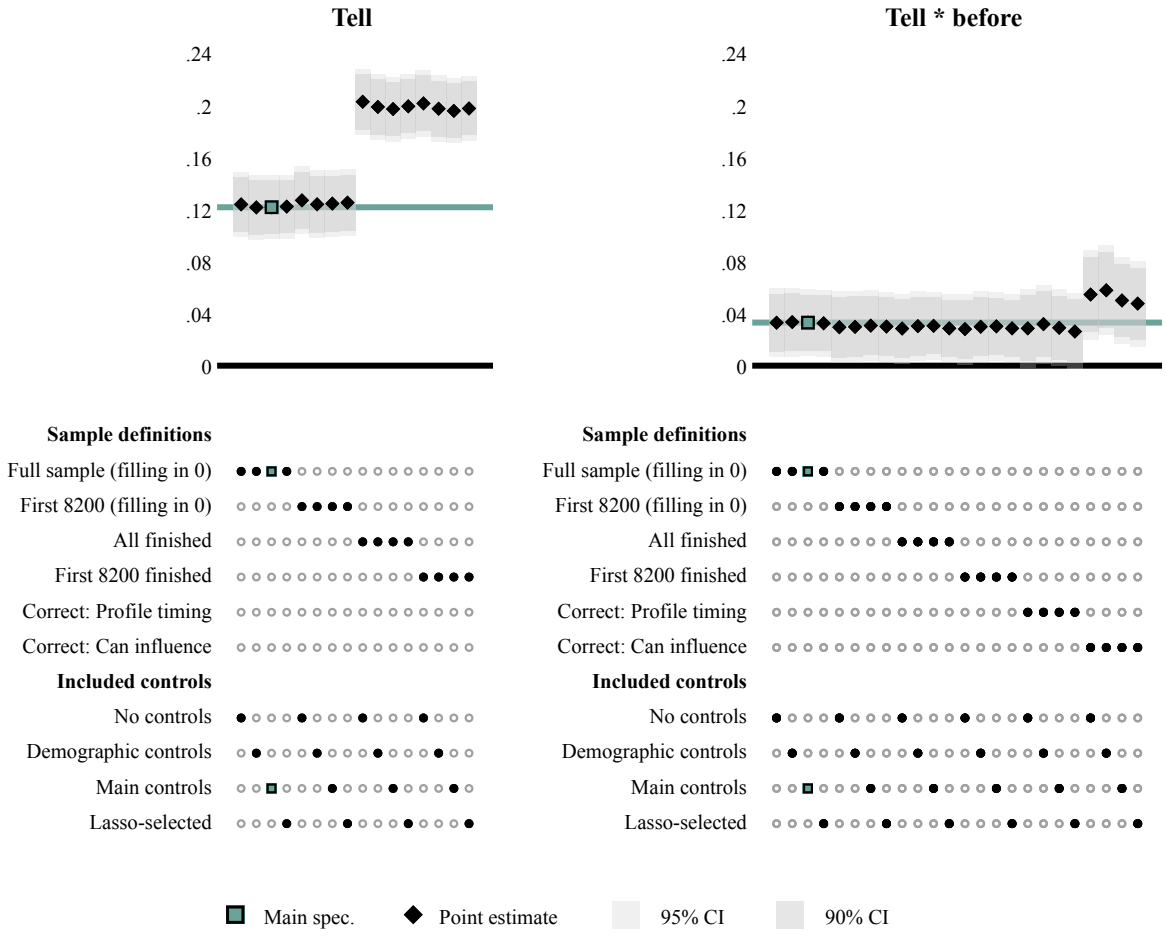


Figure A22: Specification chart: Have a record for emailing Congress

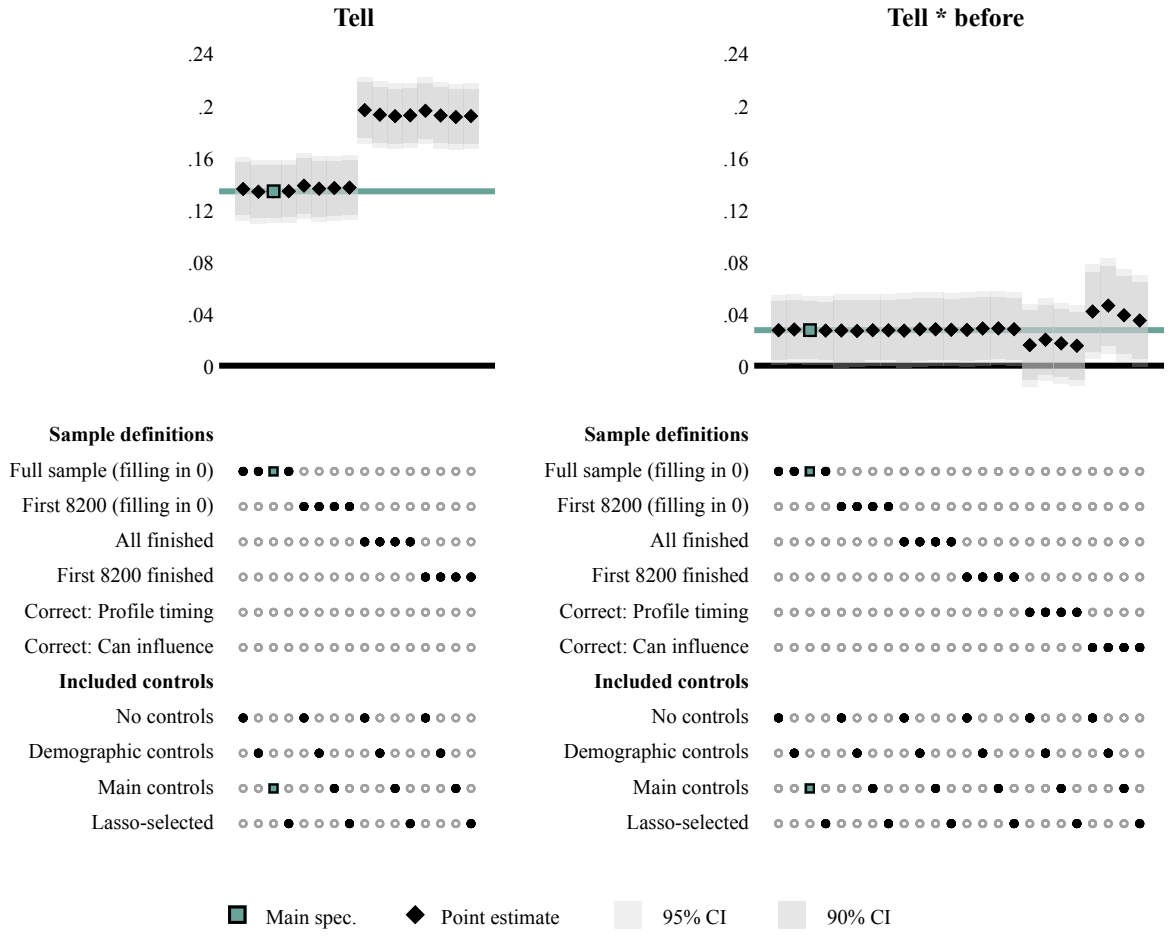
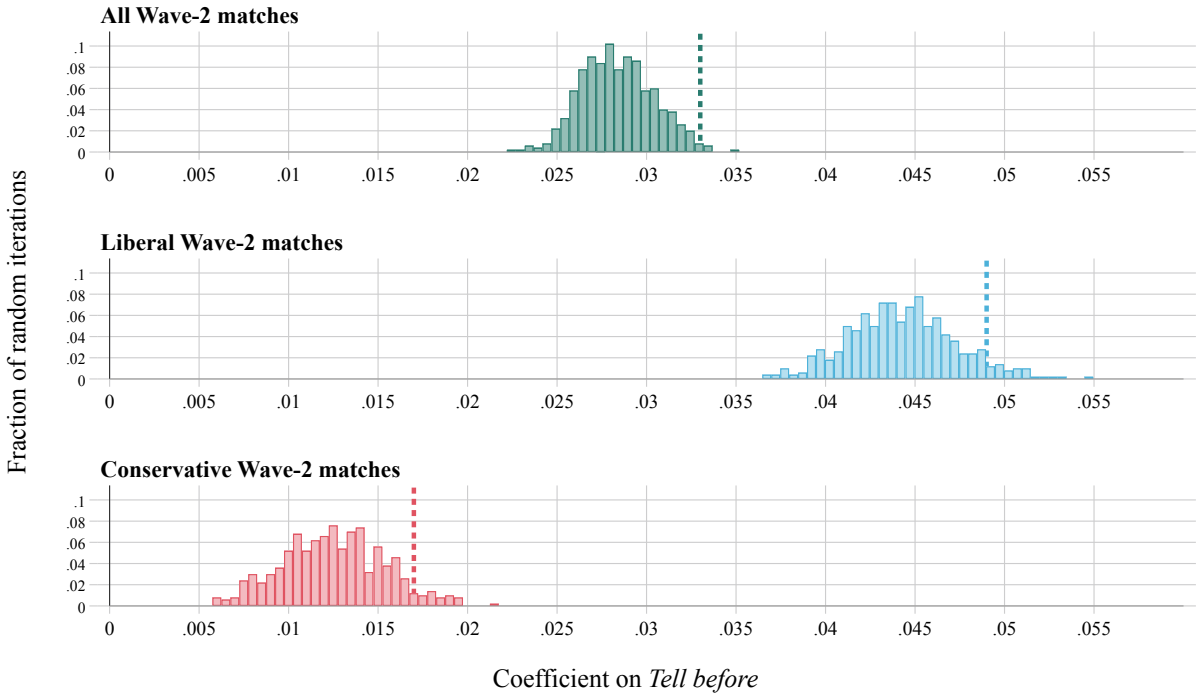


Figure A23: Point estimates for *Tell before* coefficient with adjustment for differential A2 demand effects

**Panel A.** Whether participants start the process of emailing Congress




**Panel B.** Whether participants match to records of emailing Congress

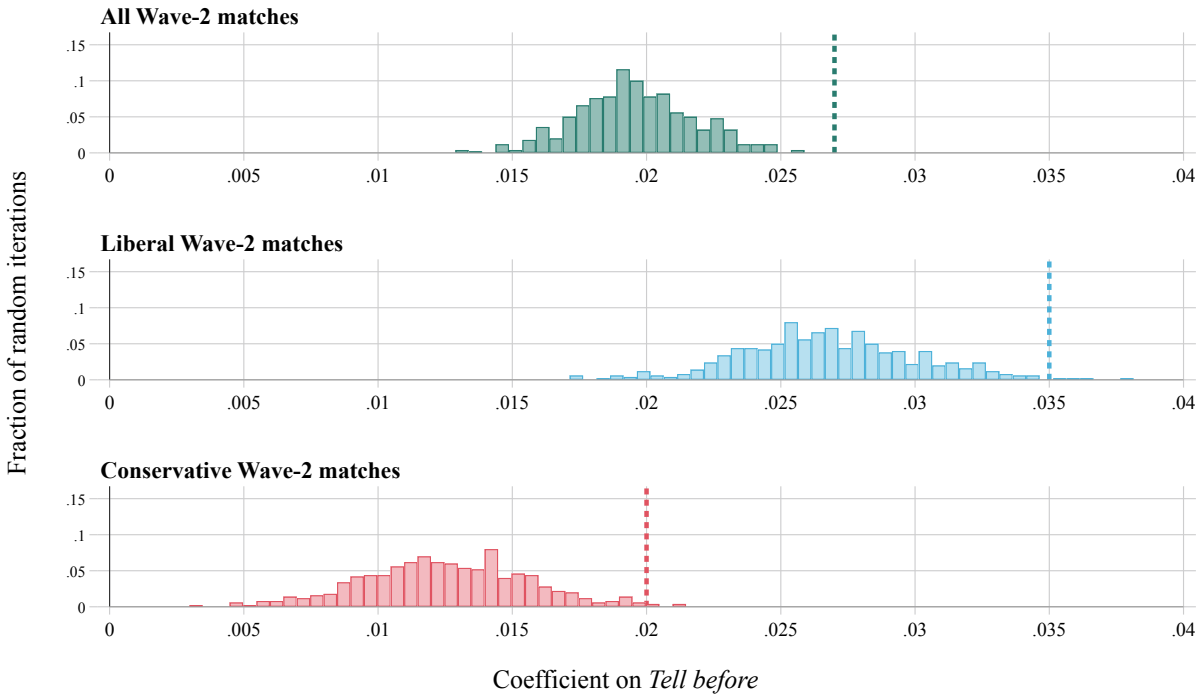


Figure A24: Qualitative vs. numeric influence beliefs

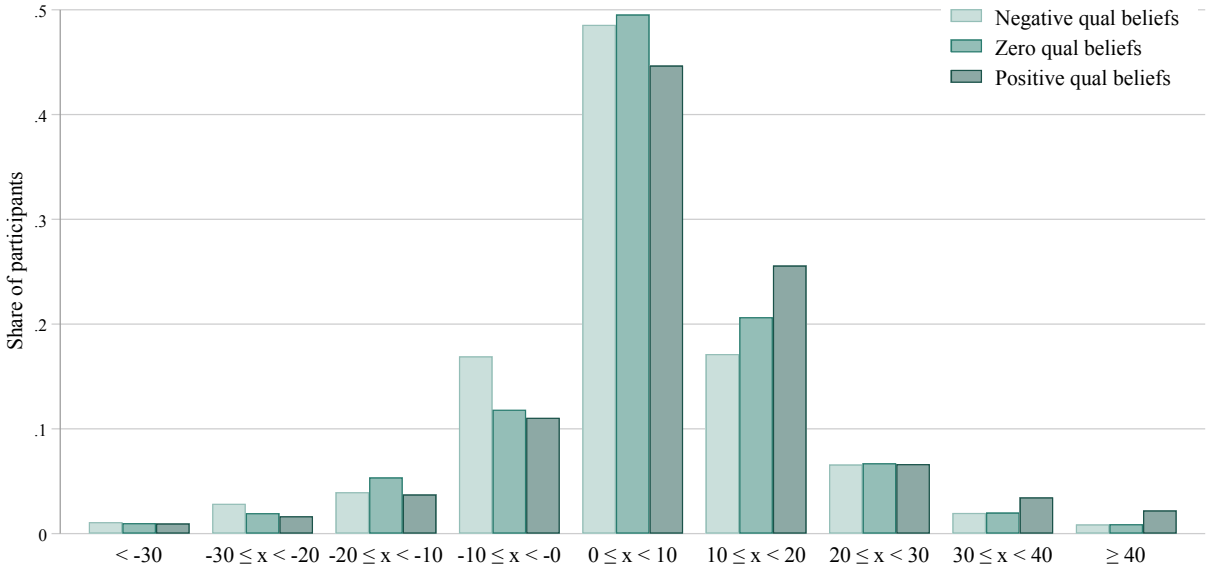
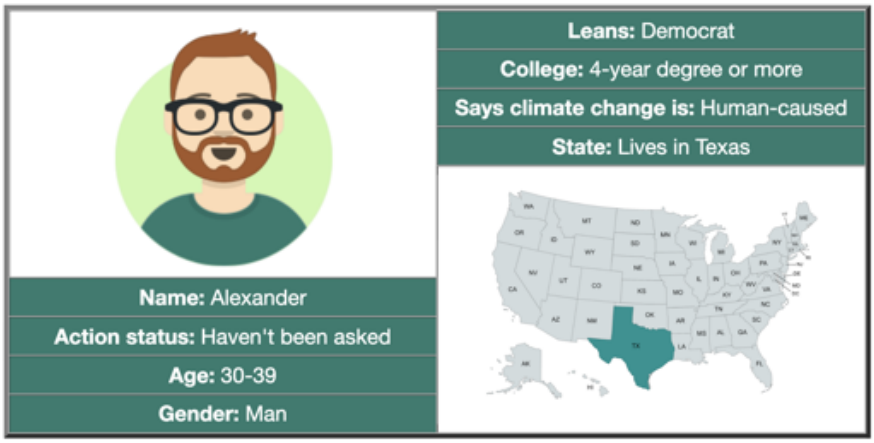



Figure A25: Examples of profiles of possible WTP matches: Desktop versions

(a) Before match, with climate-belief info




(b) After match, without climate-belief info

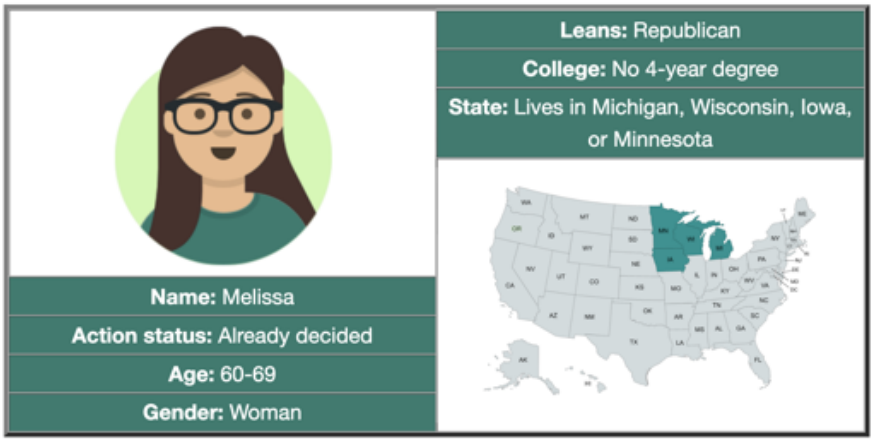


Figure A26: Examples of profiles of possible WTP matches: Mobile versions

(a) Before match, with climate-belief info


(b) After match, without climate-belief info

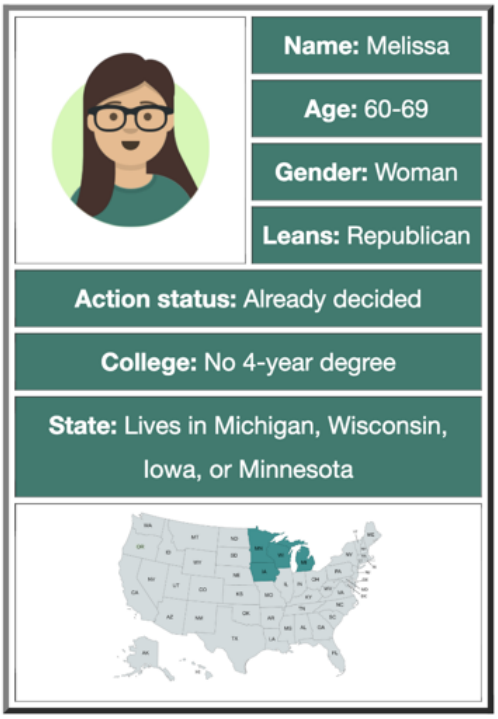



Figure A27: Distribution of accuracy on WTP comprehension questions

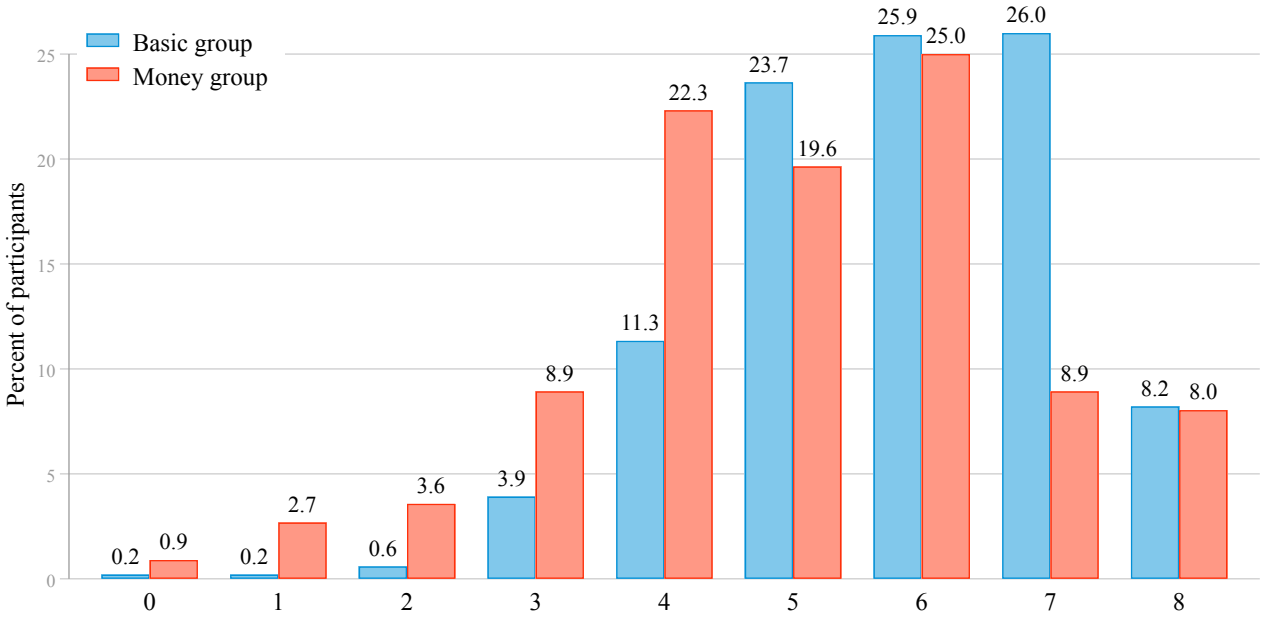


Figure A28: Main WTP results

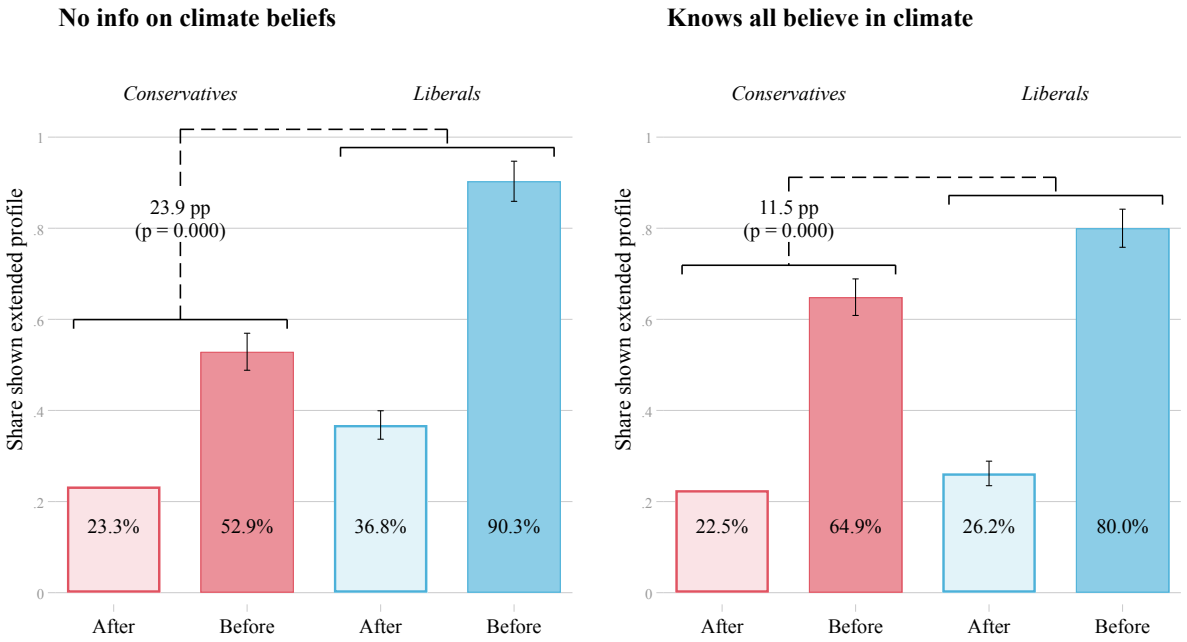
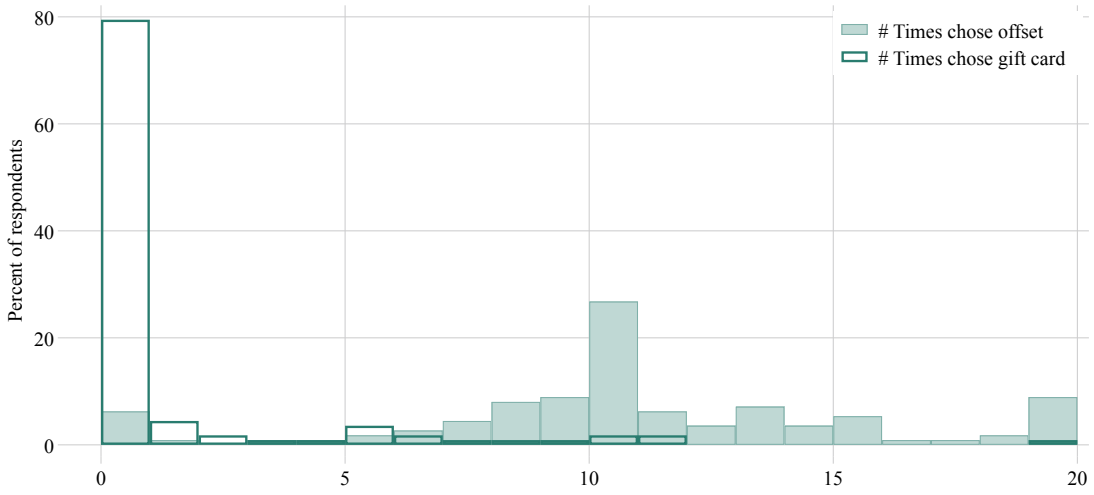


Figure A29: Share of respondents choosing offset donation vs. gift card

(a) Across all randomized amounts




(b) Among participants randomized to $5 or $6 amounts

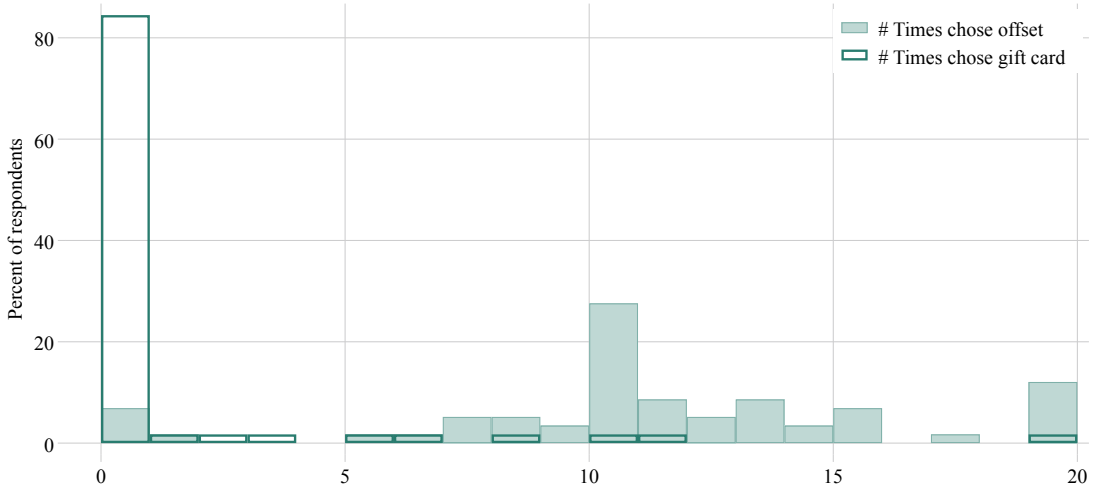
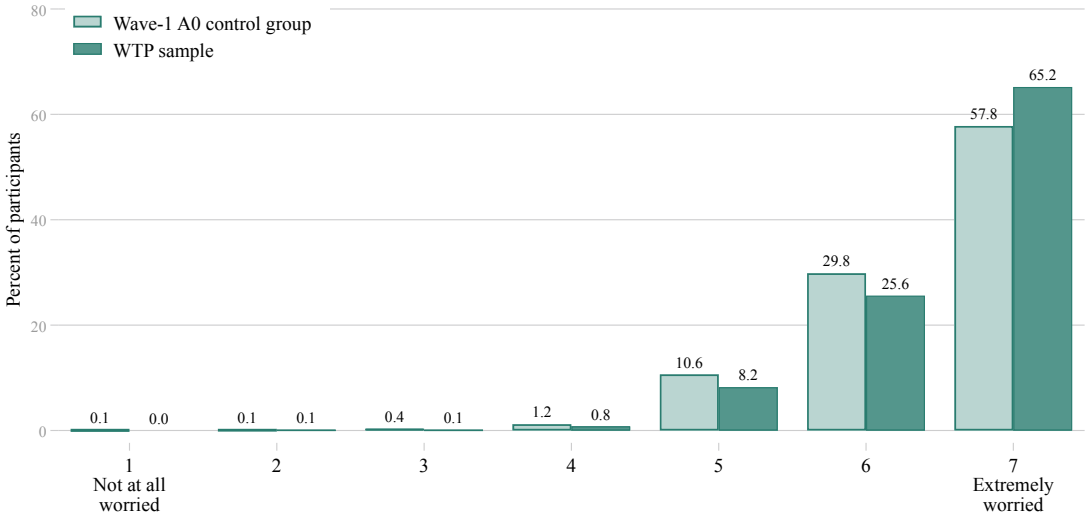
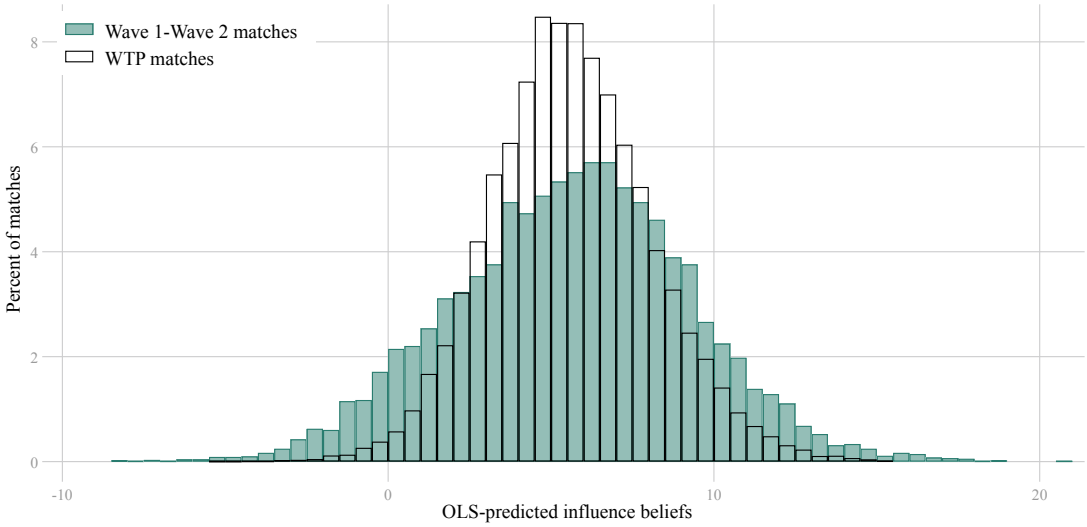


Figure A30: Comparing key heterogeneity dimensions across the Wave 1 and WTP samples

(a) Climate worry across Wave 1 and WTP




(b) OLS-predicted influence beliefs across matches in Wave 1 and WTP




(c) Lasso-predicted influence beliefs across matches in Wave 1 and WTP

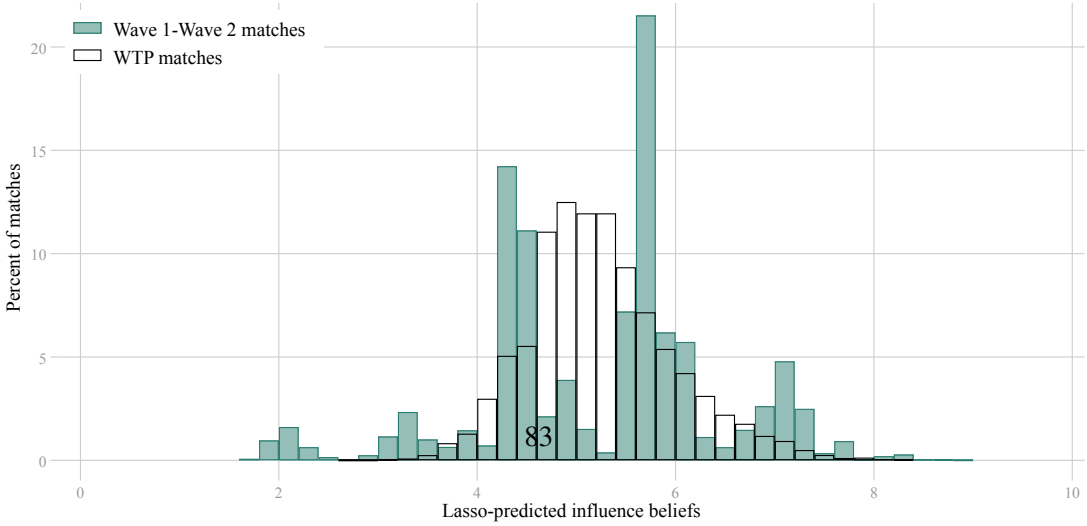


Figure A31: Specification chart: WTP experiment, Before

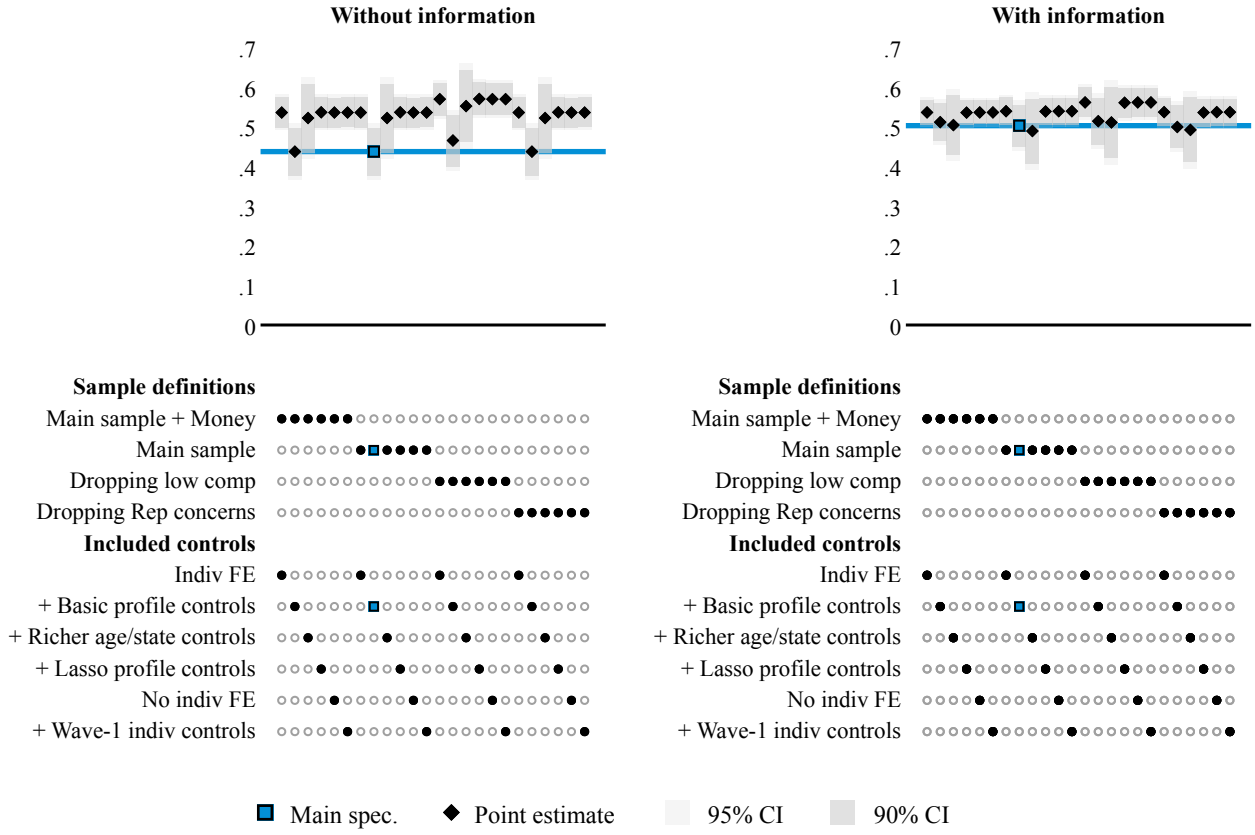


Figure A32: Specification chart: WTP experiment, Republican

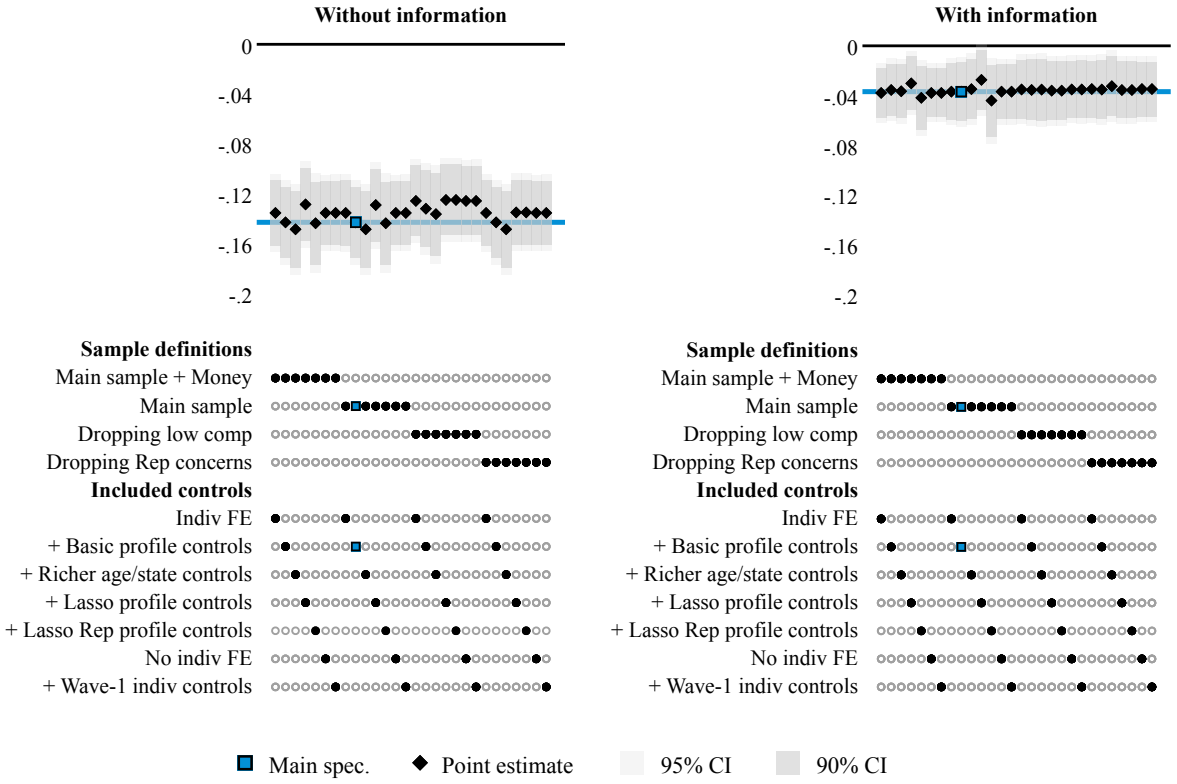


Figure A33: Specification chart: WTP experiment, Republican * Before

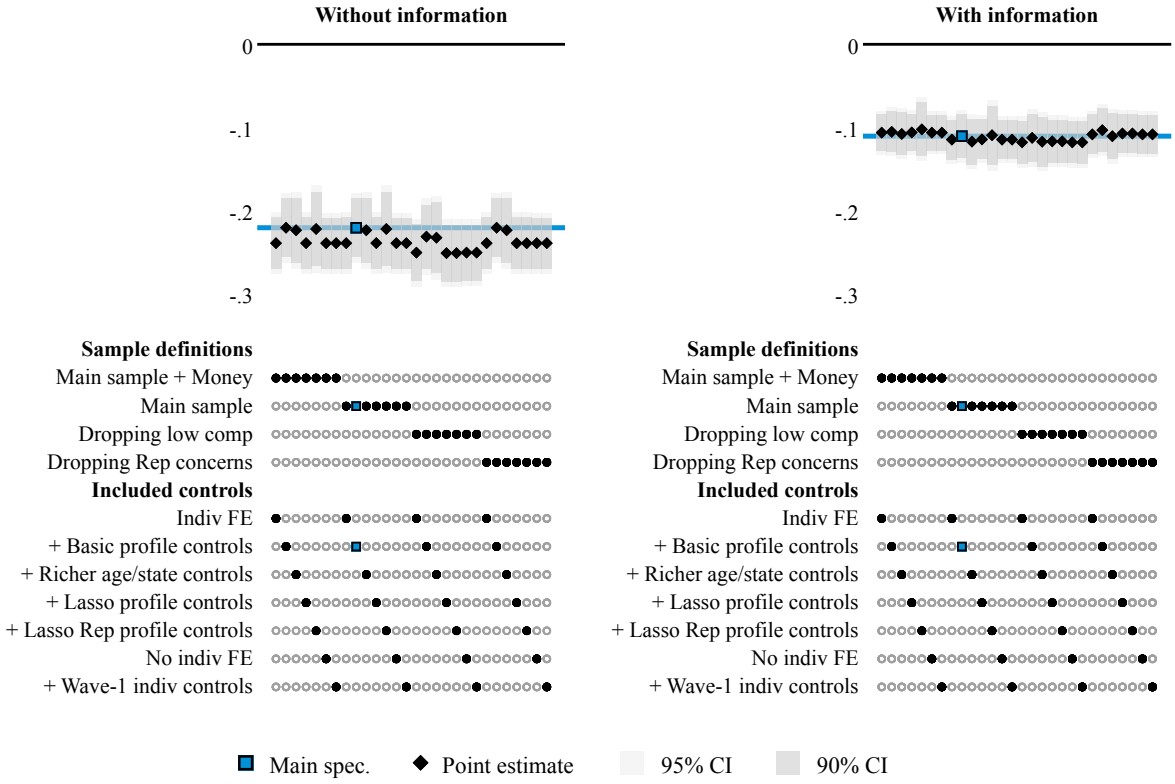


Figure A34: Heterogeneity in email choices by affective polarization

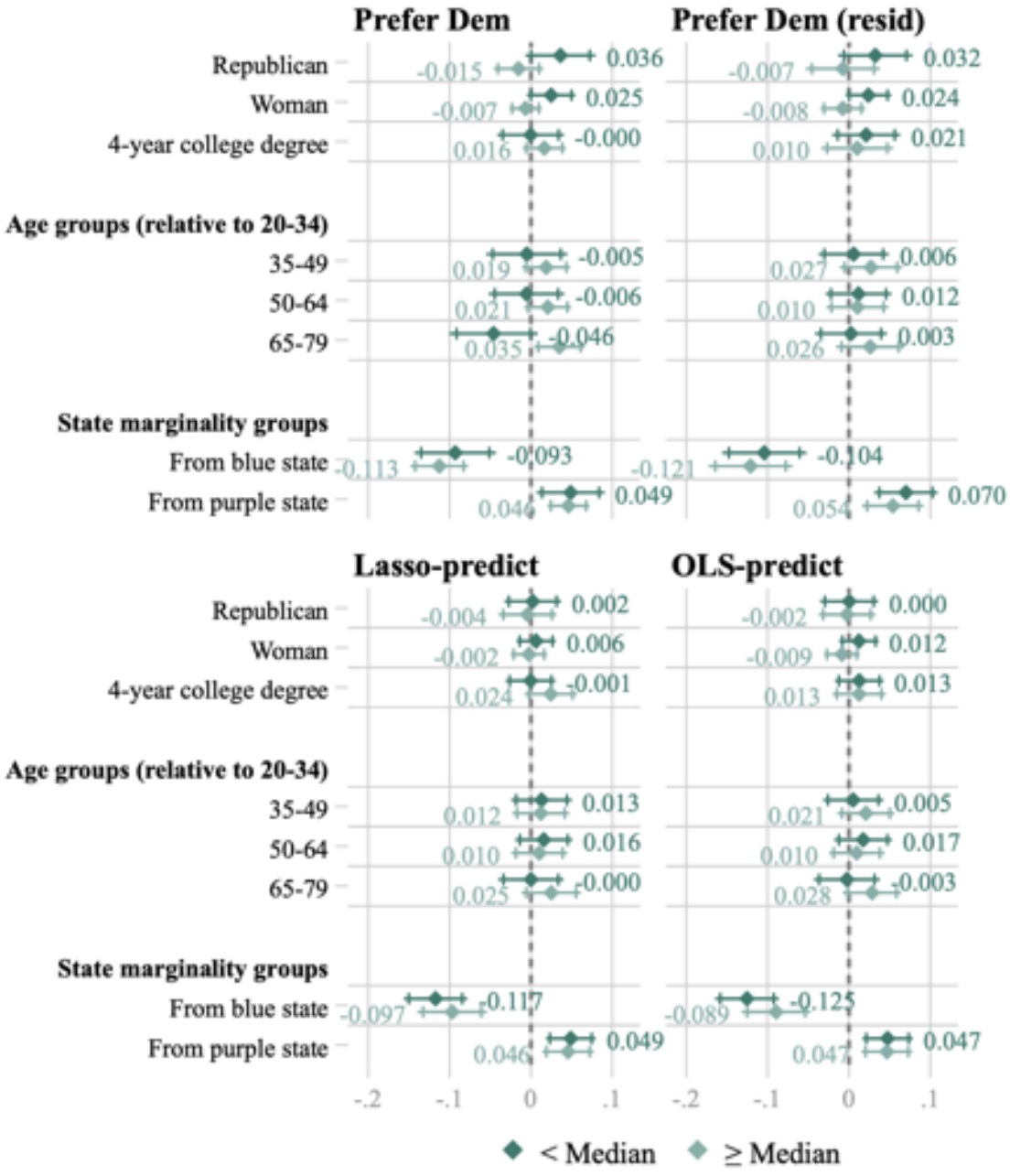
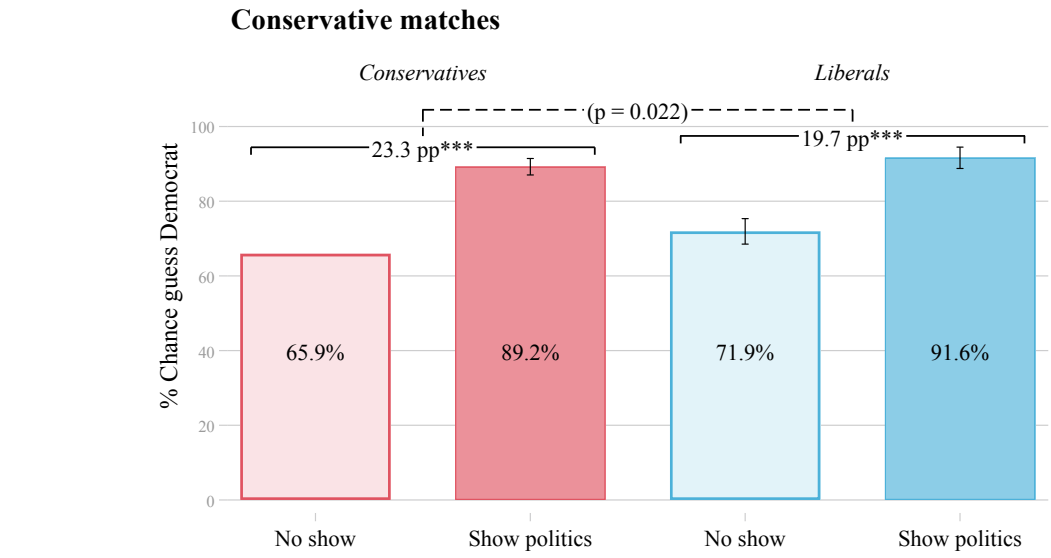


Figure A35: First stage on whether perceive Democrat, WTP round 2

**Panel A.** Overall first stage, split by whether liberal or conservative




**Panel B.** Overall first stage, split by state type in which you live

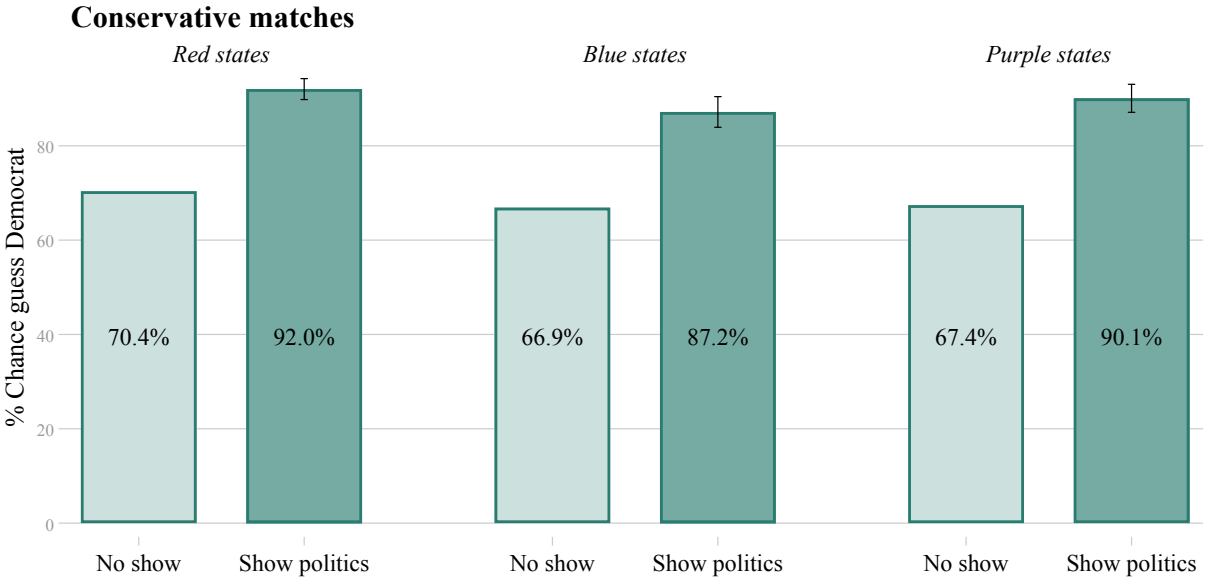
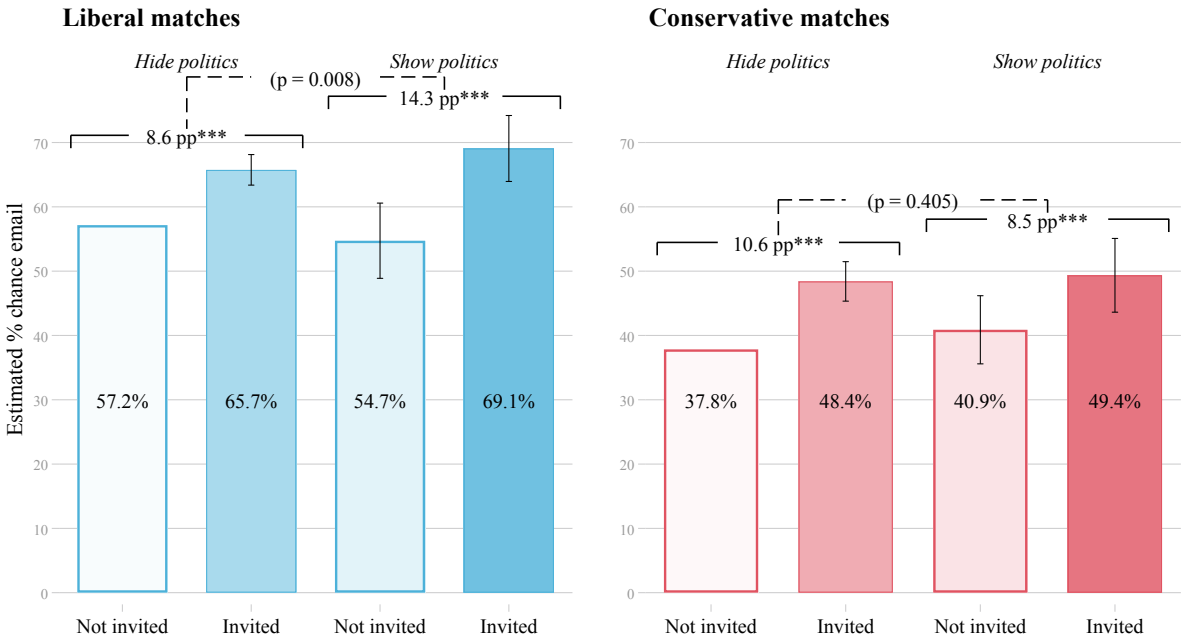


Figure A36: Influence beliefs if hide politics or not



## A.2 Tables

### A.2.1 Wave 2

Table A1: Comparing Wave-2 participants recruited via social media vs. Qualtrics Panels

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Liberals | | | Conservatives | | |
| | Social | Qualtrics | *p-value* | Social | Qualtrics | *p-value* |
| Woman | 0.581 | 0.492 | (0.000) | 0.393 | 0.528 | (0.001) |
| Hispanic | 1.043 | 1.111 | (0.000) | 1.055 | 1.104 | (0.010) |
| Has $\geq$ 4-year college degree | 0.790 | 0.604 | (0.000) | 0.755 | 0.338 | (0.000) |
| **Age ranges:** | | | | | | |
| 20-39 | 0.194 | 0.326 | (0.000) | 0.172 | 0.442 | (0.000) |
| 40-59 | 0.337 | 0.275 | (0.000) | 0.405 | 0.313 | (0.018) |
| 60-79 | 0.469 | 0.399 | (0.000) | 0.423 | 0.245 | (0.000) |
| **Income bins (USD):** | | | | | | |
| Less than 50,000 | 0.206 | 0.356 | (0.000) | 0.172 | 0.410 | (0.000) |
| 50,000-99,999 | 0.332 | 0.285 | (0.007) | 0.252 | 0.385 | (0.000) |
| 100,000-149,999 | 0.227 | 0.200 | (0.072) | 0.196 | 0.132 | (0.046) |
| 150,000-199,999 | 0.109 | 0.106 | (0.803) | 0.147 | 0.046 | (0.000) |
| 200,000 or more | 0.126 | 0.053 | (0.000) | 0.233 | 0.027 | (0.000) |
| **State marginality:** | | | | | | |
| Red state | 0.268 | 0.259 | (0.532) | 0.362 | 0.416 | (0.166) |
| Blue state | 0.409 | 0.469 | (0.002) | 0.387 | 0.250 | (0.000) |
| Purple state | 0.323 | 0.273 | (0.003) | 0.252 | 0.334 | (0.019) |
| **Climate beliefs:** | | | | | | |
| Climate worry (1-7) | 6.325 | 6.180 | (0.000) | 4.485 | 4.980 | (0.000) |
| Desire for climate action (1-7) | 6.680 | 6.389 | (0.000) | 4.423 | 5.263 | (0.000) |
| Perceived local impacts (1-7) | 5.439 | 5.696 | (0.000) | 4.172 | 4.875 | (0.000) |
| **Political engage. and beliefs:** | | | | | | |
| Member of resp. party | 0.223 | 0.785 | (0.000) | 0.460 | 0.758 | (0.000) |
| Political engage. index (std) | 1.512 | -0.339 | (0.000) | -0.863 | -2.102 | (0.000) |
| Prev. contacted reps | 0.246 | 0.372 | (0.000) | 0.129 | 0.224 | (0.000) |
| Prev. donated | 0.744 | 0.410 | (0.000) | 0.417 | 0.231 | (0.000) |
| Prev. canvassed | 0.061 | 0.081 | (0.046) | 0.061 | 0.039 | (0.247) |
| Prev. signed petition | 0.800 | 0.490 | (0.000) | 0.552 | 0.380 | (0.000) |
| Prev. phonebanked | 0.077 | 0.081 | (0.689) | 0.025 | 0.046 | (0.106) |
| Political efficacy index (std) | -0.111 | -0.102 | (0.653) | 0.133 | 0.189 | (0.205) |
| Prefer friend of own party (1-7) | 5.698 | 5.492 | (0.000) | 4.577 | 4.913 | (0.000) |
| Sample size | 4165 | 862 | | 163 | 2791 | |

Table A2: Wave-2 attrition

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| *Panel A: Attrition through observing emails* | | | | | | |
| | | | | | Social-media sample | |
| | Observe | Saw letter preview | | | Answered | Correct |
| | if email | All | Qual. | Social | attent. check | attent. check |
| Show profile | 0.005 | 0.009** | 0.000 | 0.016** | -0.001 | 0.018** |
| | (0.005) | (0.004) | (0.000) | (0.007) | (0.002) | (0.007) |
| Control means | 0.915 | 0.933 | 1.000 | 0.884 | 0.955 | 0.926 |
| Sample size | 8616 | 8616 | 3653 | 4963 | 4963 | 4755 |
| *Panel B: Attrition in secondary outcomes* | | | | | | |
| | | Have non-missing answer for: | | | | |
| | | *Secondary set 1* | | | *Secondary set 2* | |
| | Demand | Others' | How | Therm. | Policy | Social |
| | effect | action | worth. | questions | beliefs | norms |
| Show profile | 0.003 | -0.002 | -0.002 | -0.003 | 0.005 | 0.004 |
| | (0.006) | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) |
| Control means | 0.895 | 0.445 | 0.445 | 0.440 | 0.437 | 0.437 |
| Sample size | 8616 | 8616 | 8616 | 8616 | 8616 | 8616 |

Table A3: Main Wave-2 results, split by source and party

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Social-media sample | | | Qualtrics sample | | |
| | *All* | *Dem* | *Rep* | *All* | *Dem* | *Rep* |
| *Panel A: Start the process of emailing Congress* | | | | | | |
| Treatment | 0.068*** | 0.069*** | 0.208* | 0.046*** | 0.061* | 0.041** |
| | (0.015) | (0.015) | (0.107) | (0.014) | (0.032) | (0.016) |
| Control mean | 0.428 | 0.436 | 0.220 | 0.277 | 0.374 | 0.245 |
| N | 4328 | 4165 | 163 | 3653 | 862 | 2791 |
| Dem coefficient equal by source (2) = (5)? *p-value* = 0.818 | | | | | | |
| Rep coefficient equal by source (3) = (6)? *p-value* = 0.035 | | | | | | |
| *Panel B: Have a record of emailing Congress* | | | | | | |
| Treatment | 0.061*** | 0.063*** | 0.085 | 0.025** | 0.044* | 0.020* |
| | (0.014) | (0.014) | (0.074) | (0.010) | (0.026) | (0.011) |
| Control mean | 0.264 | 0.271 | 0.085 | 0.096 | 0.132 | 0.084 |
| N | 4328 | 4165 | 163 | 3653 | 862 | 2791 |
| Dem coefficient equal by source (2) = (5)? *p-value* = 0.490 | | | | | | |
| Rep coefficient equal by source (3) = (6)? *p-value* = 0.234 | | | | | | |

Table A4: Wave-2: Heterogeneous impacts on **starting the email process** by party affiliation and polarization

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | Opted into email process | | | | | | | | | |
| | *Liberal* | | | | | *Conservative* | | | | |
| Treatment | 0.079*** | 0.074*** | 0.058** | 0.049** | 0.056*** | 0.002 | 0.055** | 0.083** | 0.068*** | 0.071*** |
| | (0.020) | (0.019) | (0.028) | (0.019) | (0.019) | (0.092) | (0.022) | (0.033) | (0.023) | (0.023) |
| **Prefer friend of own party** | | | | | | | | | | |
| $\geq$ Median | -0.010 | | | | | -0.026 | | | | |
| | (0.020) | | | | | (0.063) | | | | |
| Treatment * $\geq$ Median | -0.019 | | | | | 0.043 | | | | |
| | (0.027) | | | | | (0.093) | | | | |
| **Prefer friend of own party (Resid)** | | | | | | | | | | |
| $\geq$ Median | | -0.017 | | | | | 0.044** | | | |
| | | (0.019) | | | | | (0.021) | | | |
| Treatment * $\geq$ Median | | -0.011 | | | | | -0.021 | | | |
| | | (0.027) | | | | | (0.031) | | | |
| **Affective-polarization thermometer** | | | | | | | | | | |
| $\geq$ Median | | | -0.042 | | | | | -0.015 | | |
| | | | (0.029) | | | | | (0.031) | | |
| Treatment * $\geq$ Median | | | 0.016 | | | | | -0.043 | | |
| | | | (0.040) | | | | | (0.044) | | |
| **OLS-predicted thermometer** | | | | | | | | | | |
| $\geq$ Median | | | | -0.043* | | | | | 0.028 | |
| | | | | (0.023) | | | | | (0.026) | |
| Treatment * $\geq$ Median | | | | 0.040 | | | | | -0.049 | |
| | | | | (0.027) | | | | | (0.031) | |
| **Lasso-predicted thermometer** | | | | | | | | | | |
| $\geq$ Median | | | | | -0.055** | | | | | 0.042* |
| | | | | | (0.023) | | | | | (0.025) |
| Treatment * $\geq$ Median | | | | | 0.028 | | | | | -0.055* |
| | | | | | (0.027) | | | | | (0.031) |
| Control mean | 0.425 | 0.425 | 0.425 | 0.425 | 0.425 | 0.244 | 0.244 | 0.244 | 0.244 | 0.244 |
| N | 5027 | 5027 | 2360 | 5027 | 5027 | 2954 | 2954 | 1467 | 2954 | 2954 |

Table A5: Wave-2: Heterogeneous impacts on **email records** by party affiliation and polarization

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | Has an email record | | | | | | | | | |
| | *Liberal* | | | | | *Conservative* | | | | |
| Treatment | 0.065*** | 0.063*** | 0.067*** | 0.050*** | 0.057*** | -0.023 | 0.038** | 0.021 | 0.026 | 0.037** |
| | (0.018) | (0.017) | (0.026) | (0.017) | (0.017) | (0.044) | (0.015) | (0.023) | (0.017) | (0.017) |
| **Prefer friend of own party** | | | | | | | | | | |
| $\geq$ Median | -0.014 | | | | | 0.046 | | | | |
| | (0.017) | | | | | (0.032) | | | | |
| Treatment * $\geq$ Median | -0.013 | | | | | 0.045 | | | | |
| | (0.025) | | | | | (0.045) | | | | |
| **Prefer friend of own party (Resid)** | | | | | | | | | | |
| $\geq$ Median | | -0.023 | | | | | 0.019 | | | |
| | | (0.017) | | | | | (0.015) | | | |
| Treatment * $\geq$ Median | | -0.010 | | | | | -0.032 | | | |
| | | (0.025) | | | | | (0.021) | | | |
| **Affective-polarization thermometer** | | | | | | | | | | |
| $\geq$ Median | | | -0.026 | | | | | -0.003 | | |
| | | | (0.026) | | | | | (0.022) | | |
| Treatment * $\geq$ Median | | | 0.001 | | | | | -0.009 | | |
| | | | (0.037) | | | | | (0.031) | | |
| **OLS-predicted thermometer** | | | | | | | | | | |
| $\geq$ Median | | | | -0.028 | | | | | -0.015 | |
| | | | | (0.021) | | | | | (0.017) | |
| Treatment * $\geq$ Median | | | | 0.017 | | | | | -0.010 | |
| | | | | (0.025) | | | | | (0.022) | |
| **Lasso-predicted thermometer** | | | | | | | | | | |
| $\geq$ Median | | | | | -0.039* | | | | | -0.001 |
| | | | | | (0.020) | | | | | (0.017) |
| Treatment * $\geq$ Median | | | | | 0.003 | | | | | -0.031 |
| | | | | | (0.025) | | | | | (0.022) |
| Control mean | 0.247 | 0.247 | 0.247 | 0.247 | 0.247 | 0.084 | 0.084 | 0.084 | 0.084 | 0.084 |
| N | 5027 | 5027 | 2360 | 5027 | 5027 | 2954 | 2954 | 1467 | 2954 | 2954 |

Table A6: Wave 2: Heterogeneous effects of invitations on opting into email process by match similarity

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Opted into email process | | | | | | | |
| Treatment | 0.039** | 0.045*** | 0.050*** | 0.056*** | 0.049*** | 0.018 | 0.042*** | 0.018 |
| | (0.015) | (0.014) | (0.015) | (0.011) | (0.012) | (0.021) | (0.015) | (0.021) |
| **Politics** | | | | | | | | |
| Same party | 0.003 | | | | | 0.005 | 0.006 | 0.006 |
| | (0.022) | | | | | (0.023) | (0.022) | (0.022) |
| Treatment * Same party | 0.028 | | | | | 0.025 | 0.024 | 0.022 |
| | (0.021) | | | | | (0.021) | (0.021) | (0.021) |
| **Gender** | | | | | | | | |
| Same gender | | 0.010 | | | | 0.009 | | |
| | | (0.014) | | | | (0.015) | | |
| Treatment * Same gender | | 0.025 | | | | 0.026 | | |
| | | (0.021) | | | | (0.021) | | |
| **Education** | | | | | | | | |
| Same college attainment | | | 0.005 | | | 0.008 | | |
| | | | (0.016) | | | (0.016) | | |
| Treatment * Same college | | | 0.013 | | | 0.002 | | |
| | | | (0.021) | | | (0.022) | | |
| **State** | | | | | | | | |
| Same state group | | | | -0.027 | | -0.027 | | |
| | | | | (0.025) | | (0.025) | | |
| Treatment * Same state | | | | 0.020 | | 0.023 | | |
| | | | | (0.035) | | (0.036) | | |
| **Age range** | | | | | | | | |
| Same age range | | | | | -0.008 | -0.008 | | |
| | | | | | (0.016) | (0.016) | | |
| Treatment * Same age | | | | | 0.028 | 0.026 | | |
| | | | | | (0.022) | (0.023) | | |
| **Overall non-political similarity** | | | | | | | | |
| Pred. non-pol sim | | | | | | | -0.001 | |
| | | | | | | | (0.008) | |
| Treatment * Pred. non-pol sim | | | | | | | 0.017* | |
| | | | | | | | (0.010) | |
| # Non-pol matched traits | | | | | | | | 0.001 |
| | | | | | | | | (0.008) |
| Treatment* # Non-pol matches | | | | | | | | 0.018 |
| | | | | | | | | (0.011) |
| Control mean | 0.244 | 0.355 | 0.327 | 0.359 | 0.355 | 0.244 | 0.244 | 0.244 |
| N | 7981 | 7981 | 7981 | 7981 | 7981 | 7981 | 7981 | 7981 |

Table A7: Wave 2: Heterogeneous effects of invitations on email records by match similarity

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Has an email record | | | | | | | |
| Treatment | 0.019* | 0.040*** | 0.035*** | 0.042*** | 0.033*** | 0.003 | 0.022** | 0.002 |
| | (0.011) | (0.012) | (0.012) | (0.009) | (0.010) | (0.016) | (0.011) | (0.016) |
| **Politics** | | | | | | | | |
| Same party | -0.013 | | | | | -0.014 | -0.011 | -0.011 |
| | (0.017) | | | | | (0.017) | (0.017) | (0.017) |
| Treatment * Same party | 0.038** | | | | | 0.035** | 0.034** | 0.033** |
| | (0.016) | | | | | (0.017) | (0.017) | (0.017) |
| **Gender** | | | | | | | | |
| Same gender | | -0.001 | | | | -0.003 | | |
| | | (0.012) | | | | (0.012) | | |
| Treatment * Same gender | | 0.008 | | | | 0.009 | | |
| | | (0.017) | | | | (0.018) | | |
| **Education** | | | | | | | | |
| Same college attainment | | | 0.019 | | | 0.025* | | |
| | | | (0.013) | | | (0.014) | | |
| Treatment * Same college | | | 0.015 | | | 0.003 | | |
| | | | (0.017) | | | (0.018) | | |
| **State** | | | | | | | | |
| Same state group | | | | -0.014 | | -0.014 | | |
| | | | | (0.022) | | (0.022) | | |
| Treatment * Same state | | | | 0.018 | | 0.021 | | |
| | | | | (0.031) | | (0.031) | | |
| **Age range** | | | | | | | | |
| Same age range | | | | | -0.018 | -0.019 | | |
| | | | | | (0.013) | (0.013) | | |
| Treatment * Same age | | | | | 0.035* | 0.032* | | |
| | | | | | (0.019) | (0.019) | | |
| **Overall non-political similarity** | | | | | | | | |
| Pred. non-pol sim | | | | | | | -0.008 | |
| | | | | | | | (0.006) | |
| Treatment * Pred. non-pol sim | | | | | | | 0.018** | |
| | | | | | | | (0.009) | |
| # Non-pol matched traits | | | | | | | | -0.000 |
| | | | | | | | | (0.007) |
| Treatment* # Non-pol matches | | | | | | | | 0.014 |
| | | | | | | | | (0.010) |
| Control mean | 0.084 | 0.191 | 0.155 | 0.187 | 0.186 | 0.084 | 0.084 | 0.084 |
| N | 7981 | 7981 | 7981 | 7981 | 7981 | 7981 | 7981 | 7981 |

Table A8: Wave 2: Heterogeneity by Wave-1 inviter traits

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | Opted into email process | | | | | Has an email record | | | | |
| Treatment | 0.049*** | 0.060*** | 0.080*** | 0.024 | 0.053 | 0.039*** | 0.032* | 0.041** | 0.016 | -0.005 |
| | (0.014) | (0.021) | (0.020) | (0.027) | (0.040) | (0.012) | (0.018) | (0.017) | (0.023) | (0.034) |
| **Gender** | | | | | | | | | | |
| Woman | 0.004 | | | | 0.007 | 0.002 | | | | 0.011 |
| | (0.015) | | | | (0.015) | (0.012) | | | | (0.013) |
| Treatment * Woman | 0.018 | | | | 0.016 | 0.011 | | | | 0.005 |
| | (0.021) | | | | (0.022) | (0.017) | | | | (0.019) |
| **Education** | | | | | | | | | | |
| Has 4-year college degree | | -0.009 | | | -0.010 | | -0.007 | | | -0.013 |
| | | (0.017) | | | (0.017) | | (0.014) | | | (0.015) |
| Treatment * 4-year college | | -0.004 | | | -0.005 | | 0.015 | | | 0.017 |
| | | (0.024) | | | (0.024) | | (0.020) | | | (0.021) |
| **State marginality groups** | | | | | | | | | | |
| Blue state | | | 0.007 | | 0.004 | | | -0.022 | | -0.031** |
| | | | (0.018) | | (0.019) | | | (0.015) | | (0.016) |
| Before * Blue state | | | -0.016 | | -0.014 | | | 0.011 | | 0.016 |
| | | | (0.026) | | (0.027) | | | (0.022) | | (0.023) |
| Purple state | | | 0.032* | | 0.029 | | | 0.001 | | -0.005 |
| | | | (0.018) | | (0.018) | | | (0.015) | | (0.016) |
| Before * Purple state | | | -0.048* | | -0.045* | | | -0.003 | | 0.002 |
| | | | (0.026) | | (0.027) | | | (0.022) | | (0.023) |
| **Age ranges** | | | | | | | | | | |
| Age 40-59 | | | | -0.028 | -0.023 | | | | -0.031 | -0.030 |
| | | | | (0.025) | (0.025) | | | | (0.021) | (0.021) |
| Before * Age 40-59 | | | | 0.039 | 0.031 | | | | 0.028 | 0.027 |
| | | | | (0.035) | (0.035) | | | | (0.030) | (0.030) |
| Age 60-79 | | | | -0.019 | -0.019 | | | | -0.044** | -0.053*** |
| | | | | (0.023) | (0.024) | | | | (0.020) | (0.021) |
| Before * Age 60-79 | | | | 0.041 | 0.030 | | | | 0.032 | 0.034 |
| | | | | (0.033) | (0.034) | | | | (0.028) | (0.029) |
| Control mean | 0.365 | 0.369 | 0.342 | 0.365 | 0.358 | 0.196 | 0.196 | 0.194 | 0.212 | 0.187 |
| N | 7981 | 7981 | 7981 | 7981 | 7981 | 7981 | 7981 | 7981 | 7981 | 7981 |

Table A9: Wave 2: Heterogeneity by Wave-1 recipient traits

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | Opted into email process | | | | | Has an email record | | | | |
| Treatment | 0.035** | 0.045*** | 0.055*** | 0.040* | 0.010 | 0.020 | 0.039*** | 0.026* | 0.015 | -0.024 |
| | (0.015) | (0.016) | (0.018) | (0.023) | (0.030) | (0.013) | (0.013) | (0.015) | (0.017) | (0.023) |
| **Gender** | | | | | | | | | | |
| Woman | -0.039*** | | | | -0.039*** | 0.003 | | | | 0.004 |
| | (0.015) | | | | (0.015) | (0.012) | | | | (0.012) |
| Treatment * Woman | 0.040* | | | | 0.039* | 0.042** | | | | 0.040** |
| | (0.021) | | | | (0.021) | (0.017) | | | | (0.017) |
| **Education** | | | | | | | | | | |
| Has 4-year college degree | | -0.025 | | | -0.024 | | -0.017 | | | -0.015 |
| | | (0.026) | | | (0.026) | | (0.023) | | | (0.023) |
| Treatment * 4-year college | | 0.020 | | | 0.017 | | 0.008 | | | 0.004 |
| | | (0.021) | | | (0.021) | | (0.017) | | | (0.018) |
| **State marginality groups** | | | | | | | | | | |
| Blue state | | | 0.012 | | 0.014 | | | -0.003 | | -0.001 |
| | | | (0.032) | | (0.032) | | | (0.027) | | (0.027) |
| Before * Blue state | | | 0.012 | | 0.008 | | | 0.021 | | 0.019 |
| | | | (0.025) | | (0.025) | | | (0.021) | | (0.021) |
| Purple state | | | 0.001 | | 0.003 | | | -0.013 | | -0.010 |
| | | | (0.032) | | (0.032) | | | (0.027) | | (0.027) |
| Before * Purple state | | | -0.006 | | -0.009 | | | 0.032 | | 0.030 |
| | | | (0.025) | | (0.025) | | | (0.021) | | (0.021) |
| **Age ranges** | | | | | | | | | | |
| Age 40-59 | | | | -0.009 | -0.006 | | | | 0.019 | 0.021 |
| | | | | (0.034) | (0.034) | | | | (0.026) | (0.026) |
| Before * Age 40-59 | | | | -0.000 | -0.003 | | | | 0.009 | 0.009 |
| | | | | (0.031) | (0.031) | | | | (0.025) | (0.025) |
| Age 60-79 | | | | -0.047 | -0.044 | | | | 0.017 | 0.020 |
| | | | | (0.031) | (0.031) | | | | (0.024) | (0.024) |
| Before * Age 60-79 | | | | 0.033 | 0.028 | | | | 0.055** | 0.052** |
| | | | | (0.029) | (0.029) | | | | (0.024) | (0.024) |
| Control mean | 0.370 | 0.299 | 0.331 | 0.347 | 0.358 | 0.176 | 0.141 | 0.175 | 0.146 | 0.187 |
| N | 7981 | 7981 | 7981 | 7981 | 7981 | 7981 | 7981 | 7981 | 7981 | 7981 |

### A.2.2 Wave 1

Table A10: Sample balance in full randomized sample, separating by paired Wave-2 party

| | Control | | Tell after | | Tell before | |
|---|---|---|---|---|---|---|
| | Mean: Dem | Δ Rep | Δ Dem | Δ Rep | Δ Dem | Δ Rep |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Woman | 0.637 | -0.010 | -0.016 | 0.018 | -0.009 | 0.023 |
| | | (0.016) | (0.017) | (0.017) | (0.017) | (0.017) |
| Hispanic | 1.025 | 0.003 | 0.005 | -0.003 | -0.002 | 0.006 |
| | | (0.005) | (0.006) | (0.005) | (0.006) | (0.006) |
| Has $\geq$ 4-year college degree | 0.827 | 0.001 | 0.018 | -0.032** | 0.006 | -0.004 |
| | | (0.013) | (0.013) | (0.014) | (0.014) | (0.014) |
| **Age ranges:** | | | | | | |
| 20-39 | 0.113 | 0.014 | -0.014 | -0.011 | 0.005 | 0.008 |
| | | (0.011) | (0.011) | (0.011) | (0.012) | (0.011) |
| 40-59 | 0.344 | -0.015 | 0.007 | 0.010 | -0.009 | -0.008 |
| | | (0.016) | (0.017) | (0.017) | (0.017) | (0.017) |
| 60-79 | 0.544 | 0.001 | 0.006 | 0.001 | 0.004 | 0.000 |
| | | (0.017) | (0.018) | (0.018) | (0.018) | (0.018) |
| **Income bins (USD):** | | | | | | |
| Less than 50,000 | 0.164 | 0.014 | -0.000 | 0.006 | 0.010 | -0.009 |
| | | (0.013) | (0.013) | (0.013) | (0.014) | (0.013) |
| 50,000-99,999 | 0.333 | -0.015 | -0.016 | -0.016 | -0.017 | -0.013 |
| | | (0.016) | (0.017) | (0.017) | (0.017) | (0.017) |
| 100,000-149,999 | 0.228 | 0.004 | 0.002 | -0.005 | 0.027* | 0.007 |
| | | (0.014) | (0.015) | (0.015) | (0.016) | (0.015) |
| 150,000-199,999 | 0.123 | 0.007 | 0.010 | 0.011 | -0.002 | 0.022* |
| | | (0.011) | (0.012) | (0.012) | (0.012) | (0.012) |
| 200,000 or more | 0.152 | -0.010 | 0.005 | 0.004 | -0.018 | -0.007 |
| | | (0.012) | (0.013) | (0.013) | (0.013) | (0.013) |
| **Residence by state group:** | | | | | | |
| Red state | 0.216 | 0.030** | 0.037** | 0.027* | 0.025* | 0.007 |
| | | (0.014) | (0.015) | (0.015) | (0.015) | (0.015) |
| Blue state | 0.446 | -0.017 | -0.019 | -0.007 | 0.001 | 0.014 |
| | | (0.017) | (0.018) | (0.018) | (0.018) | (0.018) |
| Purple state | 0.338 | -0.013 | -0.017 | -0.020 | -0.026 | -0.022 |
| | | (0.016) | (0.017) | (0.017) | (0.017) | (0.017) |
| Climate worry (1-7) | 6.395 | 0.072*** | 0.020 | 0.026 | 0.028 | 0.011 |
| | | (0.026) | (0.029) | (0.029) | (0.029) | (0.030) |
| Desire for climate action (1-7) | 6.722 | 0.026 | 0.009 | 0.015 | 0.004 | 0.021 |
| | | (0.019) | (0.021) | (0.021) | (0.022) | (0.020) |
| Perceived local impacts (1-7) | 5.515 | -0.039 | 0.037 | -0.025 | -0.007 | -0.060* |
| | | (0.033) | (0.035) | (0.035) | (0.036) | (0.036) |
| **Political engagement and beliefs:** | | | | | | |
| Political engage. index (std) | -0.021 | -0.017 | 0.073 | -0.050 | -0.051 | 0.073 |
| | | (0.133) | (0.145) | (0.146) | (0.148) | (0.141) |
| Prev. contacted reps | 0.743 | -0.022 | -0.015 | -0.025 | -0.018 | 0.004 |
| | | (0.015) | (0.016) | (0.016) | (0.016) | (0.016) |
| Prev. donated | 0.801 | 0.029** | 0.023 | 0.020 | 0.006 | 0.029** |
| | | (0.013) | (0.014) | (0.014) | (0.014) | (0.014) |
| Prev. canvassed | 0.079 | 0.006 | 0.006 | -0.002 | 0.003 | -0.000 |
| | | (0.009) | (0.010) | (0.010) | (0.010) | (0.010) |
| Prev. signed petition | 0.833 | 0.003 | -0.005 | -0.012 | -0.020 | -0.005 |
| | | (0.012) | (0.013) | (0.014) | (0.014) | (0.013) |
| Prev. phonebanked | 0.097 | 0.007 | 0.028** | 0.027** | 0.025** | 0.008 |
| | | (0.010) | (0.011) | (0.011) | (0.011) | (0.011) |
| Political efficacy index (std) | 0.004 | -0.000 | 0.014 | -0.021 | 0.001 | -0.024 |
| | | (0.019) | (0.020) | (0.020) | (0.021) | (0.020) |
| Degree prefer Dem friends (1-7) | 6.021 | 0.039 | 0.005 | 0.031 | -0.030 | 0.053 |
| | | (0.032) | (0.035) | (0.035) | (0.035) | (0.035) |
| Sample size | 1847 | 1769 | 1342 | 1304 | 1305 | 1370 |

Table A11: Wave 1 Attrition results

| *Panel A: Differential attrition* | | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | | Restricted to A1 and A2 samples | |
| | Observe if emailed | Answered demand effects | Answered others' participation |
| Tell | -0.115*** | | |
| | (0.007) | | |
| Tell*before | 0.005 | 0.004 | 0.012 |
| | (0.009) | (0.010) | (0.011) |
| Baseline mean | 0.992 | 0.830 | 0.820 |
| N | 8937 | 5321 | 5321 |

| *Panel B: Characteristics of those with and without email choices* | | | |
|---|---|---|---|
| | Observe if emailed Congress? | | (1) = (2) |
| | (1) Yes | (2) No | *p-value* |
| Woman | 0.645 | 0.551 | (0.000) |
| Hispanic | 1.027 | 1.021 | (0.317) |
| **Age decades:** | | | |
| 20-29 | 0.033 | 0.024 | (0.134) |
| 30-39 | 0.085 | 0.033 | (0.000) |
| 40-49 | 0.113 | 0.079 | (0.003) |
| 50-59 | 0.228 | 0.263 | (0.052) |
| 60-69 | 0.341 | 0.346 | (0.792) |
| 70-79 | 0.200 | 0.254 | (0.001) |
| Has $\geq$ 4-year college degree | 0.828 | 0.798 | (0.061) |
| **Income bins (USD):** | | | |
| Less than 50,000 | 0.167 | 0.169 | (0.894) |
| 50,000-99,999 | 0.321 | 0.317 | (0.875) |
| 100,000-149,999 | 0.234 | 0.231 | (0.814) |
| 150,000-199,999 | 0.130 | 0.136 | (0.668) |
| 200,000 or more | 0.148 | 0.147 | (0.943) |
| **Residence by state marginality:** | | | |
| Blue state | 0.237 | 0.223 | (0.410) |
| Red state | 0.444 | 0.410 | (0.089) |
| Purple state | 0.319 | 0.367 | (0.012) |
| **Climate beliefs:** | | | |
| Climate worry (std) | -0.014 | 0.002 | (0.721) |
| Desire for climate action (std) | -0.009 | -0.021 | (0.745) |
| Perceived local impacts (std) | -0.018 | 0.043 | (0.137) |
| **Political engagement and beliefs:** | | | |
| Political engage. index (std) | 0.022 | -0.493 | (0.001) |
| Prev. contacted reps | 0.733 | 0.704 | (0.096) |
| Prev. donated | 0.820 | 0.801 | (0.235) |
| Prev. canvassed | 0.082 | 0.060 | (0.028) |
| Prev. signed petition | 0.830 | 0.799 | (0.053) |
| Prev. phonebanked | 0.113 | 0.088 | (0.037) |
| Political efficacy index (std) | -0.001 | 0.009 | (0.647) |
| Degree prefer Dem friends (std) | -0.015 | 0.010 | (0.542) |
| Sample size | 8269 | 668 | |

Table A12: Baseline predictors of contacting Congress in the Wave-1 and Wave-2 control groups

| | Whether email Congress | | | |
|---|---|---|---|---|
| | Wave 1 | | Wave 2 | |
| Woman | 0.044*** | (0.017) | 0.018** | (0.009) |
| Hispanic | -0.009 | (0.048) | -0.009 | (0.017) |
| Has $\geq$ 4-year college degree | 0.036* | (0.021) | 0.005 | (0.011) |
| **Age decades:** | | | | |
| 30-39 | -0.028 | (0.049) | 0.043** | (0.017) |
| 40-49 | 0.043 | (0.048) | 0.011 | (0.017) |
| 50-59 | -0.021 | (0.045) | 0.003 | (0.015) |
| 60-69 | -0.009 | (0.044) | 0.021 | (0.016) |
| 70-79 | -0.033 | (0.045) | 0.019 | (0.018) |
| **Income bins (USD):** | | | | |
| 50,000-99,999 | -0.025 | (0.023) | -0.005 | (0.011) |
| 100,000-149,999 | 0.020 | (0.026) | -0.021 | (0.013) |
| 150,000-199,999 | -0.003 | (0.030) | -0.021 | (0.017) |
| 200,000 or more | -0.004 | (0.029) | -0.030** | (0.015) |
| **Residence by state marginality:** | | | | |
| Blue state | 0.011 | (0.020) | -0.003 | (0.010) |
| Purple state | 0.018 | (0.021) | -0.007 | (0.011) |
| **Climate beliefs:** | | | | |
| Climate worry (std) | 0.022** | (0.009) | 0.009 | (0.006) |
| Desire for climate action (std) | 0.014* | (0.008) | 0.003 | (0.006) |
| Perceived local impacts (std) | 0.008 | (0.008) | 0.016*** | (0.005) |
| **Political affiliation:** | | | | |
| R-leaning Independent | | | 0.004 | (0.016) |
| L-leaning Independent | | | 0.048*** | (0.018) |
| Democrat | | | -0.014 | (0.018) |
| **Political engagement and beliefs:** | | | | |
| Prev. contacted reps | 0.098*** | (0.018) | 0.014 | (0.012) |
| Prev. donated | 0.052*** | (0.019) | 0.023** | (0.011) |
| Prev. canvassed | -0.020 | (0.031) | -0.004 | (0.022) |
| Prev. signed petition | 0.037* | (0.021) | 0.030*** | (0.010) |
| Prev. phonebanked | 0.026 | (0.030) | -0.015 | (0.019) |
| Political efficacy index (std) | -0.046*** | (0.013) | -0.030*** | (0.009) |
| Control mean | 0.312 | | 0.081 | |
| Sample size | 3588 | | 3543 | |

Table A13: Influence belief correlations

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | **Zero** | | **Negative** | | | **Positive** | | |
| | Mean | N | Δ Mean | N | *p-value* | Δ Mean | N | *p-value* |
| Numeric influence beliefs | 4.295 | 1994 | -0.706 | 455 | (0.288) | 2.534*** | 723 | (0.000) |
| | | | | | | | | |
| Woman | 0.640 | 5393 | -0.026* | 1205 | (0.083) | 0.005 | 2339 | (0.677) |
| Hispanic | 1.026 | 5393 | 0.007 | 1205 | (0.243) | -0.001 | 2339 | (0.803) |
| Has $\geq$ 4-year college degree | 0.825 | 5393 | -0.029** | 1205 | (0.026) | 0.019** | 2339 | (0.035) |
| | | | | | | | | |
| **Age ranges:** | | | | | | | | |
| 20-39 | 0.111 | 5393 | 0.006 | 1205 | (0.549) | 0.007 | 2339 | (0.382) |
| 40-59 | 0.343 | 5393 | -0.010 | 1205 | (0.505) | -0.004 | 2339 | (0.739) |
| 60-79 | 0.546 | 5393 | 0.003 | 1205 | (0.851) | -0.003 | 2339 | (0.803) |
| | | | | | | | | |
| **Income bins (USD):** | | | | | | | | |
| Less than 50,000 | 0.165 | 5393 | 0.013 | 1205 | (0.279) | 0.001 | 2339 | (0.912) |
| 50,000-99,999 | 0.315 | 5393 | 0.025* | 1205 | (0.096) | 0.008 | 2339 | (0.505) |
| 100,000-149,999 | 0.243 | 5393 | -0.033** | 1205 | (0.011) | -0.018* | 2339 | (0.072) |
| 150,000-199,999 | 0.129 | 5393 | -0.006 | 1205 | (0.585) | 0.009 | 2339 | (0.261) |
| 200,000 or more | 0.148 | 5393 | 0.001 | 1205 | (0.928) | -0.000 | 2339 | (1.000) |
| | | | | | | | | |
| **Residence by state group:** | | | | | | | | |
| Red state | 0.239 | 5393 | 0.028** | 1205 | (0.046) | -0.024** | 2339 | (0.016) |
| Blue state | 0.431 | 5393 | -0.024 | 1205 | (0.134) | 0.053*** | 2339 | (0.000) |
| Purple state | 0.330 | 5393 | -0.003 | 1205 | (0.841) | -0.029*** | 2339 | (0.008) |
| Climate worry (1-7) | 6.401 | 5393 | -0.095*** | 1205 | (0.001) | 0.127*** | 2339 | (0.000) |
| Desire for climate action (1-7) | 6.729 | 5393 | -0.058*** | 1205 | (0.004) | 0.052*** | 2339 | (0.000) |
| Perceived local impacts (1-7) | 5.465 | 5393 | 0.035 | 1205 | (0.289) | 0.112*** | 2339 | (0.000) |
| | | | | | | | | |
| **Political engagement and beliefs:** | | | | | | | | |
| Political engage. index (std) | -0.113 | 5393 | -0.049 | 1205 | (0.114) | 0.456*** | 2339 | (0.000) |
| Prev. contacted reps | 0.704 | 5393 | -0.028* | 1205 | (0.062) | 0.120*** | 2339 | (0.000) |
| Prev. donated | 0.805 | 5393 | -0.013 | 1205 | (0.317) | 0.059*** | 2339 | (0.000) |
| Prev. canvassed | 0.057 | 5393 | 0.002 | 1205 | (0.803) | 0.088*** | 2339 | (0.000) |
| Prev. signed petition | 0.810 | 5393 | -0.009 | 1205 | (0.489) | 0.072*** | 2339 | (0.000) |
| Prev. phonebanked | 0.083 | 5393 | 0.005 | 1205 | (0.579) | 0.107*** | 2339 | (0.000) |
| Political efficacy index (std) | 0.047 | 5393 | 0.122*** | 1205 | (0.000) | -0.242*** | 2339 | (0.000) |
| Degree prefer Dem friends (1-7) | 6.047 | 5393 | -0.043 | 1205 | (0.179) | -0.014 | 2339 | (0.560) |

Table A14: Wave-1: Heterogeneity on **starting the email process** by match similarity

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Opted into email process | | | | | | | |
| Tell | 0.110*** | 0.102*** | 0.147*** | 0.119*** | 0.128*** | 0.116*** | 0.110*** | 0.122*** |
| | (0.018) | (0.017) | (0.016) | (0.013) | (0.015) | (0.025) | (0.018) | (0.025) |
| Tell before | 0.017 | 0.044** | 0.016 | 0.039*** | 0.020 | 0.005 | 0.017 | 0.004 |
| | (0.019) | (0.018) | (0.017) | (0.014) | (0.016) | (0.026) | (0.019) | (0.026) |
| **Politics** | | | | | | | | |
| Same party | -0.015 | | | | | -0.016 | -0.015 | -0.015 |
| | (0.016) | | | | | (0.016) | (0.016) | (0.016) |
| Tell * Same party | 0.023 | | | | | 0.023 | 0.021 | 0.022 |
| | (0.025) | | | | | (0.025) | (0.025) | (0.025) |
| Tell before * Same party | 0.033 | | | | | 0.034 | 0.034 | 0.034 |
| | (0.026) | | | | | (0.026) | (0.026) | (0.026) |
| **Gender** | | | | | | | | |
| Same gender | | -0.030* | | | | -0.037** | | |
| | | (0.017) | | | | (0.018) | | |
| Tell * Same gender | | 0.040 | | | | 0.051* | | |
| | | (0.025) | | | | (0.026) | | |
| Tell before * Same gender | | -0.022 | | | | -0.019 | | |
| | | (0.026) | | | | (0.027) | | |
| **Education** | | | | | | | | |
| Same college attainment | | | 0.018 | | | 0.028 | | |
| | | | (0.017) | | | (0.018) | | |
| Tell * Same college | | | -0.061** | | | -0.069*** | | |
| | | | (0.025) | | | (0.026) | | |
| Tell before * Same college | | | 0.040 | | | 0.034 | | |
| | | | (0.026) | | | (0.027) | | |
| **State** | | | | | | | | |
| Same state group | | | | -0.017 | | -0.008 | | |
| | | | | (0.030) | | (0.030) | | |
| Tell * Same state | | | | 0.028 | | 0.006 | | |
| | | | | (0.045) | | (0.045) | | |
| Tell before * Same state | | | | -0.075 | | -0.064 | | |
| | | | | (0.048) | | (0.049) | | |
| **Age range** | | | | | | | | |
| Same age range | | | | | -0.008 | -0.018 | | |
| | | | | | (0.018) | (0.019) | | |
| Tell * Same age | | | | | -0.024 | -0.007 | | |
| | | | | | (0.027) | (0.028) | | |
| Tell before * Same age | | | | | 0.046 | 0.040 | | |
| | | | | | (0.029) | (0.029) | | |
| **Overall non-political similarity** | | | | | | | | |
| Pred. non-pol sim | | | | | | | -0.005 | |
| | | | | | | | (0.008) | |
| Tell * Pred. non-pol sim | | | | | | | -0.007 | |
| | | | | | | | (0.012) | |
| Tell before * Pred. non-pol sim | | | | | | | 0.008 | |
| | | | | | | | (0.013) | |
| # Non-pol matched traits | | | | | | | | -0.007 |
| | | | | | | | | (0.009) |
| Tell * # Non-pol matches | | | | | | | | -0.009 |
| | | | | | | | | (0.014) |
| Tell before * # Non-pol matches | | | | | | | | 0.010 |
| | | | | | | | | (0.014) |
| Control mean | 0.470 | 0.472 | 0.453 | 0.461 | 0.463 | 0.461 | 0.470 | 0.470 |
| N | 8937 | 8937 | 8937 | 8937 | 8937 | 8937 | 8936 | 8937 |

Table A15: Wave-1: Heterogeneity on **email records** by match similarity

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Has an email record | | | | | | | |
| Tell | 0.125*** | 0.119*** | 0.150*** | 0.130*** | 0.140*** | 0.126*** | 0.125*** | 0.129*** |
| | (0.017) | (0.017) | (0.016) | (0.013) | (0.015) | (0.025) | (0.017) | (0.025) |
| Tell before | 0.020 | 0.028 | 0.016 | 0.035** | 0.022 | 0.013 | 0.020 | 0.015 |
| | (0.019) | (0.019) | (0.018) | (0.014) | (0.016) | (0.027) | (0.019) | (0.027) |
| **Politics** | | | | | | | | |
| Same party | -0.008 | | | | | -0.008 | -0.008 | -0.008 |
| | (0.015) | | | | | (0.015) | (0.015) | (0.015) |
| Tell * Same party | 0.017 | | | | | 0.017 | 0.016 | 0.016 |
| | (0.024) | | | | | (0.024) | (0.024) | (0.024) |
| Tell before * Same party | 0.016 | | | | | 0.016 | 0.017 | 0.017 |
| | (0.027) | | | | | (0.027) | (0.027) | (0.027) |
| **Gender** | | | | | | | | |
| Same gender | | -0.034** | | | | -0.037** | | |
| | | (0.016) | | | | (0.017) | | |
| Tell * Same gender | | 0.031 | | | | 0.035 | | |
| | | (0.024) | | | | (0.025) | | |
| Tell before * Same gender | | -0.002 | | | | 0.002 | | |
| | | (0.027) | | | | (0.028) | | |
| **Education** | | | | | | | | |
| Same college attainment | | | 0.007 | | | 0.017 | | |
| | | | (0.016) | | | (0.017) | | |
| Tell * Same college | | | -0.040 | | | -0.043* | | |
| | | | (0.025) | | | (0.026) | | |
| Tell before * Same college | | | 0.027 | | | 0.018 | | |
| | | | (0.027) | | | (0.028) | | |
| **State** | | | | | | | | |
| Same state group | | | | -0.030 | | -0.024 | | |
| | | | | (0.028) | | (0.028) | | |
| Tell * Same state | | | | 0.048 | | 0.036 | | |
| | | | | (0.045) | | (0.045) | | |
| Tell before * Same state | | | | -0.093* | | -0.089* | | |
| | | | | (0.049) | | (0.050) | | |
| **Age range** | | | | | | | | |
| Same age range | | | | | -0.008 | -0.017 | | |
| | | | | | (0.017) | (0.017) | | |
| Tell * Same age | | | | | -0.021 | -0.010 | | |
| | | | | | (0.027) | (0.027) | | |
| Tell before * Same age | | | | | 0.019 | 0.019 | | |
| | | | | | (0.029) | (0.030) | | |
| **Overall non-political similarity** | | | | | | | | |
| Pred. non-pol sim | | | | | | | -0.010 | |
| | | | | | | | (0.008) | |
| Tell * Pred. non-pol sim | | | | | | | 0.001 | |
| | | | | | | | (0.012) | |
| Tell before * Pred. non-pol sim | | | | | | | -0.000 | |
| | | | | | | | (0.013) | |
| # Non-pol matched traits | | | | | | | | -0.012 |
| | | | | | | | | (0.008) |
| Tell * # Non-pol matches | | | | | | | | -0.003 |
| | | | | | | | | (0.013) |
| Tell before * # Non-pol matches | | | | | | | | 0.004 |
| | | | | | | | | (0.015) |
| Control mean | 0.315 | 0.320 | 0.305 | 0.310 | 0.309 | 0.310 | 0.315 | 0.315 |
| N | 8937 | 8937 | 8937 | 8937 | 8937 | 8937 | 8936 | 8937 |

Table A16: Wave-1: Heterogeneity by target traits

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | Opted into email process | | | | | Has an email record | | | | |
| Tell | 0.133*** | 0.097*** | 0.146*** | 0.125*** | 0.137*** | 0.142*** | 0.122*** | 0.146*** | 0.122*** | 0.131*** |
| | (0.017) | (0.017) | (0.016) | (0.018) | (0.027) | (0.017) | (0.017) | (0.016) | (0.018) | (0.027) |
| Tell before | 0.050*** | 0.046** | 0.018 | 0.028 | 0.042 | 0.035* | 0.030 | 0.020 | 0.044** | 0.049 |
| | (0.018) | (0.018) | (0.017) | (0.019) | (0.029) | (0.019) | (0.019) | (0.018) | (0.020) | (0.030) |
| **Politics** | | | | | | | | | | |
| Republican | 0.015 | | | | 0.015 | 0.008 | | | | 0.008 |
| | (0.016) | | | | (0.016) | (0.015) | | | | (0.015) |
| Tell * Republican | -0.023 | | | | -0.024 | -0.017 | | | | -0.019 |
| | (0.026) | | | | (0.026) | (0.027) | | | | (0.027) |
| Tell before * Republican | -0.033 | | | | -0.032 | -0.016 | | | | -0.014 |
| | (0.026) | | | | (0.026) | | | | | |
| **Gender** | | | | | | | | | | |
| Woman | | -0.020 | | | -0.020 | | -0.007 | | | -0.007 |
| | | (0.017) | | | (0.017) | | (0.016) | | | (0.016) |
| Tell * Woman | | 0.050** | | | 0.054** | | 0.024 | | | 0.028 |
| | | (0.026) | | | (0.026) | | (0.027) | | | (0.027) |
| Tell before * Woman | | -0.027 | | | -0.029 | | -0.006 | | | -0.009 |
| | | (0.026) | | | (0.026) | | (0.027) | | | (0.027) |
| **Education** | | | | | | | | | | |
| Has 4-year college degree | | | 0.013 | | 0.014 | | | 0.003 | | 0.002 |
| | | | (0.017) | | (0.017) | | | (0.016) | | (0.016) |
| Tell * 4-year college | | | -0.058** | | -0.063** | | | -0.029 | | -0.031 |
| | | | (0.026) | | (0.027) | | | (0.027) | | (0.027) |
| Tell before * 4-year college | | | 0.035 | | 0.038 | | | 0.017 | | 0.017 |
| | | | (0.026) | | (0.027) | | | (0.027) | | (0.027) |
| **State marginality groups** | | | | | | | | | | |
| Blue state | | | | 0.017 | 0.016 | | | | -0.001 | -0.002 |
| | | | | (0.020) | (0.020) | | | | (0.018) | (0.018) |
| Tell * Blue state | | | | -0.014 | -0.014 | | | | 0.013 | 0.013 |
| | | | | (0.031) | (0.031) | | | | (0.030) | (0.030) |
| Tell before * Blue state | | | | 0.005 | 0.006 | | | | 0.032 | 0.034 |
| | | | | (0.031) | (0.031) | | | | (0.030) | (0.030) |
| Purple state | | | | 0.010 | 0.009 | | | | 0.013 | 0.012 |
| | | | | (0.021) | (0.021) | | | | (0.020) | (0.020) |
| Tell * Purple state | | | | 0.003 | 0.002 | | | | -0.026 | -0.027 |
| | | | | (0.033) | (0.033) | | | | (0.033) | (0.034) |
| Tell before * Purple state | | | | 0.018 | 0.017 | | | | -0.038 | -0.038 |
| | | | | (0.033) | (0.033) | | | | (0.033) | (0.034) |
| Control mean | 0.452 | 0.473 | 0.455 | 0.460 | 0.461 | 0.304 | 0.316 | 0.308 | 0.313 | 0.310 |
| N | 8937 | 8936 | 8936 | 8936 | 8936 | 8937 | 8936 | 8936 | 8936 | 8936 |

Table A17: Wave-1: Heterogeneity by influencer traits

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Opted into email process | | | | Has an email record | | | |
| Tell | 0.095*** | 0.133*** | 0.396*** | 0.092*** | 0.116*** | 0.134*** | 0.447*** | 0.127*** |
| | (0.021) | (0.019) | (0.033) | (0.034) | (0.020) | (0.018) | (0.032) | (0.032) |
| Tell before | 0.053** | 0.004 | 0.023 | 0.091** | 0.038* | 0.015 | 0.022 | 0.062* |
| | (0.022) | (0.020) | (0.043) | (0.036) | (0.022) | (0.020) | (0.044) | (0.036) |
| **Gender** | | | | | | | | |
| Woman | 0.031* | | | | 0.034** | | | |
| | (0.018) | | | | (0.016) | | | |
| Tell * Woman | 0.041 | | | | 0.028 | | | |
| | (0.027) | | | | (0.028) | | | |
| Tell before * Woman | -0.031 | | | | -0.018 | | | |
| | (0.027) | | | | (0.028) | | | |
| **Worry about climate change** | | | | | | | | |
| $\geq$ Median | | 0.041* | | | | 0.013 | | |
| | | (0.023) | | | | (0.021) | | |
| Tell * $\geq$ Median | | -0.020 | | | | -0.000 | | |
| | | (0.027) | | | | (0.027) | | |
| Tell before * $\geq$ Median | | 0.049* | | | | 0.020 | | |
| | | (0.027) | | | | (0.027) | | |
| **End-of-survey influence beliefs** | | | | | | | | |
| $\geq$ Median | | | 0.010 | | | | 0.015 | |
| | | | (0.018) | | | | (0.010) | |
| Tell * $\geq$ Median | | | 0.078* | | | | 0.084** | |
| | | | (0.054) | | | | (0.057) | |
| Tell before * $\geq$ Median | | | 0.031 | | | | 0.011 | |
| | | | (0.054) | | | | (0.057) | |
| **Qualitative influence beliefs** | | | | | | | | |
| Zero | | | | -0.014 | | | | 0.010 |
| | | | | (0.025) | | | | (0.022) |
| Tell * Zero | | | | 0.030 | | | | -0.007 |
| | | | | (0.040) | | | | (0.040) |
| Tell before * Zero | | | | -0.053 | | | | -0.020 |
| | | | | (0.040) | | | | (0.040) |
| Positive | | | | 0.083*** | | | | 0.084*** |
| | | | | (0.028) | | | | (0.026) |
| Tell * Positive | | | | 0.045 | | | | 0.042 |
| | | | | (0.043) | | | | (0.045) |
| Tell before * Positive | | | | -0.101** | | | | -0.089** |
| | | | | (0.043) | | | | (0.045) |
| Control mean | 0.425 | 0.406 | 0.180 | 0.428 | 0.274 | 0.263 | 0.032 | 0.263 |
| N | 8937 | 8937 | 3172 | 8937 | 8937 | 8937 | 3172 | 8937 |

Table A18: Correlation of affective polarization with everything else

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Prefer friends Dem (std.) | | | |
| | Wave 1 | | WTP | |
| | Bivariate | Joint | Bivariate | Joint |
| Thermometer polarization (std) | 0.295*** | | | |
| | (0.015) | | | |
| **Basic demographics:** | | | | |
| Woman | 0.171*** | 0.097*** | 0.110 | 0.065 |
| | (0.022) | (0.023) | (0.070) | (0.070) |
| Hispanic | 0.131* | 0.092 | 0.300** | 0.288* |
| | (0.067) | (0.065) | (0.134) | (0.148) |
| Has $\geq$ 4-year college degree | 0.013 | -0.016 | 0.087 | 0.025 |
| | (0.029) | (0.030) | (0.091) | (0.090) |
| **Age:** | | | | |
| Age (5-year ints) | -0.030*** | | -0.035*** | |
| | (0.004) | | (0.010) | |
| 40-59 | | -0.204*** | | -0.170** |
| | | (0.032) | | (0.084) |
| 60-79 | | -0.256*** | | -0.259*** |
| | | (0.031) | | (0.083) |
| **Income bins (USD):** | | | | |
| Income (USD) categories | -0.014** | | 0.015 | |
| | (0.006) | | (0.017) | |
| 50,000-99,999 | | -0.059* | | 0.025 |
| | | (0.032) | | (0.092) |
| 100,000-149,999 | | -0.055 | | 0.088 |
| | | (0.034) | | (0.099) |
| 150,000-199,999 | | -0.080** | | 0.075 |
| | | (0.040) | | (0.113) |
| 200,000 or more | | -0.097** | | -0.006 |
| | | (0.039) | | (0.113) |
| **Residence by state group:** | | | | |
| Red state | -0.001 | | 0.016 | |
| | (0.025) | | (0.071) | |
| Blue state | 0.028 | 0.009 | 0.066 | 0.005 |
| | (0.021) | (0.027) | (0.059) | (0.077) |
| Purple state | -0.031 | -0.025 | -0.088 | -0.091 |
| | (0.023) | (0.029) | (0.063) | (0.081) |
| **Climate beliefs:** | | | | |
| Climate worry (std) | 0.148*** | 0.073*** | 0.141*** | 0.118*** |
| | (0.011) | (0.013) | (0.033) | (0.035) |
| Desire for climate action (std) | 0.174*** | 0.117*** | 0.109** | 0.045 |
| | (0.012) | (0.013) | (0.046) | (0.040) |
| Perceived local impacts (std) | 0.045*** | 0.008 | 0.014 | -0.014 |
| | (0.011) | (0.011) | (0.029) | (0.029) |
| **Political engagement and beliefs:** | | | | |
| Political engage. index (std) | 0.023*** | 0.017*** | 0.015* | 0.013 |
| | (0.003) | (0.003) | (0.008) | (0.008) |
| Political efficacy index (std) | -0.004 | 0.015 | -0.043 | -0.046 |
| | (0.020) | (0.019) | (0.056) | (0.057) |
| Sample size | 8937 | 8937 | 1023 | 1023 |

Table A19: Wave-1 impacts on secondary outcomes

| | (1) Demand effects (std) | (2) Share Wave-1 participating | (3) Influence beliefs | (4) Therm. affective polarization (std) |
|---|---|---|---|---|
| Tell | 0.313*** | 10.782*** | | |
| | (0.031) | (0.662) | | |
| Tell before | 0.059** | -0.639 | 1.201 | -0.012 |
| | (0.028) | (0.644) | (0.795) | (0.030) |
| A0 mean | 0.000 | 29.766 | | |
| A1 mean | | | 5.307 | -0.000 |
| N | 6163 | 6071 | 1222 | 4321 |

### A.2.3 WTP

Table A20: WTP attrition

| | (1) | (2) | (3) |
|---|---|---|---|
| | **Finish 20 WTP choices** | | |
| | *Round 1* | | |
| | Full sample | Basic sample | *Round 2* |
| Has money option | -0.083** | | |
| | (0.038) | | |
| Has info | | -0.006 | |
| | | (0.016) | |
| Can choose politics | | | -0.023 |
| | | | (0.023) |
| Control means | 0.758 | 0.926 | 0.790 |
| N | 1516 | 1109 | 1278 |

Table A21: Summary statistics and balance in the WTP sample

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | | | **Balance within WTP sample** | | |
| | **Comparing samples** | | Has belief info | No belief info | |
| | Wave-1 | WTP | Mean | Δ Mean | *p-value* |
| Woman | 0.632 | 0.761 | 0.770 | -0.021 | (0.437) |
| Hispanic | 1.027 | 1.039 | 1.044 | -0.010 | (0.405) |
| Has $\geq$ 4-year college degree | 0.828 | 0.862 | 0.874 | -0.026 | (0.238) |
| **Age ranges:** | | | | | |
| 20-39 | 0.119 | 0.146 | 0.162 | -0.036 | (0.102) |
| 40-59 | 0.337 | 0.354 | 0.358 | -0.008 | (0.790) |
| 60-79 | 0.544 | 0.500 | 0.480 | 0.044 | (0.156) |
| **Income bins (USD):** | | | | | |
| Less than 50,000 | 0.170 | 0.166 | 0.177 | -0.023 | (0.318) |
| 50,000-99,999 | 0.325 | 0.338 | 0.341 | -0.007 | (0.816) |
| 100,000-149,999 | 0.231 | 0.238 | 0.226 | 0.024 | (0.374) |
| 150,000-199,999 | 0.126 | 0.118 | 0.113 | 0.011 | (0.582) |
| 200,000 or more | 0.147 | 0.140 | 0.142 | -0.005 | (0.820) |
| **State marginality:** | | | | | |
| Red state | 0.231 | 0.220 | 0.217 | 0.006 | (0.818) |
| Blue state | 0.438 | 0.457 | 0.484 | -0.058* | (0.062) |
| Purple state | 0.331 | 0.324 | 0.299 | 0.052* | (0.073) |
| **Climate beliefs:** | | | | | |
| Climate worry (1-7) | 6.430 | 6.547 | 6.560 | -0.028 | (0.525) |
| Desire for climate action (1-7) | 6.735 | 6.823 | 6.819 | 0.008 | (0.775) |
| Perceived local impacts (1-7) | 5.496 | 5.576 | 5.600 | -0.053 | (0.393) |
| **Political engage. and beliefs:** | | | | | |
| Political engage. index (std) | -0.003 | 0.201 | 0.225 | -0.052 | (0.378) |
| Prev. contacted reps | 0.733 | 0.803 | 0.801 | 0.003 | (0.905) |
| Prev. donated | 0.816 | 0.863 | 0.863 | 0.000 | (1.000) |
| Prev. canvassed | 0.081 | 0.090 | 0.089 | 0.001 | (0.956) |
| Prev. signed petition | 0.835 | 0.880 | 0.894 | -0.031 | (0.140) |
| Prev. phonebanked | 0.100 | 0.134 | 0.153 | -0.042** | (0.046) |
| Political efficacy index (std) | 0.008 | -0.142 | -0.181 | 0.084 | (0.155) |
| Degree prefer Dem friends (1-7) | 6.040 | 6.072 | 6.035 | 0.081 | (0.156) |
| Sample size | 3616 | 1023 | 548 | 475 | |

Table A22: Heterogeneous WTP results by influencer traits

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **Showed extended profile** | | | | | | | | | | | | | |
| | *Influencer gender* | | | | *Climate worry* | | | | | | | | | |
| | Woman | | Man | | $<$ Median | | $\geq$ Median | | | | | | | |
| Before | 0.462*** | 0.557*** | 0.416*** | 0.473*** | 0.466*** | 0.563*** | 0.443*** | 0.523*** | | | | | | |
| | 0.015 | (0.017) | 0.032 | (0.035) | 0.023 | (0.026) | 0.017 | (0.019) | | | | | | |
| Republican | | -0.087*** | | -0.069*** | | -0.064*** | | -0.093*** | | | | | | |
| | | (0.012) | | (0.024) | | (0.017) | | (0.013) | | | | | | |
| Before * Rep | | -0.190*** | | -0.115*** | | -0.194*** | | -0.160*** | | | | | | |
| | | (0.014) | | (0.023) | | (0.021) | | (0.014) | | | | | | |
| Mean: After | 0.220 | | 0.258 | | 0.209 | | 0.239 | | | | | | | |
| Mean: After Dem | | 0.263 | | 0.292 | | 0.242 | | 0.285 | | | | | | |
| # Participants | 778 | 778 | 245 | 245 | 356 | 356 | 667 | 667 | | | | | | |
| # Choices | 15560 | 15560 | 4900 | 4900 | 7120 | 7120 | 13340 | 13340 | | | | | | |
| *p-Values: (1) = (3) or (5) = (7)* | | | | | | | | | | | | | | |
| | *Before:* 0.183 | | | | *Before:* 0.411 | | | | | | | | | |
| *p-Values: (2) = (4) or (6) = (8)* | | | | | | | | | | | | | | |
| | *Bef:* 0.026, *Rep:* 0.488, *Rep*Bef:* 0.488 | | | | *Bef:* 0.209, *Rep:* 0.173, *Rep*Bef:* 0.173 | | | | | | | | | |
| | *Predicted influence beliefs* | | | | | | | | *Qualitative influence beliefs* | | | | | |
| | *OLS* | | | | *Lasso* | | | | | | | | | |
| | $<$ Median | | $\geq$ Median | | $<$ Median | | $\geq$ Median | | Negative | | Zero | | Positive | |
| Before | 0.456*** | 0.545*** | 0.447*** | 0.530*** | 0.460*** | 0.549*** | 0.439*** | 0.524*** | 0.431*** | 0.483*** | 0.464*** | 0.563*** | 0.437*** | 0.512*** |
| | 0.018 | (0.020) | 0.019 | (0.021) | 0.017 | (0.019) | 0.019 | (0.021) | 0.042 | (0.045) | 0.019 | (0.021) | 0.024 | (0.026) |
| Republican | | -0.075*** | | -0.091*** | | -0.082*** | | -0.082*** | | -0.069** | | -0.091*** | | -0.075*** |
| | | (0.014) | | (0.015) | | (0.014) | | (0.015) | | (0.033) | | (0.014) | | (0.018) |
| Before * Rep | | -0.176*** | | -0.167*** | | -0.178*** | | -0.171*** | | -0.104*** | | -0.199*** | | -0.151*** |
| | | (0.017) | | (0.017) | | (0.017) | | (0.017) | | (0.028) | | (0.017) | | (0.020) |
| Mean: After | 0.231 | | 0.226 | | 0.224 | | 0.233 | | 0.228 | | 0.216 | | 0.249 | |
| Mean: After Dem | | 0.271 | | 0.269 | | 0.267 | | 0.273 | | 0.261 | | 0.261 | | 0.287 |
| # Participants | 787 | 787 | 801 | 801 | 854 | 854 | 931 | 931 | 108 | 108 | 555 | 555 | 360 | 360 |
| # Choices | 10230 | 10230 | 10230 | 10230 | 10227 | 10227 | 10233 | 10233 | 2160 | 2160 | 11100 | 11100 | 7200 | 7200 |
| *p-Values: (1) = (3), (5) = (7), or (9)=(11)=(13)* | | | | | | | | | | | | | | |
| | *Before:* 0.690 | | | | *Before:* 0.320 | | | | *Before:* 0.582 | | | | | |
| *p-Values: (2) = (4), (6) = (8), or (10)=(12)=(14)* | | | | | | | | | | | | | | |
| | *Bef:* 0.564, *Rep:* 0.390, *Rep*Bef:* | | | | *Bef:* 0.295, *Rep:* 0.987, *Rep*Bef:* 0.987 | | | | *Bef:* 0.128, *Rep:* 0.691, *Rep*Bef:* 0.691 | | | | | |

Table A23: Heterogeneous WTP partisan gaps by affective polarization

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **Showed extended profile** | | | | | | | | | | | |
| | *How much prefer Dem* | | | | *Residualized: How much prefer Dem* | | | | *Lasso-predicted therm* | | | |
| | No belief info | | Has belief info | | No belief info | | Has belief info | | No belief info | | Has belief info | |
| | $<$ Med | $\geq$ Med | $<$ Med | $\geq$ Med | $<$ Med | $\geq$ Med | $<$ Med | $\geq$ Med | $<$ Med | $\geq$ Med | $<$ Med | $\geq$ Med |
| Before | 0.570*** | 0.523*** | 0.552*** | 0.531*** | 0.563*** | 0.509*** | 0.561*** | 0.510*** | 0.591*** | 0.490*** | 0.541*** | 0.479*** |
| | (0.043) | (0.027) | (0.037) | (0.025) | (0.031) | (0.034) | (0.028) | (0.031) | (0.035) | (0.037) | (0.034) | (0.039) |
| **Politics** | | | | | | | | | | | | |
| Republican | -0.106*** | -0.145*** | -0.025 | -0.043** | -0.129*** | -0.141*** | -0.024 | -0.053** | -0.122*** | -0.167*** | -0.043** | -0.076*** |
| | (0.025) | (0.020) | (0.023) | (0.017) | (0.021) | (0.024) | (0.017) | (0.022) | (0.024) | (0.027) | (0.021) | (0.025) |
| Before * Rep | -0.206*** | -0.251*** | -0.112*** | -0.114*** | -0.193*** | -0.285*** | -0.120*** | -0.106*** | -0.253*** | -0.222*** | -0.125*** | -0.092*** |
| | (0.036) | (0.022) | (0.024) | (0.018) | (0.026) | (0.027) | (0.018) | (0.023) | (0.031) | (0.029) | (0.023) | (0.028) |
| Base mean | 0.275 | 0.309 | 0.219 | 0.255 | 0.289 | 0.311 | 0.221 | 0.270 | 0.267 | 0.345 | 0.232 | 0.294 |
| # Participants | 125 | 350 | 170 | 378 | 236 | 239 | 290 | 258 | 186 | 203 | 204 | 172 |
| # Choices | 2500 | 7000 | 3400 | 7560 | 4720 | 4780 | 5800 | 5160 | 3720 | 4060 | 4080 | 3440 |
| *p-val* Before: | 0.088 | | 0.389 | | 0.026 | | 0.025 | | 0.000 | | 0.027 | |
| *p-val* Republican: | 0.147 | | 0.436 | | 0.627 | | 0.207 | | 0.084 | | 0.241 | |
| *p-val* Before*Rep: | 0.262 | | 0.944 | | 0.009 | | 0.678 | | 0.422 | | 0.408 | |

Table A24: WTP heterogeneity by match traits: Without climate-belief information

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Showed extended profile | | | | | |
| Before | 0.535*** | 0.365*** | 0.394*** | 0.352*** | 0.376*** | 0.437*** |
| | (0.023) | (0.021) | (0.024) | (0.022) | (0.028) | (0.037) |
| **Politics** | | | | | | |
| Republican | -0.135*** | | | | | -0.143*** |
| | (0.016) | | | | | (0.017) |
| Before * Republican | -0.239*** | | | | | -0.220*** |
| | (0.019) | | | | | (0.021) |
| **Gender** | | | | | | |
| Woman | | 0.036*** | | | | 0.001 |
| | | (0.012) | | | | (0.012) |
| Before * Woman | | 0.112*** | | | | 0.052*** |
| | | (0.017) | | | | (0.017) |
| **Education** | | | | | | |
| 4-year college degree | | | -0.004 | | | -0.005 |
| | | | (0.012) | | | (0.012) |
| Before * 4-year college degree | | | 0.031* | | | 0.024 |
| | | | (0.018) | | | (0.017) |
| **State marginality groups** | | | | | | |
| Blue state | | | | 0.030** | | 0.002 |
| | | | | (0.015) | | (0.015) |
| Before * Blue state | | | | 0.124*** | | 0.065*** |
| | | | | (0.021) | | (0.021) |
| Purple state | | | | 0.016 | | 0.019 |
| | | | | (0.013) | | (0.013) |
| Before * Purple state | | | | 0.065*** | | 0.059*** |
| | | | | (0.019) | | (0.019) |
| **Age ranges** | | | | | | |
| Age 40-59 | | | | | -0.002 | 0.002 |
| | | | | | (0.015) | (0.015) |
| Before * Age 40-59 | | | | | -0.003 | 0.007 |
| | | | | | (0.024) | (0.023) |
| Age 60-79 | | | | | 0.050*** | -0.013 |
| | | | | | (0.017) | (0.017) |
| Before * Age 60-79 | | | | | 0.110*** | 0.009 |
| | | | | | (0.026) | (0.026) |
| Control mean | 0.300 | 0.217 | 0.234 | 0.211 | 0.213 | 0.300 |
| Num. participants | 475 | 475 | 475 | 475 | 475 | 475 |
| Num. choices | 9500 | 9500 | 9500 | 9500 | 9500 | 9500 |

Table A25: WTP heterogeneity by match traits: With climate-belief information

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Showed extended profile | | | | | |
| Before | 0.538*** | 0.456*** | 0.476*** | 0.464*** | 0.457*** | 0.502*** |
| | (0.021) | (0.021) | (0.022) | (0.023) | (0.024) | (0.032) |
| **Politics** | | | | | | |
| Republican | -0.037*** | | | | | -0.037** |
| | (0.014) | | | | | (0.014) |
| Before * Republican | -0.115*** | | | | | -0.110*** |
| | (0.015) | | | | | (0.016) |
| **Gender** | | | | | | |
| Woman | | 0.011 | | | | 0.003 |
| | | (0.010) | | | | (0.011) |
| Before * Woman | | 0.057*** | | | | 0.030* |
| | | (0.014) | | | | (0.016) |
| **Education** | | | | | | |
| 4-year college degree | | | 0.009 | | | 0.008 |
| | | | (0.011) | | | (0.011) |
| Before * 4-year college degree | | | 0.006 | | | 0.003 |
| | | | (0.015) | | | (0.015) |
| **State marginality groups** | | | | | | |
| Blue state | | | | 0.008 | | -0.002 |
| | | | | (0.015) | | (0.014) |
| Before * Blue state | | | | 0.036* | | 0.011 |
| | | | | (0.020) | | (0.020) |
| Purple state | | | | -0.002 | | -0.003 |
| | | | | (0.013) | | (0.013) |
| Before * Purple state | | | | 0.016 | | 0.017 |
| | | | | (0.019) | | (0.019) |
| **Age ranges** | | | | | | |
| Age 40-59 | | | | | -0.011 | -0.011 |
| | | | | | (0.014) | (0.014) |
| Before * Age 40-59 | | | | | 0.020 | 0.018 |
| | | | | | (0.019) | (0.019) |
| Age 60-79 | | | | | 0.006 | -0.009 |
| | | | | | (0.014) | (0.014) |
| Before * Age 60-79 | | | | | 0.046** | -0.001 |
| | | | | | (0.020) | (0.021) |
| Control mean | 0.244 | 0.223 | 0.218 | 0.226 | 0.225 | 0.244 |
| Num. participants | 548 | 548 | 548 | 548 | 548 | 548 |
| Num. choices | 10960 | 10960 | 10960 | 10960 | 10960 | 10960 |

Table A26: WTP heterogeneity by similarity: Without climate-belief information

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Showed extended profile | | | | | | | |
| Before | 0.296*** | 0.395*** | 0.376*** | 0.410*** | 0.418*** | 0.243*** | 0.287*** | 0.255*** |
| | (0.021) | (0.021) | (0.025) | (0.020) | (0.020) | (0.027) | (0.021) | (0.025) |
| **Politics** | | | | | | | | |
| Same party | 0.135*** | | | | | 0.131*** | 0.136*** | 0.135*** |
| | (0.016) | | | | | (0.016) | (0.016) | (0.016) |
| Before * Same party | 0.239*** | | | | | 0.238*** | 0.235*** | 0.235*** |
| | (0.019) | | | | | (0.019) | (0.019) | (0.019) |
| **Gender** | | | | | | | | |
| Same gender | | 0.049*** | | | | 0.032*** | | |
| | | (0.012) | | | | (0.012) | | |
| Before * Same gender | | 0.044** | | | | 0.017 | | |
| | | (0.019) | | | | (0.018) | | |
| **Education** | | | | | | | | |
| Same college attainment | | | -0.028** | | | -0.029** | | |
| | | | (0.014) | | | (0.014) | | |
| Before * Same college | | | 0.062*** | | | 0.069*** | | |
| | | | (0.022) | | | (0.021) | | |
| **State** | | | | | | | | |
| Same state group | | | | 0.007 | | -0.005 | | |
| | | | | (0.021) | | (0.021) | | |
| Before * Same state | | | | 0.061** | | 0.060** | | |
| | | | | (0.031) | | (0.029) | | |
| **Age range** | | | | | | | | |
| Same age range | | | | | 0.010 | 0.001 | | |
| | | | | | (0.015) | (0.015) | | |
| Before * Same age | | | | | -0.011 | -0.020 | | |
| | | | | | (0.023) | (0.021) | | |
| **Overall non-political similarity** | | | | | | | | |
| Pred. non-pol sim | | | | | | | -0.005 | |
| | | | | | | | (0.007) | |
| Before * Pred. non-pol sim | | | | | | | 0.037*** | |
| | | | | | | | (0.010) | |
| # Non-pol matched traits | | | | | | | | 0.002 |
| | | | | | | | | (0.007) |
| Before * # Non-pol matches | | | | | | | | 0.031*** |
| | | | | | | | | (0.011) |
| Control mean | 0.165 | 0.211 | 0.247 | 0.232 | 0.228 | 0.300 | 0.300 | 0.300 |
| Num. participants | 475 | 475 | 475 | 475 | 475 | 475 | 475 | 475 |
| Num. choices | 9500 | 9500 | 9500 | 9500 | 9500 | 9500 | 9500 | 9500 |

Table A27: WTP heterogeneity by similarity: With climate-belief information

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Showed extended profile | | | | | | | |
| Before | 0.424*** | 0.466*** | 0.466*** | 0.479*** | 0.486*** | 0.405*** | 0.420*** | 0.409*** |
| | (0.020) | (0.021) | (0.024) | (0.019) | (0.020) | (0.026) | (0.021) | (0.025) |
| **Politics** | | | | | | | | |
| Same party | 0.037*** | | | | | 0.034** | 0.036*** | 0.035** |
| | (0.014) | | | | | (0.014) | (0.014) | (0.014) |
| Before * Same party | 0.115*** | | | | | 0.112*** | 0.113*** | 0.113*** |
| | (0.015) | | | | | (0.015) | (0.015) | (0.015) |
| **Gender** | | | | | | | | |
| Same gender | | 0.024** | | | | 0.020* | | |
| | | (0.010) | | | | (0.010) | | |
| Before * Same gender | | 0.033** | | | | 0.020 | | |
| | | (0.015) | | | | (0.015) | | |
| **Education** | | | | | | | | |
| Same college attainment | | | 0.001 | | | -0.000 | | |
| | | | (0.012) | | | (0.012) | | |
| Before * Same college | | | 0.024 | | | 0.021 | | |
| | | | (0.019) | | | (0.019) | | |
| **State** | | | | | | | | |
| Same state group | | | | 0.010 | | 0.008 | | |
| | | | | (0.021) | | (0.021) | | |
| Before * Same state | | | | 0.022 | | 0.019 | | |
| | | | | (0.027) | | (0.027) | | |
| **Age range** | | | | | | | | |
| Same age range | | | | | 0.016 | 0.014 | | |
| | | | | | (0.015) | (0.015) | | |
| Before * Same age | | | | | -0.024 | -0.023 | | |
| | | | | | (0.021) | (0.021) | | |
| **Overall non-political similarity** | | | | | | | | |
| Pred. non-pol sim | | | | | | | 0.008 | |
| | | | | | | | (0.006) | |
| Before * Pred. non-pol sim | | | | | | | 0.012 | |
| | | | | | | | (0.010) | |
| # Non-pol matched traits | | | | | | | | 0.011* |
| | | | | | | | | (0.007) |
| Before * # Non-pol matches | | | | | | | | 0.011 |
| | | | | | | | | (0.010) |
| Control mean | 0.206 | 0.218 | 0.226 | 0.225 | 0.221 | 0.244 | 0.244 | 0.244 |
| Num. participants | 548 | 548 | 548 | 548 | 548 | 548 | 548 | 548 |
| Num. choices | 10960 | 10960 | 10960 | 10960 | 10960 | 10960 | 10960 | 10960 |

Table A28: Comparing the belief and certainty samples to Wave 1 and the WTP samples

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | Belief sample | | | Certainty sample | | | WTP Round 2 | | |
| | Wave-1 | WTP | Mean | Δ Wave1 | Δ WTP | Mean | Δ Wave1 | Δ WTP | Mean | Δ Wave1 | Δ WTP |
| Woman | 0.632 | 0.761 | 0.587 | -0.045* | -0.174*** | 0.727 | 0.095*** | -0.033* | 0.677 | 0.045*** | -0.083*** |
| Hispanic | 1.027 | 1.039 | 1.028 | 0.001 | -0.011 | 1.026 | -0.000 | -0.013 | 1.025 | -0.001 | -0.014* |
| Has $\geq$ 4-year college degree | 0.828 | 0.862 | 0.854 | 0.026 | -0.008 | 0.867 | 0.040*** | 0.005 | 0.845 | 0.018 | -0.017 |
| **Age ranges:** | | | | | | | | | | | |
| 20-39 | 0.119 | 0.146 | 0.264 | 0.145*** | 0.119*** | 0.179 | 0.060*** | 0.033** | 0.055 | -0.064*** | -0.090*** |
| 40-59 | 0.337 | 0.354 | 0.385 | 0.049* | 0.032 | 0.422 | 0.086*** | 0.068*** | 0.264 | -0.072*** | -0.090*** |
| 60-79 | 0.544 | 0.500 | 0.350 | -0.194*** | -0.150*** | 0.399 | -0.145*** | -0.102*** | 0.680 | 0.136*** | 0.180*** |
| **Income bins (USD):** | | | | | | | | | | | |
| Less than 50,000 | 0.170 | 0.166 | 0.113 | -0.057*** | -0.053*** | 0.143 | -0.027** | -0.023 | 0.172 | 0.002 | 0.006 |
| 50,000-99,999 | 0.325 | 0.338 | 0.229 | -0.096*** | -0.109*** | 0.281 | -0.045*** | -0.057*** | 0.350 | 0.024 | 0.012 |
| 100,000-149,999 | 0.231 | 0.238 | 0.300 | 0.069*** | 0.062** | 0.251 | 0.020 | 0.013 | 0.234 | 0.004 | -0.003 |
| 150,000-199,999 | 0.126 | 0.118 | 0.181 | 0.055*** | 0.063*** | 0.150 | 0.024** | 0.032** | 0.117 | -0.010 | -0.002 |
| 200,000 or more | 0.147 | 0.140 | 0.176 | 0.029 | 0.037* | 0.175 | 0.028** | 0.035** | 0.128 | -0.020* | -0.012 |
| **State marginality:** | | | | | | | | | | | |
| Red state | 0.231 | 0.220 | 0.227 | -0.004 | 0.007 | 0.240 | 0.009 | 0.020 | 0.243 | 0.013 | 0.023 |
| Blue state | 0.438 | 0.457 | 0.481 | 0.043* | 0.025 | 0.451 | 0.013 | -0.005 | 0.454 | 0.016 | -0.002 |
| Purple state | 0.331 | 0.324 | 0.292 | -0.039 | -0.031 | 0.309 | -0.022 | -0.015 | 0.303 | -0.029* | -0.021 |
| **Climate beliefs:** | | | | | | | | | | | |
| Climate worry (1-7) | 6.430 | 6.547 | 6.395 | -0.034 | -0.152*** | 6.466 | 0.036 | -0.082** | 6.605 | 0.175*** | 0.058* |
| Desire for climate action (1-7) | 6.735 | 6.823 | 6.725 | -0.009 | -0.098*** | 6.744 | 0.010 | -0.079*** | 6.809 | 0.075*** | -0.014 |
| Perceived local impacts (1-7) | 5.496 | 5.576 | 5.479 | -0.018 | -0.097* | 5.490 | -0.006 | -0.086* | 5.610 | 0.114*** | 0.034 |
| **Political engage. and beliefs:** | | | | | | | | | | | |
| Prev. contacted reps | 0.733 | 0.803 | 0.703 | -0.030 | -0.100*** | 0.727 | -0.006 | -0.076*** | 0.806 | 0.073*** | 0.003 |
| Prev. donated | 0.816 | 0.863 | 0.798 | -0.017 | -0.065*** | 0.813 | -0.002 | -0.050*** | 0.841 | 0.026** | -0.022 |
| Prev. canvassed | 0.081 | 0.090 | 0.091 | 0.009 | 0.001 | 0.098 | 0.016 | 0.008 | 0.107 | 0.025** | 0.017 |
| Prev. signed petition | 0.835 | 0.880 | 0.816 | -0.019 | -0.064*** | 0.822 | -0.013 | -0.058*** | 0.855 | 0.021 | -0.024 |
| Prev. phonebanked | 0.100 | 0.134 | 0.091 | -0.010 | -0.043** | 0.118 | 0.018 | -0.016 | 0.143 | 0.042*** | 0.009 |
| Degree prefer Dem friends (1-7) | 6.040 | 6.072 | 6.033 | -0.008 | -0.040 | 6.049 | 0.009 | -0.023 | 6.005 | -0.035 | -0.067 |
| Sample size | 3616 | 1023 | | 397 | | | 1033 | | | 995 | |

Table A29: Influence beliefs by recipients traits: Incentivized

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Probability of emailing Congress | | | | | |
| Before | 6.155*** | 5.135*** | 4.286*** | 6.049*** | 4.724*** | 7.421*** |
| | (0.810) | (0.940) | (0.914) | (1.152) | (1.266) | (2.179) |
| **Politics** | | | | | | |
| Republican | -10.164*** | | | | | -10.164*** |
| | (1.135) | | | | | (1.145) |
| Before * Rep | -2.990** | | | | | -2.990** |
| | (1.177) | | | | | (1.188) |
| **Gender** | | | | | | |
| Woman | | 6.100** | | | | 6.317** |
| | | (2.594) | | | | (2.580) |
| Before * Woman | | -0.942 | | | | -0.899 |
| | | (1.433) | | | | (1.507) |
| **Education** | | | | | | |
| College | | | 6.440** | | | 6.886** |
| | | | (2.644) | | | (2.728) |
| Before * College | | | 1.035 | | | 1.641 |
| | | | (1.471) | | | (1.505) |
| **State groups** | | | | | | |
| Blue state | | | | -0.234 | | -0.628 |
| | | | | (3.391) | | (3.354) |
| Before * Blue state | | | | -2.492 | | -2.718 |
| | | | | (1.833) | | (1.799) |
| Purple state | | | | -2.354 | | -1.581 |
| | | | | (3.004) | | (2.985) |
| Before * Purple state | | | | -2.295 | | -2.262 |
| | | | | (1.633) | | (1.698) |
| **Age ranges** | | | | | | |
| Age 40-59 | | | | | -2.684 | 0.369 |
| | | | | | (3.697) | (3.771) |
| Before * 40-59 | | | | | 1.283 | 1.626 |
| | | | | | (1.776) | (1.883) |
| Age 60-79 | | | | | -1.769 | -0.528 |
| | | | | | (3.917) | (3.974) |
| Before * 60-79 | | | | | -2.836 | -2.987 |
| | | | | | (2.371) | (2.497) |
| Control mean | 38.907 | 30.609 | 31.367 | 34.494 | 35.408 | 38.907 |
| # Participants | 194 | 194 | 194 | 194 | 194 | 194 |
| # Choices | 776 | 776 | 776 | 776 | 776 | 776 |

Table A30: Influence beliefs by similarity: Incentivized

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Probability of emailing Congress | | | | | | | |
| Before | 3.165*** | 4.758*** | 5.237*** | 4.651*** | 4.794*** | 3.885** | 3.224*** | 3.999** |
| | (1.034) | (1.151) | (0.967) | (0.769) | (0.883) | (1.634) | 1.048 | 1.594 |
| **Matching on demographics** | | | | | | | | |
| Before * Same party | 2.990** | | | | | 2.990** | 2.990** | 2.990** |
| | (1.177) | | | | | (1.183) | 1.179 | 1.179 |
| Before * Same gender | | -0.189 | | | | 0.004 | | |
| | | (1.451) | | | | (1.470) | | |
| Before * Same college | | | -1.515 | | | -1.522 | | |
| | | | (1.415) | | | (1.429) | | |
| Before * Same state | | | | 0.069 | | -0.113 | | |
| | | | | (2.145) | | (2.261) | | |
| Before * Same age | | | | | -0.373 | -0.352 | | |
| | | | | | (1.515) | (1.616) | | |
| **Overall non-political similarity** | | | | | | | | |
| Before * Pred. non-pol sim | | | | | | | -0.582 | |
| | | | | | | | (0.653) | |
| Before * # Non-pol matched traits | | | | | | | | -0.599 |
| | | | | | | | | (0.723) |
| Control mean | 28.474 | 30.978 | 32.679 | 34.382 | 35.149 | 28.474 | 38.907 | 38.907 |
| Num. participants | 194 | 194 | 194 | 194 | 194 | 194 | 194 | 194 |
| Num. choices | 776 | 776 | 776 | 776 | 776 | 776 | 776 | 776 |

Table A31: Demographics across certainty rounds

| | (1) | (2) | |
|---|---|---|---|
| | Means: | | *p-value* |
| | Round 1 | Round 2 | (1) = (2) |
| Woman | 0.584 | 0.841 | (0.000) |
| Hispanic | 1.028 | 1.024 | (0.689) |
| Has $\geq$ 4-year college degree | 0.863 | 0.871 | (0.703) |
| | | | |
| **Age ranges:** | | | |
| 20-39 | 0.251 | 0.122 | (0.000) |
| 40-59 | 0.375 | 0.460 | (0.006) |
| 60-79 | 0.375 | 0.418 | (0.166) |
| | | | |
| **Income bins (USD):** | | | |
| Less than 50,000 | 0.113 | 0.167 | (0.014) |
| 50,000-99,999 | 0.244 | 0.310 | (0.019) |
| 100,000-149,999 | 0.294 | 0.216 | (0.004) |
| 150,000-199,999 | 0.172 | 0.132 | (0.082) |
| 200,000 or more | 0.176 | 0.174 | (0.934) |
| | | | |
| **State marginality:** | | | |
| Red state | 0.240 | 0.240 | (0.970) |
| Blue state | 0.466 | 0.439 | (0.384) |
| Purple state | 0.294 | 0.321 | (0.370) |
| | | | |
| **Climate beliefs:** | | | |
| Climate worry (1-7) | 6.388 | 6.528 | (0.003) |
| Desire for climate action (1-7) | 6.708 | 6.774 | (0.049) |
| Perceived local impacts (1-7) | 5.484 | 5.495 | (0.859) |
| | | | |
| **Political engage. and beliefs:** | | | |
| Political engage. index (std) | -0.098 | 0.088 | (0.004) |
| Prev. contacted reps | 0.688 | 0.758 | (0.014) |
| Prev. donated | 0.793 | 0.829 | (0.150) |
| Prev. canvassed | 0.089 | 0.105 | (0.405) |
| Prev. signed petition | 0.802 | 0.838 | (0.134) |
| Prev. phonebanked | 0.092 | 0.139 | (0.017) |
| Political efficacy index (std) | -0.001 | -0.043 | (0.509) |
| Degree prefer Dem friends (1-7) | 6.024 | 6.070 | (0.412) |
| | | | |
| Sample size | 459 | 574 | |

Table A32: Certainty results table

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Chose email over offsets | | | | Impact on climate policy | | | |
| | | *State groups* | | | | *State groups* | | |
| | All | Red | Blue | Purple | All | Red | Blue | Purple |
| Republican | -0.001 | -0.003 | 0.012 | -0.019 | 0.080 | 0.199* | -0.045 | 0.121 |
| | (0.013) | (0.017) | (0.020) | (0.026) | (0.063) | (0.119) | (0.101) | (0.123) |
| Woman | 0.001 | 0.011 | -0.002 | -0.002 | -0.007 | -0.085 | 0.073 | -0.049 |
| | (0.008) | (0.014) | (0.018) | (0.032) | (0.069) | (0.127) | (0.119) | (0.245) |
| Has 4-year college | 0.012 | 0.002 | -0.000 | 0.006 | -0.004 | 0.032 | -0.014 | -0.150 |
| | (0.011) | (0.019) | (0.020) | (0.046) | (0.082) | (0.162) | (0.133) | (0.201) |
| **Age ranges:** | | | | | | | | |
| 35-49 | 0.013 | -0.014 | 0.009 | -0.009 | -0.080 | -0.280 | 0.021 | 0.049 |
| | (0.013) | (0.019) | (0.035) | (0.054) | (0.108) | (0.175) | (0.195) | (0.237) |
| 50-64 | 0.014 | 0.010 | 0.027 | 0.011 | -0.139 | -0.163 | -0.288 | -0.015 |
| | (0.012) | (0.019) | (0.034) | (0.051) | (0.105) | (0.187) | (0.181) | (0.253) |
| 65-79 | 0.013 | -0.032 | 0.032 | 0.035 | -0.201** | -0.359 | -0.264* | 0.021 |
| | (0.014) | (0.020) | (0.035) | (0.056) | (0.100) | (0.238) | (0.141) | (0.195) |
| **State policy groups:** | | | | | | | | |
| Blue state | -0.108*** | | | | 0.245*** | | | |
| | (0.015) | | | | (0.080) | | | |
| Purple state | 0.047*** | | | | 0.200** | | | |
| | (0.011) | | | | (0.088) | | | |
| Sample mean | 0.601 | 0.630 | 0.523 | 0.671 | -0.025 | -0.160 | 0.054 | 0.017 |
| N | 11593 | 4862 | 4020 | 2711 | 1002 | 326 | 400 | 276 |

Table A33: Balance table, Round 2 WTP

| | (1) | (2) | ) |
|---|---|---|---|
| | Means | | *p-value* |
| | Baseline | Choose politics | (1) = (2) |
| Woman | 0.675 | 0.680 | (0.894) |
| Hispanic | 1.022 | 1.029 | (0.484) |
| Has $\geq$ 4-year college degree | 0.818 | 0.873 | (0.015) |
| **Age ranges:** | | | |
| 20-39 | 0.053 | 0.057 | (0.790) |
| 40-59 | 0.267 | 0.261 | (0.830) |
| 60-79 | 0.679 | 0.682 | (0.947) |
| **Income bins (USD):** | | | |
| Less than 50,000 | 0.196 | 0.147 | (0.041) |
| 50,000-99,999 | 0.350 | 0.349 | (0.947) |
| 100,000-149,999 | 0.220 | 0.249 | (0.283) |
| 150,000-199,999 | 0.119 | 0.114 | (0.803) |
| 200,000 or more | 0.115 | 0.141 | (0.216) |
| **State marginality:** | | | |
| Red state | 0.263 | 0.222 | (0.129) |
| Blue state | 0.446 | 0.463 | (0.574) |
| Purple state | 0.291 | 0.314 | (0.428) |
| **Climate beliefs:** | | | |
| Climate worry (1-7) | 6.600 | 6.610 | (0.807) |
| Desire for climate action (1-7) | 6.812 | 6.806 | (0.830) |
| Perceived local impacts (1-7) | 5.586 | 5.635 | (0.444) |
| **Political engage. and beliefs:** | | | |
| Political engage. index (std) | 0.195 | 0.159 | (0.549) |
| Prev. contacted reps | 0.822 | 0.790 | (0.201) |
| Prev. donated | 0.830 | 0.853 | (0.318) |
| Prev. canvassed | 0.095 | 0.118 | (0.250) |
| Prev. signed petition | 0.871 | 0.839 | (0.134) |
| Prev. phonebanked | 0.141 | 0.145 | (0.856) |
| Political efficacy index (std) | -0.080 | -0.032 | (0.439) |
| Degree prefer Dem friends (1-7) | 6.016 | 5.994 | (0.723) |
| Sample size | 505 | 490 | |

Table A34: Main WTP results, Round 2

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Showed extended profile | | | |
| | Must show politics | | Can hide politics | |
| Before | 0.448*** | 0.497*** | 0.409*** | 0.453*** |
| | (0.022) | (0.024) | (0.021) | (0.022) |
| Republican | | -0.040*** | | -0.064*** |
| | | (0.013) | | (0.015) |
| Before * Republican | | -0.097*** | | -0.088*** |
| | | (0.015) | | (0.015) |
| Mean: After Dem + Rep | 0.265 | | 0.294 | |
| Mean: After Dem | | 0.284 | | 0.326 |
| Num. participants | 500 | 500 | 486 | 486 |
| Num. choices | 10000 | 10000 | 9720 | 9720 |
| *p-values (2) = (4)* | | | | |
| Republican: 0.234 | | | | |
| Before * Republican: 0.673 | | | | |

# B Details of the experimental set-up

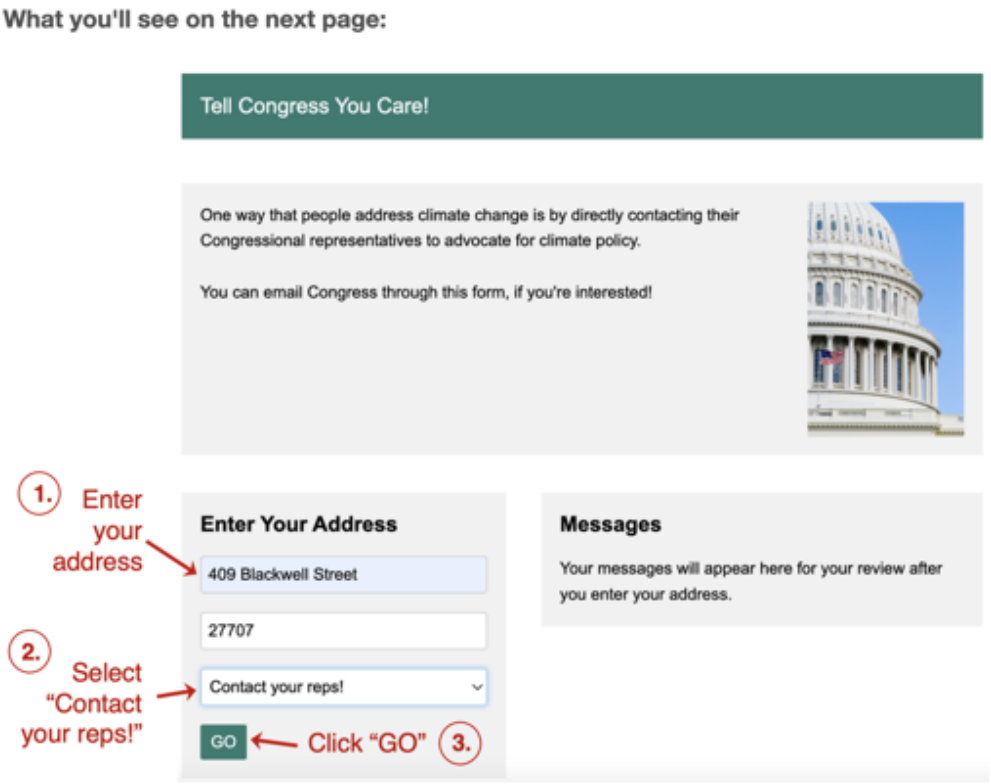
## B.1 The opportunity to email Congress

Throughout this project, our measure of climate advocacy is whether participants email Congress about climate change through a form embedded in our survey. We license the form from the Soft Edge, which licenses nearly identical forms to a range of advocacy groups in the US. The following sections describe how this form works, how we introduce the email opportunity to participants, and how we match participants to email records.

### B.1.1 The email software

Appendix Figures B37 through B39 depict the process of emailing Congress via the contact form in our survey. We use the same screenshots to explain the process to survey participants, as we describe in the next section. The email form is embedded in a page of our Qualtrics survey. Participants start the email process by entering their residential address into the form landing page:

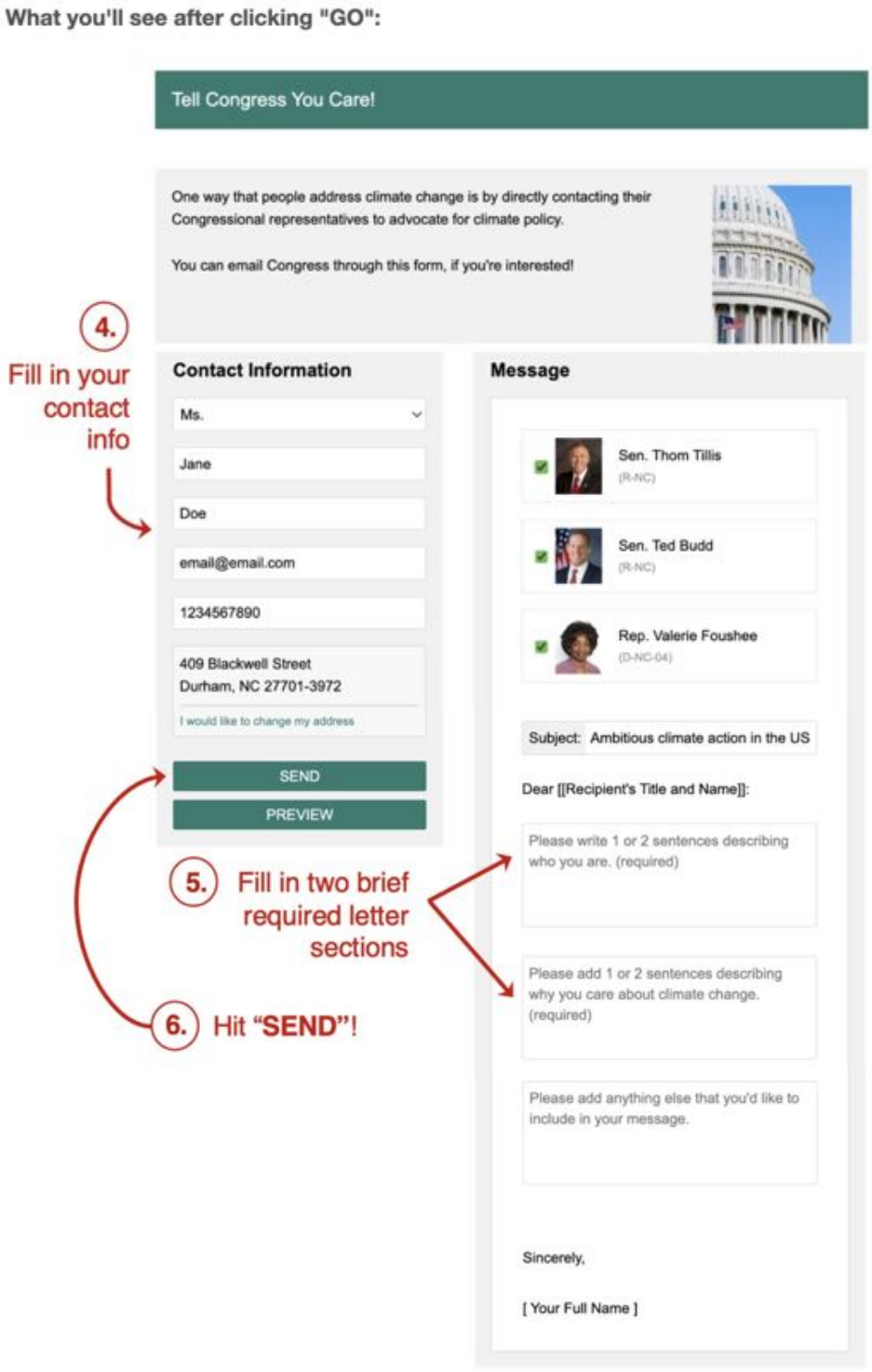
Figure B37: SoftEdge email form, Page 1




Once participants click "Go," the form autopopulates their national Senators and U.S. Representative, as shown in Appendix Figure B38. Participants fill in their name and contact information and then fill in the email text. The form requires that participants fill out two open response fields. We recommend to participants that they use these spaces to describe who they are and why they care about climate change, but they can fill in any text they wish. While the email body is fully customizable, the email subject line is fixed

to support ambitious climate policy. We randomize the subject line from options such as "Ambitious climate action in the US," "Addressing climate change is crucial," and "My strong support for US climate policy." While we make the email body fully customizable to ensure that emailing Congress is a meaningful, fairly costly action, we fix the subject line to ensure that no participant can use the email form to fully oppose climate policy; this restriction will be common knowledge to all participants, as we describe in Section B.1.2 below.

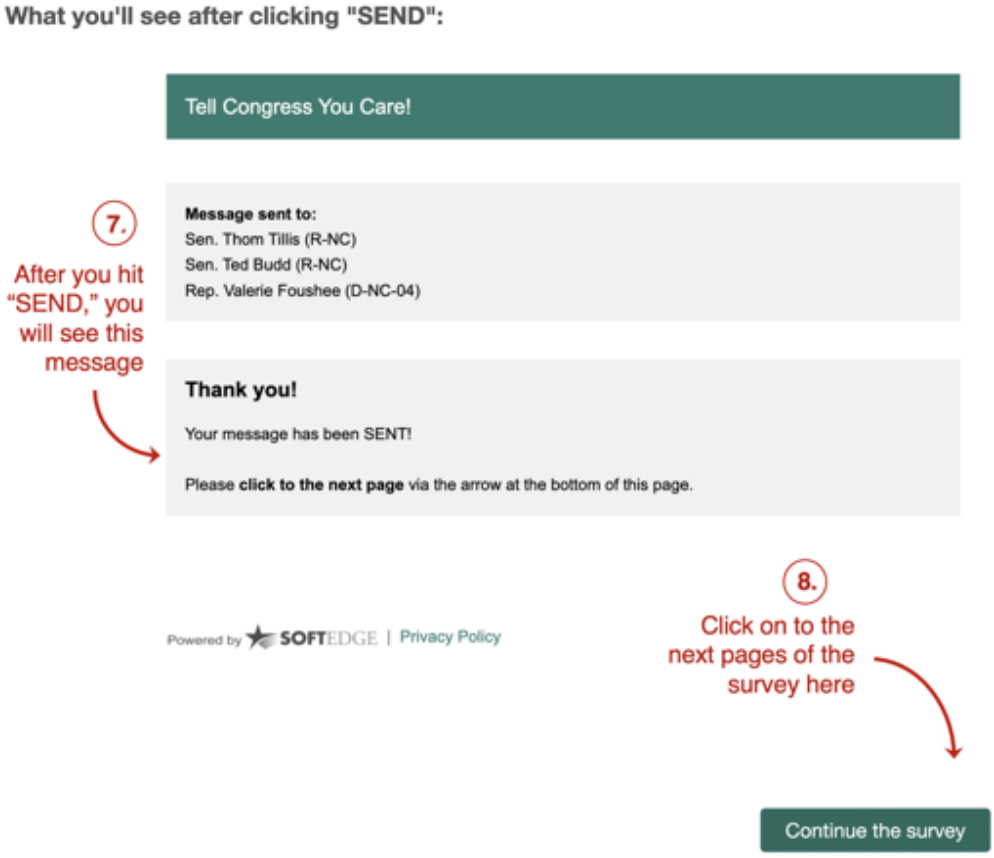
Figure B38: SoftEdge email form, Page 2




Participants can send their email by clicking the "SEND" button shown in Appendix Figure B38. Once they do so, the form updates to a confirmation and thank you message, as shown in Figure B39. Participants can then click on to continue with their survey. While we cannot directly observe participants' clicks or

activity in the SoftEdge form itself, we observe the full text of all emails sent via this form. Appendix Section B.1.3 below describes the process by which we match participants to email records.

Figure B39: SoftEdge email form, Page 3




### B.1.2 How participants choose whether to email Congress

This section briefly describes the process by which participants learn about and decide whether to participate in the opportunity to email Congress via our form. In both the Wave-1 and Wave-2 surveys, we first introduce the upcoming opportunity to email Congress with the text shown in Appendix Figure B40a. We try to phrase this introduction as neutrally as possible. Next, we describe the SoftEdge email form in more detail, as shown in Appendix Figure B40b, alongside a preview of the email form itself; Appendix Figure B40c shows this email preview.

Note that this description points out that the email form will have an un-editable subject line supporting climate policy. Participants assigned to either the Invitation group (A2) or the Tell-After group (A1) in the Wave-1 experimental survey see this preview and description of the SoftEdge form before learning that their decision to email Congress will be passed on to others. Thus, when A2 participants first consider the possibility that their own action could influence Wave-2 participants' action, they know that no Wave-2 participant can use the email form to uniformly oppose climate policy. Across all of our experimental surveys, participants see these previews before deciding whether to start the email process or not.

Figure B40: Previewing the upcoming opportunity to email Congress

(a) Initial intro to action

One way that some people choose to address climate change is to **contact their representatives in Congress** and directly advocate for climate policy.

In a few slides, we'll provide you with a form, embedded in this survey, through which **you can write a short email to Congress** asking them to prioritize ambitious climate policy, if you'd like.

(Please note: It's **totally fine** if you'd rather not. It doesn't help or hurt our research either way, and you'll be entered into the lottery for gift card prizes no matter what.)

(b) Description of the SoftEdge form

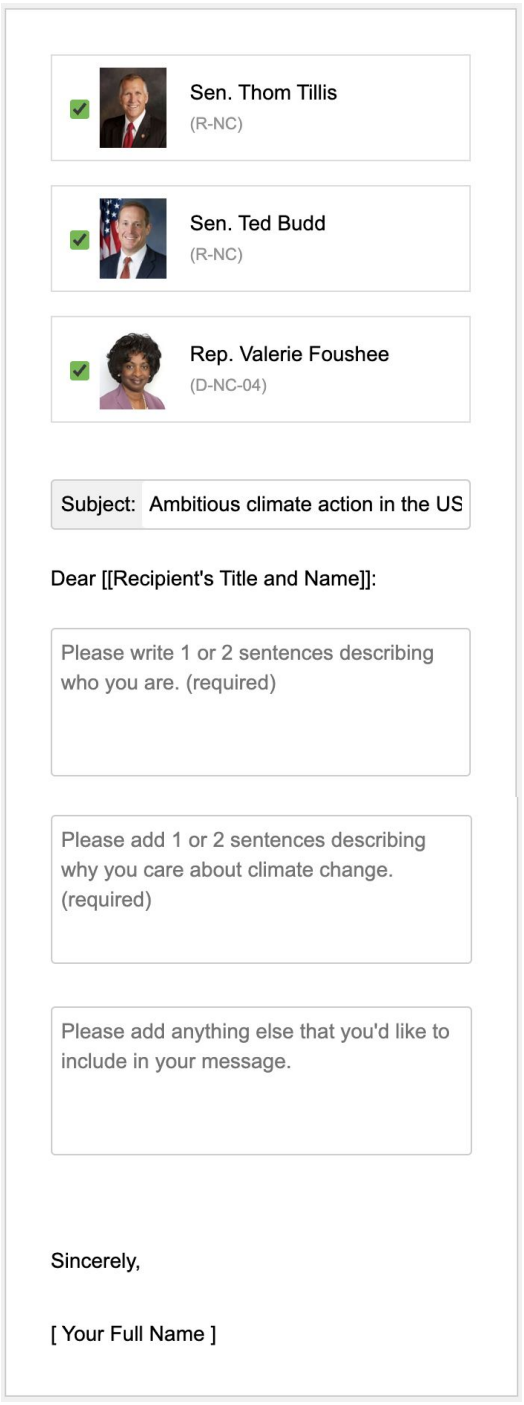
Here's a screenshot of what the form to contact Congress will look like. Based on your zip code, the form will identify your Senators and House Representative and address the email to them. (Right now it's filled in like you live in **North Carolina,** but this is **just an example.**)

The email will have an **un-editable** subject line **supporting climate policy**.

Then, the body of the message has several blanks where we'll ask you to **fill in details about who you are and why you care about climate change**.

(Surveys of political staffers show that emails are much more effective when they're personalized!)

(c) Preview of the email form

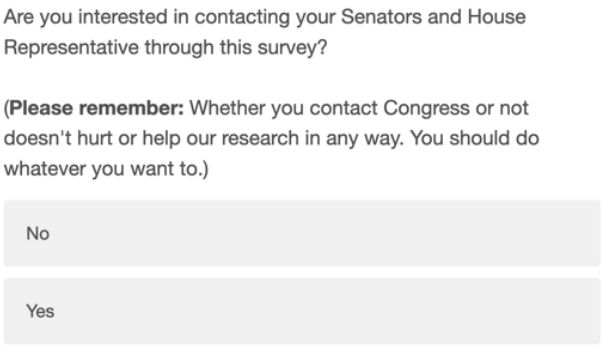



After the treatment interventions in both Waves 1 and 2, we then offer participants the chance to email Congress or not. First, we ask participants whether they want to opt into starting the email process with the questions shown in Appendix Figure B41. Across each of our main experimental surveys, we add text to

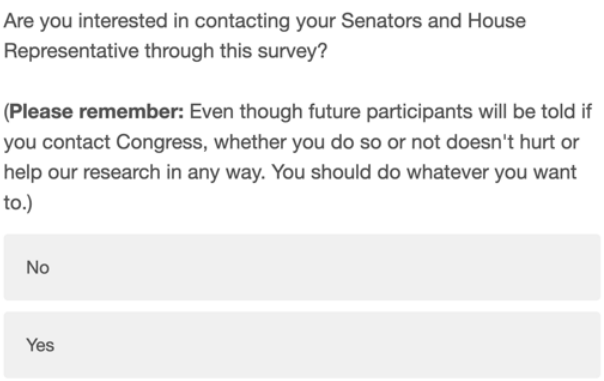
this initial question reiterating to participants that they should feel free not to email Congress if they do not wish to; we vary the text of this disclaimer somewhat between Waves 1 and Waves 2 and, within the Wave-1 sample, between participants assigned to the A0 pure control group versus the A1 or A2 treatment groups. Throughout this project, we use a binary variable for whether participants initially opt into the email process at this step as a secondary measure of climate action. As we describe below, or primary measure of action is whether participants ultimately match to an email record.

Figure B41: Initial invitations to opt into the email process

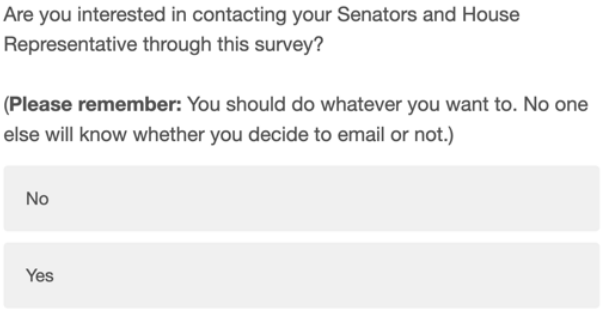
(a) Wave 1: A0 group




(b) Wave 1: A1 and A2 groups

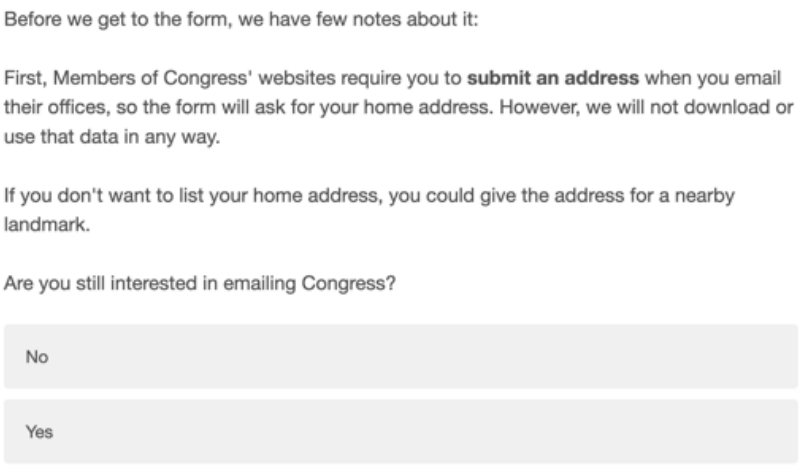



(c) Wave 2




If participants initially express interest in emailing Congress at this point, we then tell them that the form will ask for a home address and ask if they are still interested in emailing Congress (Appendix Figure B42). If participants again say yes, we then ask them to commit to continuing the survey after sending their email and lay out detailed instructions for using the email form. In particular, we walk participants through the email process via the instruction figures shown in Appendix Figures B37 through B39. The email form itself is then embedded in the next page of the survey.

Figure B42: Second invitation to email Congress




### B.1.3 Merging participants with email records

We merge email records to individual study participants using combinations of name, email address, treatment status, state, and the time and day on which participants completed the survey. (Note that we can perfectly observe treatment status because we embed and see records from separate email forms for each treatment variation.) In total, 4,124 emails were sent by unique Wave-1 participants[49] and 1,834 were sent by unique Wave-2 participants.

1. In both Waves 1 and 2, we first merge SoftEdge email records to participants using email address and treatment assignment. In Wave 1, 3,968 emails (90%) merge to participants at this step. In Wave 2, 1,587 emails (87%) merge to participants at this step.

2. Next, we merge on full name, state, treatment assignment, and the date and time at which participants took their survey. We restrict matches to those where the email was sent within 2 hours of a participant starting the survey. In Wave 1, an additional 145 emails merge at this stage, for a total of 93% of emails merged. In Wave 2, an addition 59 emails merge at this stage, for a total of 90% of emails merged.

3. Finally, we merge participants with email records on first name, state, treatment status, and the date and time at which participants took their survey. We restrict matches to whose where the participant did not provide a last name in the survey consent, the participant attested in the survey that they emailed Congress, and the email was sent within 1 hour of the participant starting they survey. This step merges an additional 130 Wave-1 emails and 28 Wave-2 emails. In total, then, we merge 3,968 out of 4,124 total Wave-1 emails (96%) and 1,674 out of 1,834 total Wave-2 emails (91%).

In both Waves 1 and 2, there are no differential match rates by treatment status.

## B.2 Forming state groups

A key piece of information included in study participants' demographic profiles is the state or group of states in which they live. We form groups of states, rather than simply showing the single state in which

[49] This total includes emails sent by participants who are not included in the sample for the Wave-1 action experiment, but rather who were recruited directly for the WTP experiment (Appendix Section E.2) or randomized to the belief sample (Section 7.1).

participants live, both to protect participants' anonymity and to ensure that the demographic cells that we randomly pair across Waves 1 and 2 are large enough to facilitate our research design. We create groups of states that are both geographically close and are perceived to have legislators whose support for climate policy is similarly marginal with respect to citizen advocacy. In other words, we aim to create groups of states that all fall in one of the following categories:

- "Red states:" States where legislators would be unlikely to vote in favor of a climate bill, even if a fair number of state residents called them to say that they supported the bill;
- "Blue states:" States where legislators would be very likely to vote in favor of a climate bill, even if not many state residents called them to say that they supported the bill;
- "Purple states:" States where legislators are on the fence about climate policy, and where legislators could be convinced to vote for a climate bill if they knew that enough of their constituents supported it.

Creating groups of states that fall in each of these categories allows us to test whether Democrats strategically try to mobilize climate action in states where they expect it to have more impact on legislators' choices. To create our ultimate set of geographically close and politically similar state groups, we first elicit Americans' beliefs about which states fall in the climate policy-marginality groups above. We ask a sample of 101 Democrats recruited from Twitter and Facebook to classify each of the 48 states in the contiguous US into these three groups.

We then form clusters of geographically close states among those that most social-media participants classed in the same tier of climate-policy marginality. We vary the number of states in each group to ensure that the projected number of eligible study participants is roughly equal across these groups. In particular, we use Mildenberger et al. (2017)'s estimates to approximate the number of Democrats and Republicans who believe that climate change is mostly human-caused for each state and then sum these values within each state group. By these estimates, our final groups range from about 3 million to 11 million estimated eligible study participants.

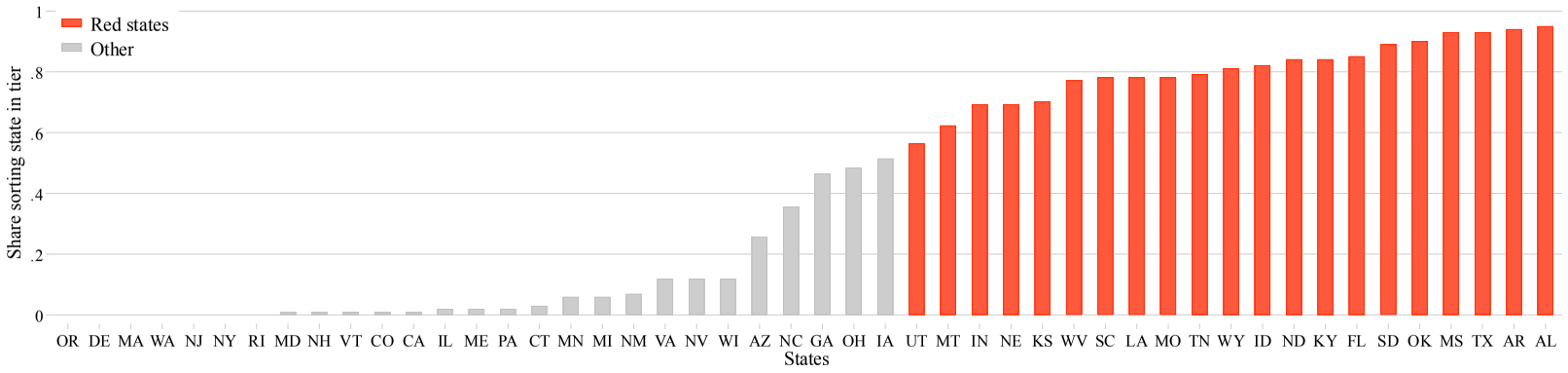
The table below presents our final state groups, classified by the three climate marginality tiers, as well as the percent of eligible Americans that we estimate live in each group, the percent of Democrats in the Wave-1 action experiment sample who live in each group, and the percent of participants (liberal and conservative) in the Wave-2 experimental sample who live in each group. No state group is strikingly over- or under-represented in our experimental samples. As expected, the Wave-1 sample of Democrats is more likely to live in Blue states than the bipartisan Wave-2 sample.

| | % Eligible | % Wave 2 | % Wave 1 |
|---|---|---|---|
| **Red state groups:** | | | |
| South Carolina, Louisiana, Mississippi, Alabama | 6% | 4% | 2% |
| Montana, Wyoming, Idaho, Utah, North Dakota, South Dakota, Nebraska, Kansas, Oklahoma | 4% | 6% | 4% |
| Missouri, Arkansas, Tennessee, Kentucky, Indiana, West Virginia | 9% | 10% | 7% |
| Florida | 7% | 6% | 5% |
| Texas | 8% | 7% | 4% |
| **Blue state groups:** | | | |
| Maine, Vermont, New Hampshire, Massachusetts, Connecticut, Rhode Island | 4% | 8% | 10% |
| New York, New Jersey, Maryland, Delaware | 13% | 10% | 12% |
| Washington, Oregon, California | 14% | 14% | 18% |
| Illinois | 5% | 4% | 4% |
| **Purple state groups:** | | | |
| Ohio, Pennsylvania | 7% | 9% | 9% |
| Virginia, North Carolina, Georgia | 11% | 9% | 8% |
| Wisconsin, Michigan, Minnesota, Iowa | 8% | 9% | 9% |
| Nevada, Arizona, Colorado, New Mexico | 5% | 6% | 6% |

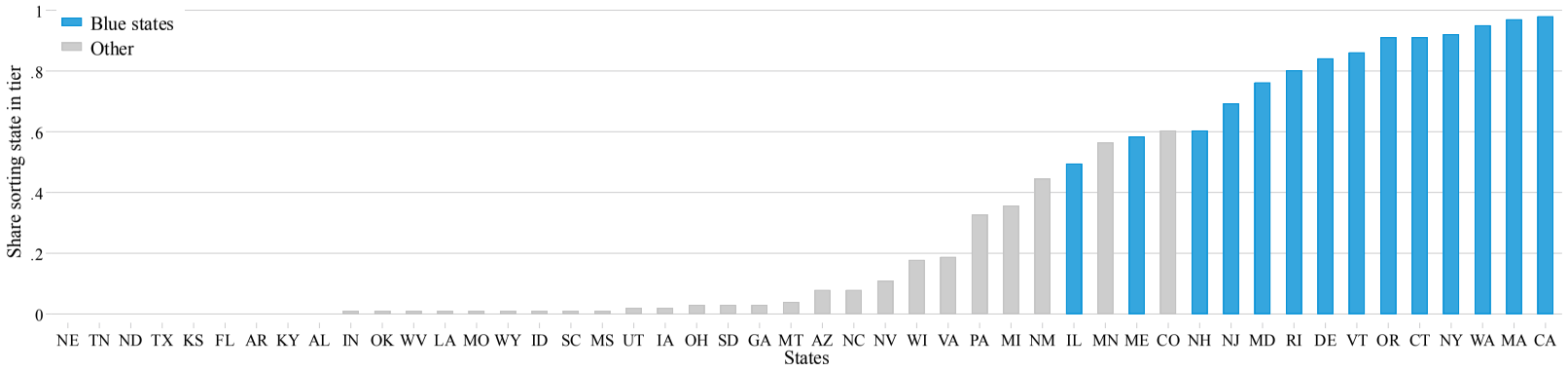
The state groups that we ultimately construct remain quite connected to social media participants' beliefs about state-level climate policy marginality. Figure B43 plots the share of social-media participants who classified a given state as falling in the red, blue, or purple tiers by whether we ultimately assign the state to a state group matched to that tier. In all cases, social media participants were very likely to classify each state in the tier to which their group is ultimately assigned.

Figure B43: Social-media sample responses by ultimate state-group classifications

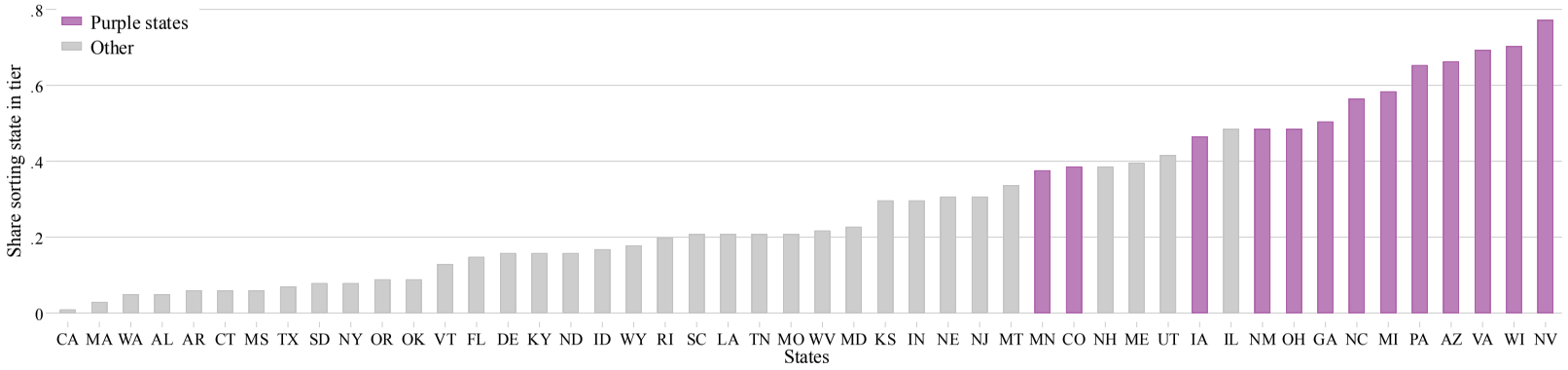
**Panel A.** Red states: Always very unlikely to support climate policy




**Panel B.** Blue states: Always very likely to support climate policy




**Panel C.** Purple states: May or may not support climate policy, depending on citizen support



## B.3 Control variables and screening questions

After first screening out any participant who does not currently live in the US, we elicited all control variables and screening questions in baseline questions at the beginning of the Wave-1 and Wave-2 surveys. We measure these variables as follows:

**Demographic controls and screeners:**

- *Age:* Participants select their age from the following categories: {18-19, 20-24, 25-29, 30-34, 35-39, 40-44, 45-49, 50-54, 55-59, 60-64, 65-69, 70-74, 75-79, 80-84, 85 or older}. We restrict the sample to those between ages 20 and 79. Our main specifications include indicators for each of the remaining age bins.

- *State:* Participants select their current state of residence from a dropdown list of all 50 states and Washington D.C. We restrict the sample to those who live in the contiguous United States to facilitate grouping participants in geographically close and politically similar state clusters. Our main specifications include indicators for each state.

- *Gender:* Participants select their gender from {Man, Woman, Non-binary, Other (with open-response field}. We restricct the sample to those who identify as men or women. Our main specifications include an indicator for identifying as a woman.

- *Education:* Participants select the highest level of education they have completed from the following categories: {Less than high school; High school graduate (including GED); Some college, no degree; Associate's (2-year college) degree; Bachelor's (4-year college) degree; Master's degree; Post-bachelor professional degree (MD or JD) or doctorate (PhD)}. Our main specifications include indicators for each attainment level.

- *Income:* Participants select their total household income before taxes in the last 12 months from the following categories: {Less than $25,000; $25,000-$49,999; $50,000-74,999; $75,000-$99,999; $100,000-$149,999; $150,000-$199,999; $200,000 or more}. Our main specifications include indicators for each income category.

- *Race:* Participants select the racial group with which they most strongly identify from the following categories: {Black / African American; Native American; Asian or Pacific Islander; White; Multiracial (with open-response field); Other (with open-response field)}. We restrict the sample to those who identify as white.

- *Ethnicity:* Participants report whether they identify as Hispanic or Latino or not. Our main specifications include an indicator that participants answer yes on this variable.

**Climate-belief controls and screeners:**

- *Beliefs about the causes of climate change:* We ask participants to choose which of the following statements they most agree with:

- Climate change is caused mostly by human activites.
- Climate change is caused mostly by natural changes in the environment.
- Neither, since climate change isn't happening.
- Other (with open-response field).

We screen out participants who do not say that climate change is caused mostly by human activities.

- *Climate worry:* "How worried are you about climate change?" Participants select an integer response from 1 (Not at all worried) to 7 (Extremely worried). We standardize this variable to have mean zero and standard deviation 1 in the Wave-1 and Wave-2 experimental samples; we include this control in our main specifications.

- *Desire for climate action:* "How much do you want the federal government to do to slow or stop climate change, relative to what it's currently doing?" Participants select an integer response from 1 (Much less) to 4 (The same as it's currently doing) to 7 (Much more). We standardize this variable to have mean zero and standard deviation 1 in the Wave-1 and Wave-2 experimental samples; we include this control in our main specifications.

- *Perceived local climate impacts:* "How much would you say you are currently seeing the effects of climate change in your local area, like changes in weather patterns or natural disasters?" Participants select an integer response from 1 (Not at all) to 7 (To an extremely high degree). We standardize this variable to have mean zero and standard deviation 1 in the Wave-1 and Wave-2 experimental samples; we include this control in our main specifications.

**Political controls and screeners:**

- *Political affiliation:* We elicit participants' political affiliation in two steps.

  1. First, we ask participants whether, generally, speaking they would say they lean towards the Democratic Party, the Republican Party, or neither. (We identify "liberals" and "conservatives" as those who say they lean towards the Democratic or Republican Parties, respectively.)
  2. If participants chose either the Democratic or Republican Parties, we then ask them the following: "Would you consider yourself a member of the [Democratic/Republican] Party, an Independent, or something else?"

  In the Wave-1 sample, we restrict participants to those who say that they are members of the Democratic Party. In the Wave-2 sample, we restrict participants to those who say they lean towards either the Democratic or Republican Parties.

- *Political efficacy:* We elicit participatnts' agreement with the following statements from 1 (Strongly disagree) to 7 (Strongly agree):

  - People like me don't have any say about what the federal government does about issues like climate change;

- Fossil fuel companies and their lobbyists have more power than citizens in determining what the US government does about climate change;
- When groups of citizens push for policy on issues like climate change; the US government responds to thier demands.

We standardize these variables to have mean zero and standard deviation 1 in the Wave-1 and Wave-2 experimental samples. We then construct an index as the sum of these standardized variables, flipping the sign of agreement with the first and second statements. We then standardize this sum to have mean zero and standard deviation one in the Wave-1 and Wave-2 experimental samples and control for this index in our main specifications.

- *Baseline political engagement:* We elicit participants' baseline political engagement with the following framing: "Some people get directly involved in social and political issues, while others don't have the time or interest. In the last two years, have you done any of the following things? (In other words, since June 2021). Please select all that apply:
  - Emailed elected representatives about a political or social issue
  - Donated money to an organization working on a social or political issue
  - Canvassed door-to-door about a political or social issue
  - Signed a petition about a political or social issue
  - Phone-banked for a political or social issue
  - Phoned elected representatives about a political or social issue"

  We create an index for political engagement by standardizing indicators for each of the above to have mean zero and standard deviation one, adding these together, and then standardizing the sum to have mean zero and standard deviation one in the Wave-1 and Wave-2 experimental sample.

# C Wave-2 Appendix

## C.1 Wave-2 recruitment

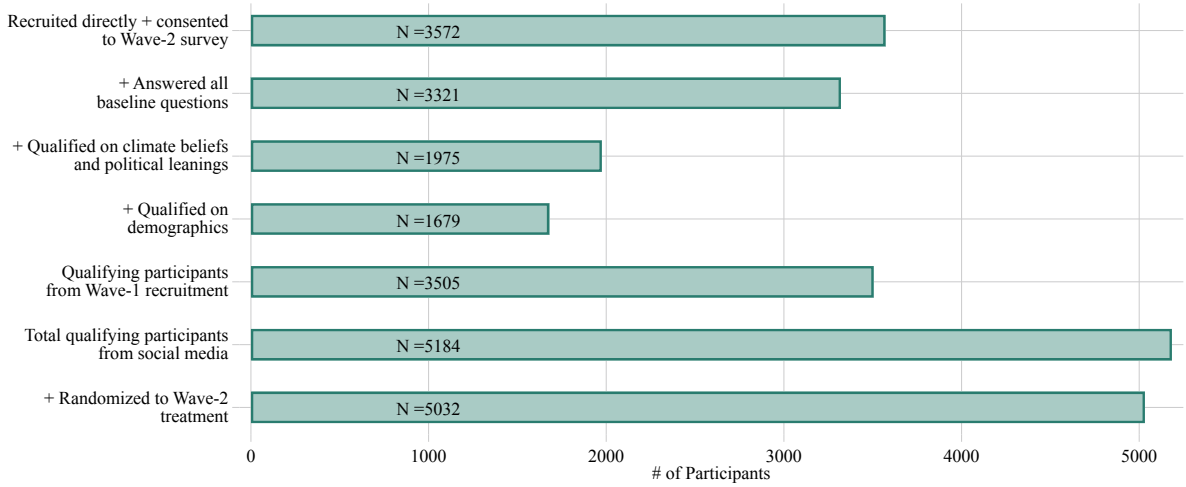
While we recruited most Wave-2 liberals from social media, we recruited nearly all Wave-2 conservatives directly from Qualtrics, which aggregates a range of online panels.

**Social-media recruitment:** We recruited Wave-2 participants via social media in two ways (Panel A of Appendix Figure B44). First, we redirected participants to Wave 2 if they initially started but were ineligible for the Wave-1 survey because they were not members of the Democratic party. Any such participant had completed the full suite of baseline questions on demographics, climate and political beliefs, and political engagement in the Wave-1 survey, and they were redirected to the Wave-2 survey at the point of constructing a basic avatar of themselves. In total, 3,505 participants started the Wave-2 survey via this route.

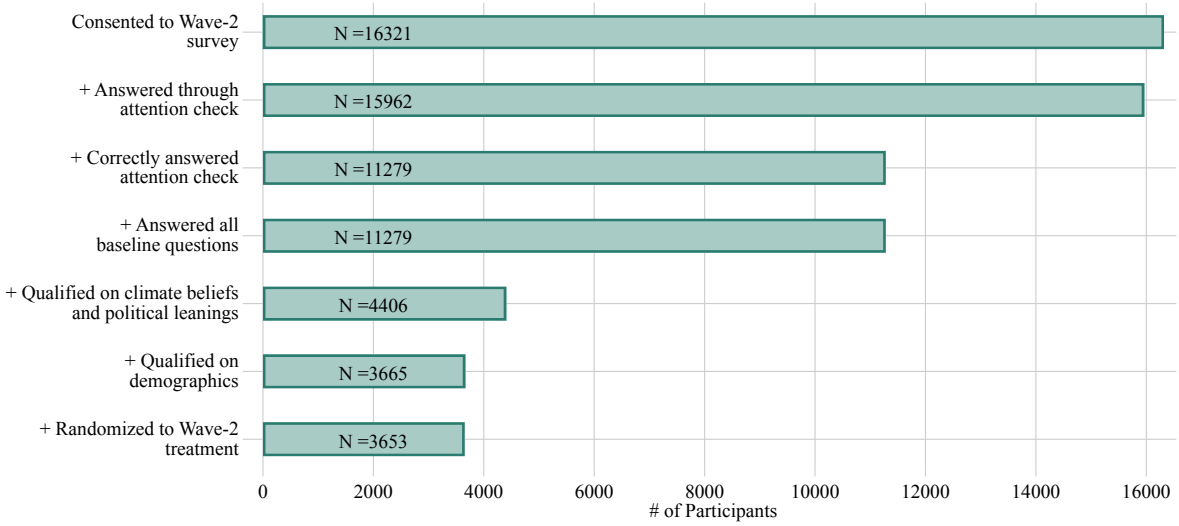
Other participants were initially recruited from social media directly to the Wave-2 Qualtrics survey. In total, 3,572 unique participants from social media consented directly to the Wave-2 survey and provided

Figure B44: Wave-2 recruitment

**Panel A.** Recruitment from social media




**Panel B.** Recruitment from Qualtrics




an email address, which we required of all participants in order to link them with records of emailing Congress. Of these participants, 3,321 participants completed the full suite of baseline questions and 1,975 state that they believe that climate change is mostly human-caused and lean towards either the Republican or Democratic party. We then impose the feasibility-based screening criteria (Section 2.2), restricting to participants who live within the contiguous United States, are between age 20 and age 79, identify as a man or a woman, and identify as White. These restrictions leave 1,679 qualifying participants recruited directly to the Wave-2 survey, or 85% of those who qualify for Wave-2 on politics and climate beliefs and 51% of those who completed the baseline screening questions.

The full sample of N = 5,184 qualifying participants recruited from social media (either by way of the Wave-1 experiment or directly to Wave 2) then build an avatar, and the remaining 5,032 participants recruited from social media are randomized to a Wave-2 treatment group. This sample is highly skewed

towards liberals: only 211 participants recruited via social media and randomized to a Wave-2 treatment are conservative

**Qualtrics recruitment:** While we recruited most liberals in the Wave-2 sample from social media, we recruited nearly all conservatives in the Wave-2 sample directly from Qualtrics, which aggregates respondents from over 20 partnering market-research panels and other online samples. Participants recruited via Qualtrics were subject to identical screening criteria on demographics, political affiliation, and climate beliefs as Wave-2 participants recruited from social media (Panel B of Appendix Figure B44).

In total, 16,321 unique participants recruited via Qualtrics consented to the Wave-2 survey and provided an email address. Of these, 16,058 completed baseline questions through stating their beliefs about the drivers of climate change. In the survey fielded for Qualtrics recruitment, participants then answered a basic attention check question: 15,962 participants answered the attention check, and 11,279 (71%) did so correctly. These participants then reported their political affiliations, leaving 4,406 participants who state that they believe that climate change is mostly human-caused and lean towards either the Republican or Democratic party. Imposing the same demographic restrictions as above leaves 3,665 qualifying participants. These remaining participants are asked to complete a simple pledge to provide thoughtful and honest survey answers, and 3,652 do so. This remaining sample then answers additional baseline survey questions, build their basic avatar, and are randomized into Wave-2 treatment arms.

By construction, the sample recruited via Qualtrics is heavily conservative: 2,791 participants randomized to a treatment arm lean towards the Republican party, while 862 lean towards the Democratic party.

## C.2 Wave-2 set-up details

### C.2.1 Randomization to Wave-1 profiles

As in Wave 1, Wave-2 participants begin their survey by answering a series of questions on their demographics, political engagement, and climate beliefs, which we then combine in a basic demographic profile (Appendix Figure A6). From participants' perspective, the next step is to show them the basic demographic profile of a randomly-chosen Wave-1 Democrat. If Wave-2 participants later see a profile inviting them to join in emailing Congress, it will depict an invitation from this same Wave-1 match.

Before we show Wave-2 participants the basic demographics of their randomly-paired Wave-1 match, we randomly assign each participant to a treatment status: whether they ultimately see an invitation to join an earlier participant in emailing Congress or not. We perform this randomization at this point because participants' treatment status determines the earlier Wave-1 participant to which they can be randomly paired. In particular, we must truthfully carry out what we told each Wave-1 participant about whether and to whom their demographic profile, and in some cases extra climate-action profiles, will be shown. Thus, any Wave-2 participant assigned to see a profile inviting them to join in emailing Congress must be paired with a Wave-1 participant from the A2 (Invitation) group who chose to email Congress.[50] (See Section 5 for details on this treatnennt group.) Wave-2 participants assigned not to see an invitation to join in action can be paired with any other Wave-1 participants.

[50]Note that we cannot show a profile of an A0 participant who emailed Congress because these participants are invited to complete a separate sub-experiment, described in Section 6, in which we tell participants that we will show profiles saying that they emailed Congress only to specific returning participants.

Wherever possible, we randomize Wave-2 participants to be matched with a Wave-1 participant from their paired demographic group (Section 2.2) for whom an action invitation can or cannot be shown, depending on the Wave-2 participant's treatment status. In some cases, Wave-2 participants' matched Wave-1 demographic groups are small enough that there are no Wave-1 participants to whom they can be paired; recall that these demographic groups are defined by the interactions of gender, 15-year age bins, whether participants have a 4-year college degree, and 13 groups of geographically close and politically similar states. In these cases, we randomly pair Wave-2 participants with an earlier participant from a small sample of Democrats recruited alongside Wave 1 that we refer to as "Wave-1 blanks." We told all of these participants that we would show future participants invitations to join them in emailing Congress, if they did so, without specifying the demographics of these future participants. (We do not include these Wave-1 blanks in our Wave-1 experimental analysis.) 49% of Wave-2 participants are paired with a Wave-1 participant from their matched demographic group, while the remaining participants are paired with a Wave-1 blank.

#### C.2.2 Wave-2 comprehension questions

At the end of the Wave-2 experimental survey, we randomize between two sets of comprehension questions: multiple-choice recall questions about their paired Wave-1 participant's traits and multiple-choice questions about the survey set-up and their match's action. All of these questions are incentivized: we tell participants that we will randomly choose 10 people to win a gift card with $5 for each question they answer correctly.

**Questions about Wave-1 match's traits:** We randomize half of Wave-2 participants to answer multiple choice questions about their paired Wave-1 match. We lead in to these questions as follows: "Earlier in this survey, you might remember that we showed you a profile for someone who took a similar survey earlier. We'd like to check what you remember about that participant." Then, we randomize participants either to answer multiple-choice questions about matches' gender, age group, and political party or about whether they have a 4-year college degree and the state group in which they live.

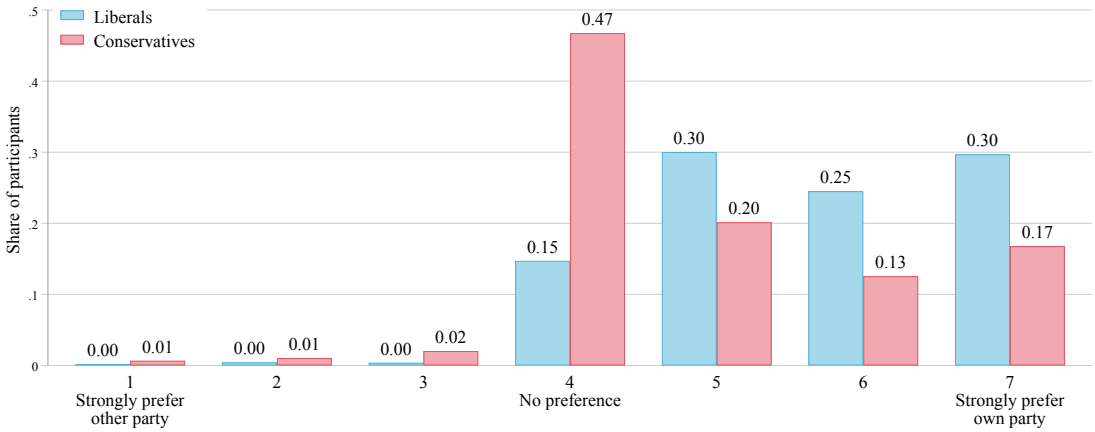
**Questions about the survey set-up:** We randomize half of Wave-2 participants to answer multiple choice questions about the survey set-up and whether their paired Wave-1 match emailed Congress, as follows:

1. "Earlier in the survey, you might remember that we showed you a profile for someone who took a similar survey earlier. Do you know whether that past participant emailed Congress while taking our survey or not?" [Yes, they did; No, they didn't; I don't know what they did]

2. If participants answer that they don't know what their paired match did, we then ask: "This question doesn't have a correct answer; we're just looking for your guess. What would you guess is the probability that the past participant we showed you emailed Congress via our form?" [Answered on a scale from 0% (Definitely not) to 100% (Definitely yes)]

3. We then ask all participants: "Will any future participants be told whether or not you contacted Congress?" [Yes; No; I don't know]

Figure B45: How much would you prefer being friends with someone from one of the major political parties versus the other?




### C.2.3 Measuring affective polarization in Wave 2

We use several measures of affective polarization in the Wave-2 survey.

**Preferences for friends of own party:** For our simplest measure of affective polarization, we ask all participants to report in a baseline question how strongly they would prefer being friends with someone from one of the major political parties versus the other. We elicit responses on a 7-point scale, where 4 denotes "No preference" and the endpoints are randomized between "Strongly prefer Republican" and "Strongly prefer Democrat." To obscure our focus on partisan polarization, we ask this question in a series where we also ask participants to rate their preference to be friends with someone who is either very religious or not and who has varying degrees of education. Figure B45 plots the distribution of these preferences among liberal and conservative Wave-2 participants. Liberals in our sample show substantially higher affective polarization than conservatives on this measure. One key limitation of this measure is that conservatives' responses are highly lumped at the middle of the scale; only 111 conservatives offer a below-median response, limiting our ability to test for heterogeneous treatment effects by this measure. To isolate more granular variation in these preferences separate from other participants' other traits, we also test estimate the residuals of this measure when regressed on demographics, climate beliefs, and political engagement, separately by political party.

**Polarization feelings thermometer:** Towards the end of the Wave-2 experimental survey, we measure affective polarization using the "feelings thermometer" measured over time in the American National Election Study (Iyengar et al. 2019). In this classic measure of affective polarization, participants rate how warmly they feel towards each political party on a scale from 0 to 100 degrees; we then calculate affective polarization as the gap in warmth they feel towards their own versus the out party. We elicit this measure at the end of the survey, rather than before our experimental treatments, in order to minimize the salience of partisanship. While doing so opens the risk that the treatments might actually affect participants' responses to this measure, we find no evidence of treatment effects on these measures (Appendix Table B35).

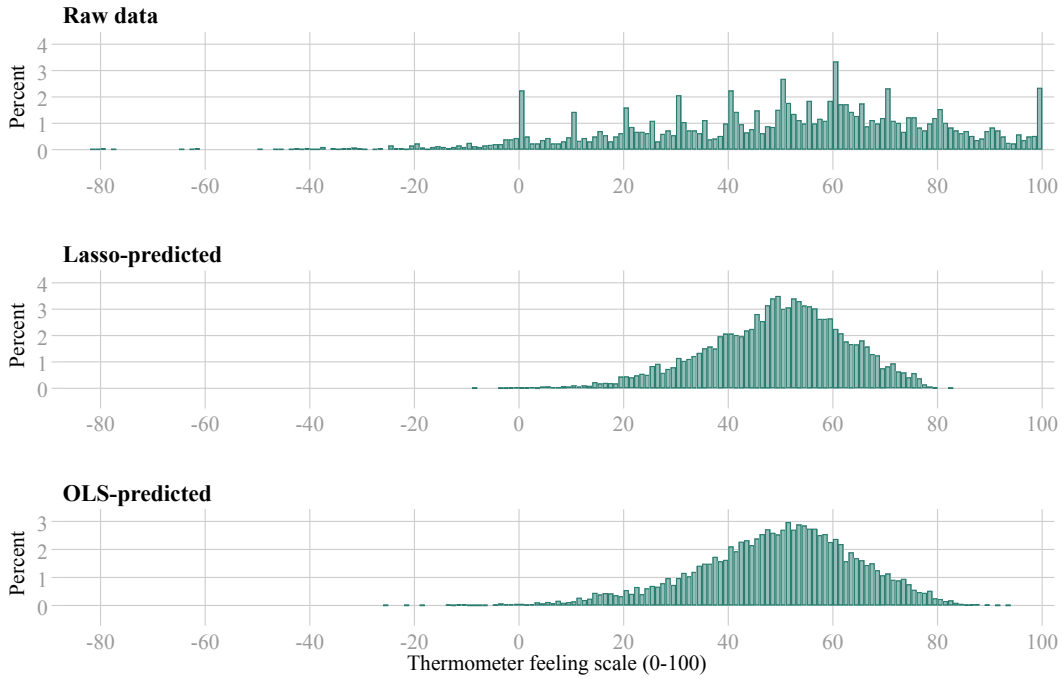
We only observe this thermometer-based polarization measure for a random half of the Wave-2 survey. We

Table B35: Impacts on Wave-2 thermometer values

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Warmth for Dems | | Warmth for Reps | | Own-party gap | |
| | Dem | Rep | Dem | Rep | Dem | Rep |
| Treatment | 0.847 | 0.558 | 0.963 | 0.853 | -0.116 | 0.295 |
| | (0.657) | (1.192) | (0.665) | (1.027) | (0.886) | (1.723) |
| Control mean | 67.929 | 31.282 | 12.968 | 71.180 | 54.962 | 39.898 |
| N | 2360 | 1467 | 2360 | 1467 | 2360 | 1467 |
| *p-val:* | 0.827 | | 0.926 | | 0.827 | |

thus also use OLS and lasso to predict values for this measure in the full sample. Appendix Figure B46 plots the distributions of these variables. Both OLS and lasso predict our observed thermometer polarization measures reasonably well: the lasso- and OLS-predicted measures have correlation coefficients of 0.52 and 0.53 with the observed measure, respectively.

Figure B46: Observed and predicted Wave-2 thermometer polarization measures




## C.3 Why do invitations affect action?

Why do invitations from Wave-1 participants increase action? To understand these mechanisms, we test the impacts of treatment on a range of secondary outcomes that we collected after participants choose whether to email Congress or not; to reduce the survey length, we randomize participants between two sets of secondary outcomes.

### C.3.1 Beliefs about the impact of emailing Congress

First, seeing that an earlier participant emailed Congress and passed on an invitation to join may shift Wave-2 participants' action by sending a signal that emails to Congress are worthwhile (Table B36, column 1). We ask participants to report on a scale from 1 (A total waste of time) to 7 (Extremely impactful) how impactful it would be for them to email their Senators, assuming that 30 other survey-takers from their state were also doing so; conditioning these beliefs on a fixed amount of participation by others ensures that effects here do not arise from treatment effects on participants' beliefs about others' political participation coupled with beliefs about strategic complementarity or substitutability. Seeing that an earlier participant emailed Congress and invited them to join increases participants' perceptions of emails' impact by 0.08sd. While our point estimate for this impact is larger among liberals, we cannot reject that the effects on liberals and conservatives are equal (columns 1 and 2, Appendix Table B37).

Table B36: Wave-2 results on beliefs about others' participation

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | | *Descriptive norms* | | | | *Injunctive norms* | |
| | How worthwhile emails are | % Others emailed | % Americans who: Think govt should act | % Americans who: Would call for bill | Prob. climate bill would pass | # Agree that: Advocating for climate policy is right: Of 20 friends | # Agree that: Advocating for climate policy is right: Of 100 Amer. |
| Treatment | 0.077*** | 7.483*** | -0.399 | 0.302 | 0.229 | -0.034 | -0.347 |
| | (0.029) | (0.695) | (0.527) | (0.598) | (0.614) | (0.080) | (0.666) |
| Control mean | -0.000 | 35.997 | 62.565 | 25.642 | 40.323 | 5.878 | 49.096 |
| N | 3868 | 3872 | 3831 | 3822 | 3841 | 3821 | 3817 |

Table B37: Wave-2 results on beliefs about others' participation: Split by party

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | % Others participating | | % Americans who: Think govt should address climate | | % Americans who: Would call to support climate bill | | Prob. that a climate bill would pass | |
| | Dem | Rep | Dem | Rep | Dem | Rep | Dem | Rep |
| Treatment | 8.408*** | 6.454*** | -0.281 | -0.181 | 0.184 | 0.635 | 0.725 | -0.810 |
| | (0.893) | (1.147) | (0.628) | (0.941) | (0.713) | (1.069) | (0.791) | (0.990) |
| Control mean | 34.283 | 38.923 | 61.462 | 64.286 | 22.412 | 30.668 | 32.436 | 52.655 |
| N | 2404 | 1468 | 2353 | 1478 | 2345 | 1477 | 2362 | 1479 |
| *p-val:* | 0.169 | | 0.928 | | 0.719 | | 0.214 | |

Table B38: Wave-2 results on perceived norms: Split by party

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | How worthwhile emails are | | # Agreeing that: Helping to advocate for climate policy is the right thing to do. | | | |
| | | | Of 20 friends | | Of 100 Americans | |
| | Dem | Rep | Dem | Rep | Dem | Rep |
| Treatment | 0.100*** | 0.042 | -0.089 | 0.009 | -0.915 | 0.641 |
| | (0.037) | (0.045) | (0.103) | (0.128) | (0.830) | (1.133) |
| Control mean | -0.090 | 0.154 | 6.283 | 5.248 | 45.929 | 54.015 |
| N | 2400 | 1468 | 2344 | 1477 | 2340 | 1477 |
| *p-val:* | 0.306 | | 0.543 | | 0.256 | |

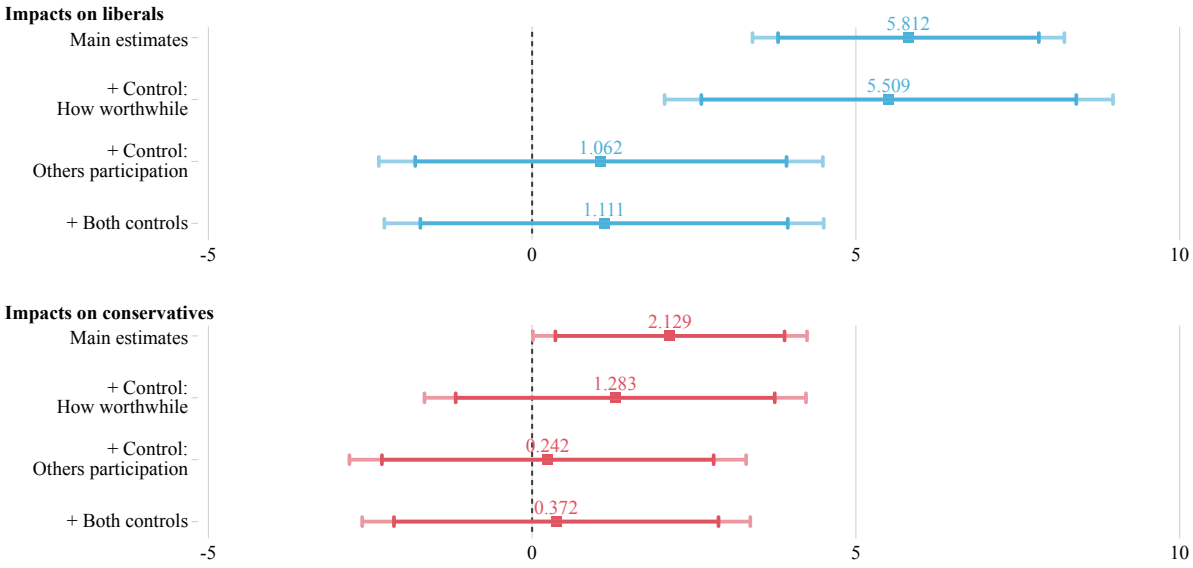
### C.3.2 Perceptions of others' action and social norms

Next, seeing that an earlier participant emailed Congress could affect action by shifting Wave-2 participant's beliefs about others' beliefs about climate change and engagement in climate action, either just within the study sample or in the US more broadly. The impact of shifting these beliefs is theoretically ambiguous. While social psychologists have long documented convergence to two classes of social norms: "descriptive norms"—what is typically done—and "injunctive norms"—what ought to be done (Miller and Prentice 2016; Rogers et al. 2018; Gerber et al. 2018), growing research in economics finds that shifting up beliefs about anonymous others' political participation can reduce engagement in collective political action in settings where people perceive strategic substitutability across actors (Cantoni et al. 2019, Hager et al.).

Columns 2 through 5 of Table B36 present the impacts of seeing a Wave-1 invitation on Wave-2 participants' perceived descriptive norms of political climate advocacy. Seeing an invitation increases participants' estimates for the share of other survey participants who emailed Congress via our form by 7.5 pp, or about 21% of the control mean; liberals and conservatives show similar patterns of updating (columns 1 and 2, Appendix Table B37) On the other hand, participants do not seem to extrapolate this signal to beliefs about Americans' political activity at large: seeing an invitation has no effect on participants' beliefs about the share of Americans who would say they think the US government should address climate change (column 3), the share of Americans who would contact Congress to support a climate bill if it were introduced in September 2023 (column 4), or the probability that such a climate bill would pass (column 5). We also find no impact on the injunctive norms about the ethics of climate advocacy that participants perceive among their friends or Americans at large (columns 6 and 7).

Thus, seeing a Wave-1 invitation shifts up participants' beliefs about others' action precisely during the survey—without extrapolation to broader climate action—but it is unclear whether this mechanism increases or decreases the invitations' effects on action.

Figure B47: Correlational role of Wave-2 mechanisms




### C.3.3 Combining mechanisms

Appendix Figure B47 provides suggestive evidence on the contribution of each of these possible mechanisms to the Wave-2 treatment effects among liberals and conservatives. In particular, we estimate the treatment effects of invitations when we control for participants' beliefs about how worthwhile it is to email Congress, their beliefs about how many others are also emailing Congress during our survey, and both of these intermediate outcomes. While controlling for participants' beliefs about the value of emailing Congress reduces the impact of invitations somewhat among conservatives, it has very little impact on our estimated treatment effect among liberals. In contrast, controlling for participants' beliefs about others participation substantially reduces the treatment effects we estimate both among liberals and conservatives. While this correlational decomposition is only suggestive, it is consistent with invitations primarily working by changing participants' perceived norms of political action during the survey.

## C.4 Attrition assumption in robustness check

One of the Wave-2 robustness checks that we conduct in Section 8.1 is to show that our main results hold when we assume that those who leave the experimental survey after the email preview but before choosing whether to email Congress or not would not have done so. (We impose this assumption in our main specifications to deal with differential attrition in Wave 1.) As in Wave 1, this assumption appears to be justified in Wave 2. The small sample of participants in the main Wave-2 sample for whom we do not observe explicit email choices are more likely to be under 40, less likely to be over 70, express less concern about climate change, and are less politically engaged, though some of these gaps are not statistically significant (Appendix Table B40). In the Wave-2 control group, many of these patterns correlate with lower likelihood of explicitly choosing not to email Congress (column 2 of Appendix Table A12).

However, we do not make this assumption in our main Wave-2 results because it actually induces dif-

ferential attrition by treatment arm. In Wave 1, all participants randomized to a treatment group had already seen the email-form preview, so this assumption allows us to retain all randomized participants in the main Wave-1 experimental sample. In contrast, our Wave-2 approach opens up the possibility of differential selection into the main Wave-2 experimental sample. This attrition could arise simply from participants leaving the survey between randomization and the email preview, but it also arises from an attention check question that was asked of participants recruited via social media after randomization and just before the email preview. In the social-media sample, treatment participants are 1.9 pp more likely to answer this attention check correctly (column 6, Panel A, Appendix Table A2), despite the fact the two treatment groups' surveys have not diverged by this point. Overall, treatment participants are 1.4 pp more likely to see the email preview (column 3, Panel A, Appendix Table A2), driven entirely by participants recruited from social media (column 4, Panel A, Appendix Table A2).

However, we find no evidence for differential selection on observable traits in attrition across treatment arms. Appendix Table B39 compares participants who see the email preview separately among the treatment and control groups. While those who attrit differ substantially from those who do not in both treatment groups, we cannot reject that the attriters are identical across treatment groups on nearly every trait. Moreover, as we note above, there are no substantive differences in the treatment versus control surveys to this point, so there is no obvious mechanism that could drive differential selection. Nevertheless, for simplicity we restrict the main Wave-2 analysis to those who remained in the survey through choosing whether to email Congress or not.

Table B39: Comparing Wave-2 participants who do and do not see email preview

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | **Control group:** | | | **Treatment group:** | | | | |
| | See email preview | Δ Attrit | *(p-val)* | See email preview | Δ Attrit | *(p-val)* | **Among attrited** Δ Treat | *(p-val)* |
| Woman | 0.521 | 0.172*** | (0.000) | 0.521 | 0.161*** | (0.000) | 0.015 | (0.745) |
| Hispanic | 1.079 | 0.017 | (0.191) | 1.075 | 0.012 | (0.391) | -0.000 | (1.000) |
| Has $\geq$ 4-year college degree | 0.588 | 0.038 | (0.190) | 0.588 | 0.048 | (0.146) | -0.022 | (0.601) |
| **Age ranges:** | | | | | | | | |
| 20-39 | 0.311 | 0.061** | (0.011) | 0.309 | 0.095*** | (0.000) | -0.039 | (0.224) |
| 40-59 | 0.322 | 0.050* | (0.096) | 0.309 | -0.006 | (0.864) | 0.042 | (0.329) |
| 60-79 | 0.367 | -0.111*** | (0.001) | 0.383 | -0.089** | (0.016) | -0.003 | (0.948) |
| **Income bins (USD):** | | | | | | | | |
| Less than 50,000 | 0.301 | -0.012 | (0.668) | 0.308 | 0.014 | (0.641) | -0.015 | (0.701) |
| 50,000-99,999 | 0.350 | 0.060** | (0.046) | 0.336 | -0.013 | (0.710) | 0.072 | (0.102) |
| 100,000-149,999 | 0.182 | -0.042 | (0.148) | 0.186 | 0.019 | (0.527) | -0.065 | (0.105) |
| 150,000-199,999 | 0.079 | -0.022 | (0.317) | 0.091 | 0.010 | (0.664) | -0.030 | (0.318) |
| 200,000 or more | 0.087 | 0.016 | (0.467) | 0.080 | -0.029 | (0.283) | 0.037 | (0.263) |
| **Residence by state marginality:** | | | | | | | | |
| Red state | 0.331 | -0.015 | (0.629) | 0.328 | 0.014 | (0.671) | -0.030 | (0.486) |
| Blue state | 0.353 | 0.038 | (0.235) | 0.353 | -0.038 | (0.304) | 0.065 | (0.167) |
| Purple state | 0.316 | -0.023 | (0.472) | 0.319 | 0.024 | (0.480) | -0.036 | (0.414) |
| **Climate beliefs:** | | | | | | | | |
| Climate worry (1-7) | 5.758 | 0.188** | (0.022) | 5.738 | 0.273*** | (0.005) | -0.144 | (0.246) |
| Desire for climate action (1-7) | 6.040 | 0.285*** | (0.000) | 6.072 | 0.314*** | (0.000) | -0.032 | (0.785) |
| Perceived local impacts (1-7) | 5.237 | -0.018 | (0.872) | 5.220 | 0.215* | (0.086) | -0.215 | (0.194) |
| **Political affiliation:** | | | | | | | | |
| Republican | 0.297 | -0.031* | (0.053) | 0.305 | -0.006 | (0.668) | -0.027 | (0.199) |
| R-leaning Independent | 0.107 | -0.017 | (0.191) | 0.101 | -0.024 | (0.134) | 0.007 | (0.727) |
| L-leaning Independent | 0.368 | 0.031 | (0.317) | 0.368 | -0.010 | (0.769) | 0.029 | (0.510) |
| Democrat | 0.228 | 0.017 | (0.558) | 0.226 | 0.040 | (0.211) | -0.009 | (0.822) |
| **Other political beliefs:** | | | | | | | | |
| Political engage. index (std) | 0.034 | 0.756*** | (0.004) | 0.049 | 0.424 | (0.137) | 0.258 | (0.480) |
| Prev. contacted reps | 0.277 | 0.041 | (0.172) | 0.287 | 0.080** | (0.012) | -0.030 | (0.465) |
| Prev. donated | 0.496 | 0.113*** | (0.000) | 0.502 | 0.053 | (0.130) | 0.058 | (0.198) |
| Prev. canvassed | 0.055 | 0.011 | (0.463) | 0.058 | -0.031 | (0.159) | 0.049* | (0.051) |
| Prev. signed petition | 0.601 | 0.126*** | (0.000) | 0.599 | 0.091*** | (0.007) | 0.020 | (0.650) |
| Prev. phonebanked | 0.070 | -0.003 | (0.875) | 0.065 | -0.020 | (0.363) | 0.009 | (0.748) |
| Political efficacy index (std) | -0.009 | -0.029 | (0.421) | 0.004 | -0.063* | (0.097) | 0.061 | (0.223) |
| Prefer friend of own party (1-7) | 5.383 | 0.238*** | (0.004) | 5.354 | 0.179** | (0.037) | 0.013 | (0.909) |
| Sample size | 3605 | 252 | | 3632 | 198 | | | |

Add note here that we're missing perceptions of local climate impacts for some people.

Table B40: Comparing Wave-2 participants for whom we observe emails or not, among those who saw email preview

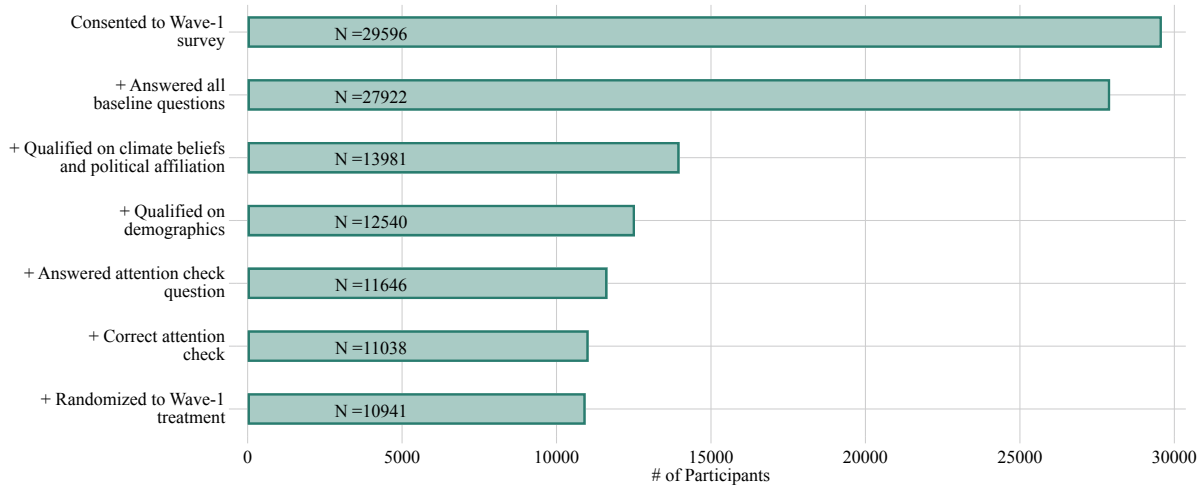
| | (1) Mean: Observe if email | (2) Δ Not observe if email | (3) *(p-value)* |
|---|---|---|---|
| Woman | 0.523 | 0.064 | (0.137) |
| Hispanic | 1.077 | -0.016 | (0.446) |
| Has $\geq$ 4-year college degree | 0.585 | 0.045 | (0.236) |
| **Age ranges:** | | | |
| 20-39 | 0.314 | 0.109*** | (0.000) |
| 40-59 | 0.315 | 0.005 | (0.901) |
| 60-79 | 0.371 | -0.114*** | (0.007) |
| **Income bins (USD):** | | | |
| Less than 50,000 | 0.305 | -0.044 | (0.234) |
| 50,000-99,999 | 0.345 | 0.068* | (0.066) |
| 100,000-149,999 | 0.184 | 0.018 | (0.597) |
| 150,000-199,999 | 0.084 | -0.022 | (0.448) |
| 200,000 or more | 0.082 | -0.020 | (0.519) |
| **Residence by state marginality:** | | | |
| Red state | 0.329 | -0.067* | (0.094) |
| Blue state | 0.352 | 0.015 | (0.721) |
| Purple state | 0.319 | 0.052 | (0.171) |
| **Climate beliefs:** | | | |
| Climate worry (1-7) | 5.736 | -0.119 | (0.108) |
| Desire for climate action (1-7) | 6.044 | -0.045 | (0.489) |
| Perceived local impacts (1-7) | 5.223 | -0.088 | (0.334) |
| **Political affiliation:** | | | |
| Republican | 0.306 | -0.004 | (0.739) |
| R-leaning Independent | 0.105 | 0.000 | (1.000) |
| L-leaning Independent | 0.362 | 0.007 | (0.861) |
| Democrat | 0.226 | -0.003 | (0.937) |
| **Other political beliefs:** | | | |
| Political engage. index (std) | -0.000 | -0.386 | (0.311) |
| Prev. contacted reps | 0.281 | -0.032 | (0.424) |
| Prev. donated | 0.495 | 0.046 | (0.250) |
| Prev. canvassed | 0.056 | -0.014 | (0.543) |
| Prev. signed petition | 0.596 | -0.007 | (0.837) |
| Prev. phonebanked | 0.066 | -0.061** | (0.035) |
| Political efficacy index (std) | 0.000 | 0.005 | (0.913) |
| Prefer friend of own party (1-7) | 5.363 | 0.066 | (0.463) |
| Sample size | 7094 | 143 | |

# D Wave-1 Appendix

## D.1 Recruitment details

We recruit Wave-1 participants using ads on Facebook, Instagram, and Twitter. In total, 29,596 unique participants consented to the survey and provided an email address, which we required of all participants in order to link them with records of emailing Congress. Of these, 27,922 answered all baseline questions and 13,981 participants met our criteria that Wave-1 participants believe climate change is mostly human-caused and identify as members of the Democratic party. We then impost the demographic screening criteria (Section 2.2), leaving us with a sample of 12,540 participants.

Figure B48: Wave-1 recruitment




Qualifying participants then build their avatar, see that up to 10 Wave-2 participants may see their basic demographic profile, answer additional questions about their beliefs on climate policy, and answer a simple attention check. 11,646 participants answered the attention check and 11,038 did so correctly. We inform those who passed the attention check about the upcoming opportunity to email Congress during our survey, and finally randomize 10,941 participants into several samples associated with the Wave-1 experiment. 8,937 were randomized into the Wave-1 experimental sample, and the remaining 2,004 participants were randomized into parallel experimental samples described in Sections 6 and 7 below.

## D.2 Wave-1 set-up details

### D.2.1 Text of the A1 and A2 treatments

Appendix Figure B49 first shows how we initially introduce both the Invitation (A2) and Tell-after (A1) treatments to Wave-1 participants. Next, Appendix Figures B50 and B51 show the survey screens that Tell-after (A1) and Invitation (A2) participants see describing that future participants in their paired Wave-2 demographic group will see that they emailed Congress. Finally, B52 shows the flowcharts that we then show to participants to reiterate the timing of when future participants will be told that they emailed

Congress. Note that the flowcharts fill in the demographic profiles of each Wave-1 participant's paired Wave-2 demographic group.

Figure B49: Introduction to the A1 and A2 treatments

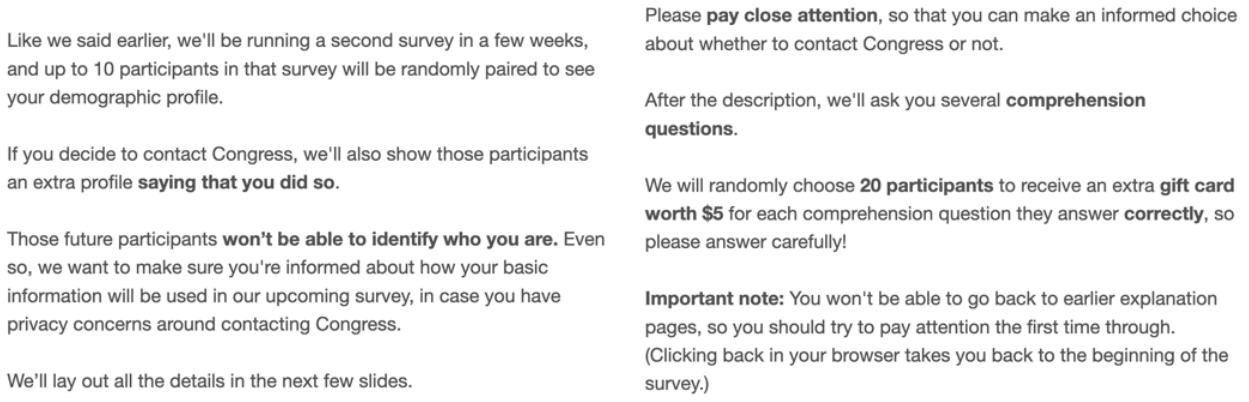
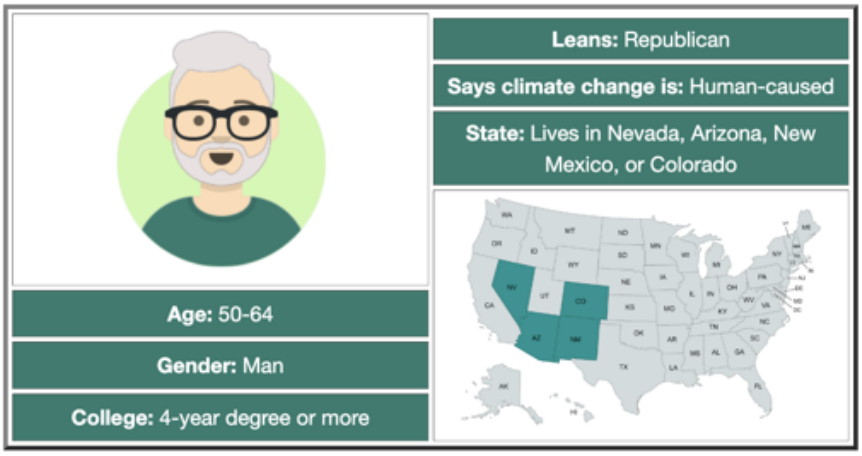


Figure B50: A1 (Tell after) treatment description

Remember that up to 10 participants in the demographic group below will see your own demographic profile when they take our survey.

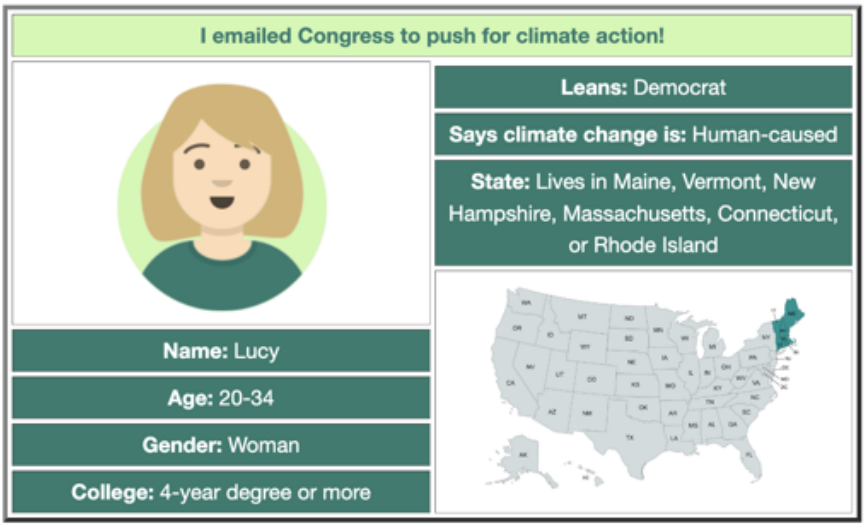
Besides showing these future participants your basic profile, we will also tell them **if you contact Congress, after they decide** whether to contact Congress or not.




---

In particular, if you choose to contact Congress, we'll show these participants the following extra profile of you **after** they choose whether or not to take action.

The profile will say that you contacted Congress:




That means that they **won't know** you contacted Congress **when they decide** whether to do so or not.

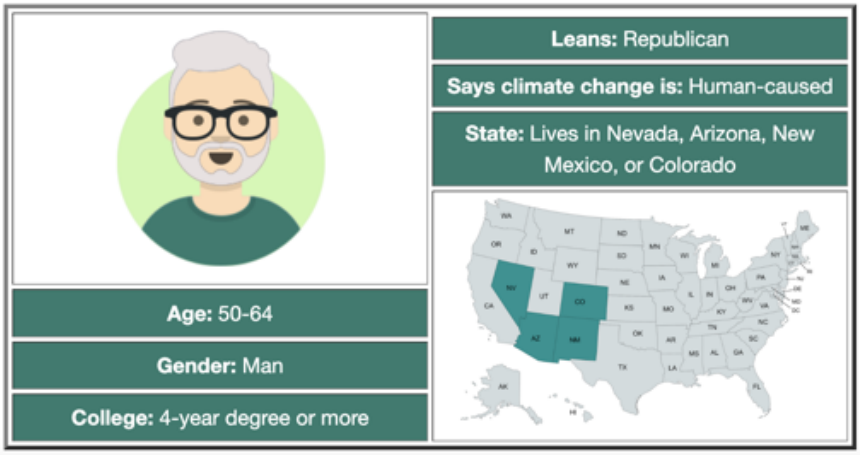
---

If you **decide not to contact Congress**, we won't show them this profile or give them any information about whether you or others contacted Congress.

Figure B51: A2 (Invitation) treatment description

Remember that up to 10 participants in the demographic group below will see your own demographic profile when they take our survey.

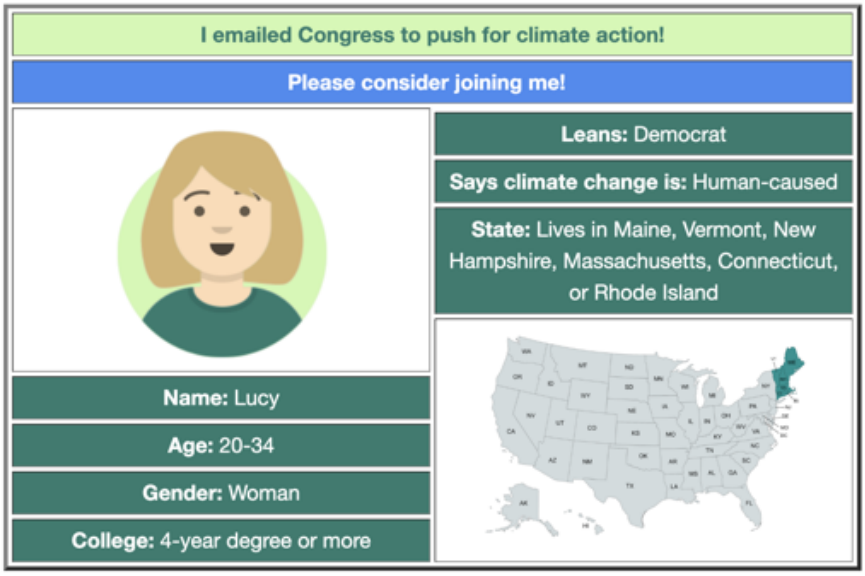
Besides showing these future participants your basic profile, we will also tell them **if you decide to contact Congress, before they decide** whether to contact Congress or not.




---

In particular, if you choose to contact Congress, we'll show these participants the following extra profile of you **before** they choose whether or not to take action.

The profile will say that you contacted Congress and will **invite them to do the same**:



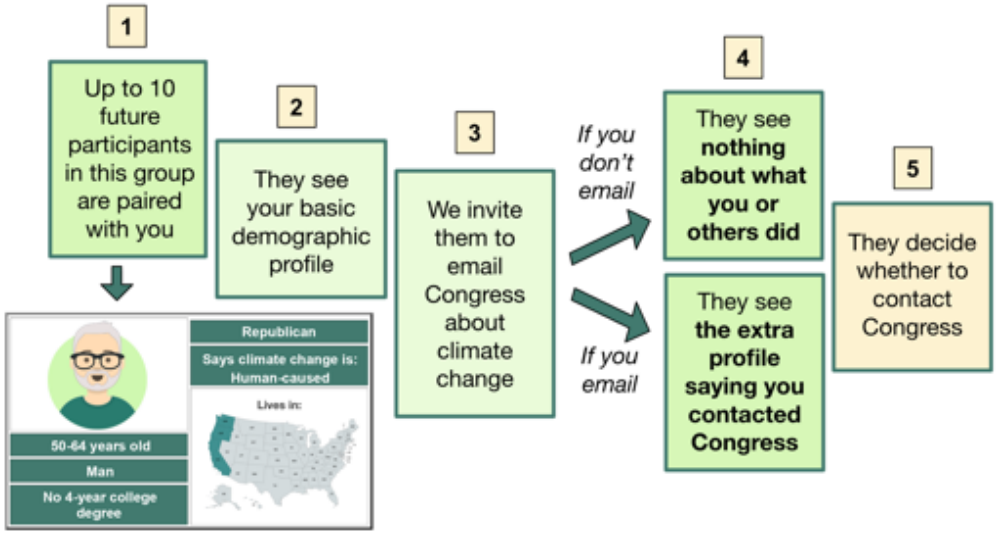

That means that they **will know** you contacted Congress **when they decide** whether to do so or not.

---

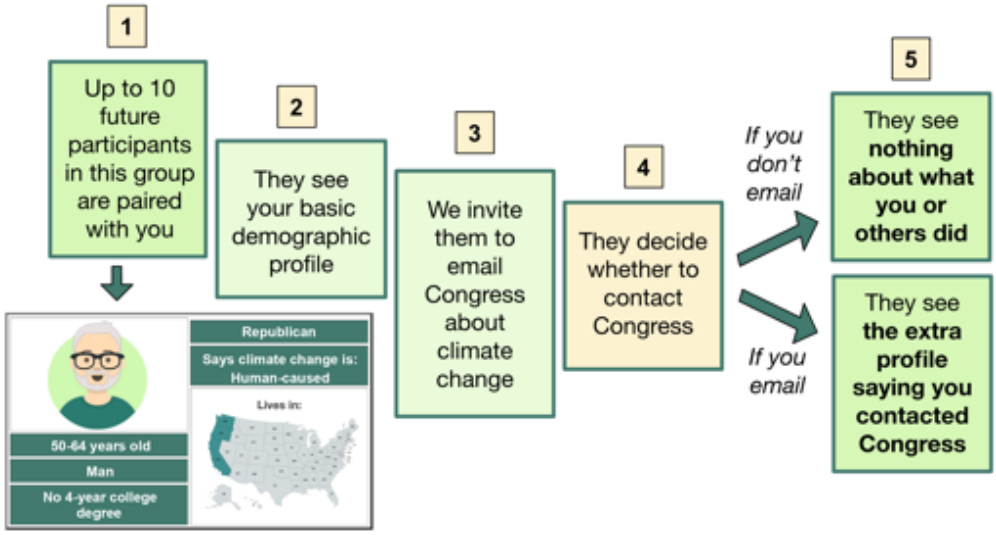
If you **decide not to contact Congress**, we won't show them this profile or give them any information about whether you or others contacted Congress.

Figure B52: Sample flowcharts

For those in the Invitation group (A2):




For those in the Tell-after group (A1):




### D.2.2 A1 and A2 comprehension checks

Right after we implement the A1 and A2 treatments, we ask A1 and A2 participants a series of comprehension checks. In asking these questions, we try to strike a balance between ensuring that participants understand the survey's information structure and alerting participants to our study purpose. We incentivize the questions by telling participants that we will randomly choose 20 people to receive a gift card with $5 for each question that they answer correctly. We then ask the following questions:

1. We told you that up to 10 future participants in a certain group will see your basic demographic profile. If you choose to contact Congress later in this survey, what will they see about you after your demographic profile?

    - A repeat of your demographic profile

- Nothing
- An additional profile that also says that you contacted Congress

We tell participants whether their answer is correct or incorrect and again show them an image of the extra profile saying that they contacted Congress that future participants would see.

2. If you choose to contact Congress, when would these future participants see the extra profile of you saying that you did so?

    - Before they decide whether to contact Congress or not
    - After they decide whether to contact Congress or not

    We tell participants whether their answer is correct or incorrect and reiterate whether future participants would see their profile before or after they decide whether to email Congress.

3. What about if you decide not to contact Congress through our survey? What would your paired future participants see about you in that case?

    - Only your original demographic profile
    - Nothing about you–not even a basic demographic profile
    - Your demographic profile plus an extra profile saying you didn't contact Congress

    We tell participants whether their answer is correct or incorrect and reiterate that if they do not email Congress, the up to 10 future participants they're paired with would still see their basic demographic profile but would not see an additional profile saying whether they emailed Congress or not. We also reiterate that these matched participants would not see any information about whether other participants emailed Congress.

# E WTP Appendix

## E.1 WTP set-up explanation

### E.1.1 Explanation to participants

Figure B53: Explanation of the WTP survey

**Slide 1.**

We have a roster of other study participants who we'll be recontacting **to take a second short survey** soon. In a few slides, we'll show you profiles for 20 of these other study participants.

In order to give these returning participants a sense of who else is participating in this survey, **we'll randomly choose one of these 20 participants** to **pair with you**.

**Slide 2.**

During today's survey, we'll ask you to **choose between two options** for each possible participant you could be paired with.

The two options are:

1. We just show the returning participant your **basic demographic profile** and then **add $6** to our team's purchase of **carbon offsets**.
2. We show the returning participant an **extended profile** of you saying that you **emailed Congress** and, in some cases, **inviting them** to join you.

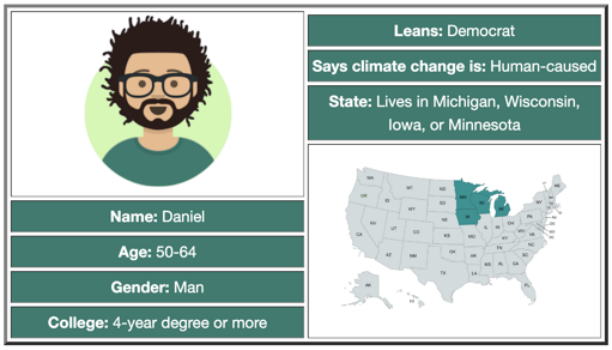
We'll provide more detail about each of these options in the next few slides.

**Slide 3.**

**Option 1:**

Your first option for each possible matched participant is for us to show them just the **basic demographic profile** that you made earlier in this survey and also **add $6** to our team's purchase of **carbon offsets**.

As a reminder, here's the basic demographic profile that you made earlier in this survey:




The next page will give more background on carbon offsets.

**Slide 4.**

In addition to showing the returning participant your basic demographic profile, we would also **add $6** to our team's purchase of **carbon offsets** when that participant returns.

What are **carbon offsets**?

- Buying carbon offsets means paying to reduce greenhouse gas emissions somewhere in the world.
- We will buy carbon offsets through a company called **Clear**, which is certified to invest in verified emissions-reduction projects.

How much will **$6 in offsets** accomplish?

- Adding $6 to our offset purchase will offset the equivalent of **driving 650 miles** in the average 2WD SUV on the market today.
- That's roughly the distance to drive from St. Louis, MO to Pittsburgh, PA or Baton Rouge, LA.

**Slide 5.**

**Option 2:**

Your second option for each possible matched participant is for us to show them an **extended profile** that includes your basic demographics, tells them that you **contacted Congress**, and, in some cases, **invites them to join you.**

The appearance of this extended profile will depend on one key feature of the future participant you're paired with: whether **they've already decided whether to contact Congress or not**.

We'll explain this more in the next few slides.

**Slide 6.**

The study participants that you could be paired with will differ from each other in lots of ways, like having different ages, avatars, education, etc.

One less intuitive way in which participants will vary is that some will be marked as **"Already decided,"** while others will be marked as **"Hasn't been asked."**

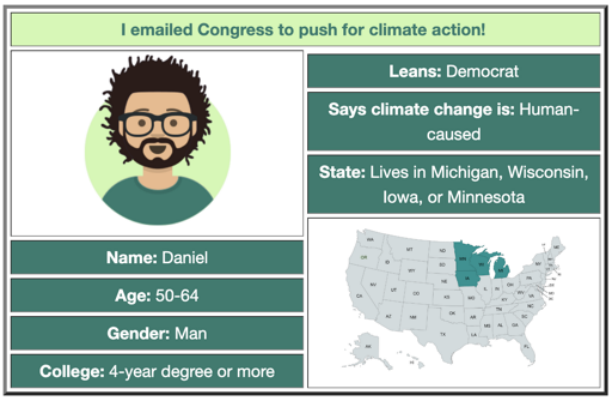

Here's what those mean:

- **"Already decided:"** When they see your basic or extended profile, these participants will **already have decided** whether to contact Congress or not, based on the structure of the survey they're signed up to complete.
- **"Hasn't been asked:"** These participants will see your profile **before they decide** whether to contact Congress, based on the survey structure.

Figure B54: Explanation of the WTP survey, cont.

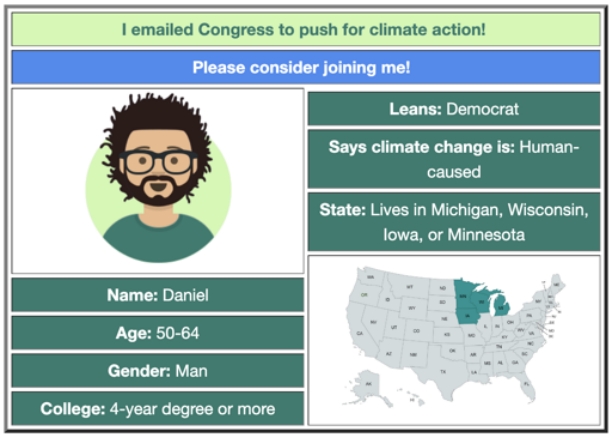
**Slide 7.**

If you're paired with an **"Already decided"** participant and you decide to show them that you contacted Congress, they'll see the extended profile shown below:




**Slide 8.**

On the other hand, if you're paired with a **"Haven't been asked"** participant and you decide to tell them that you contacted Congress, they'll see the extended profile below:



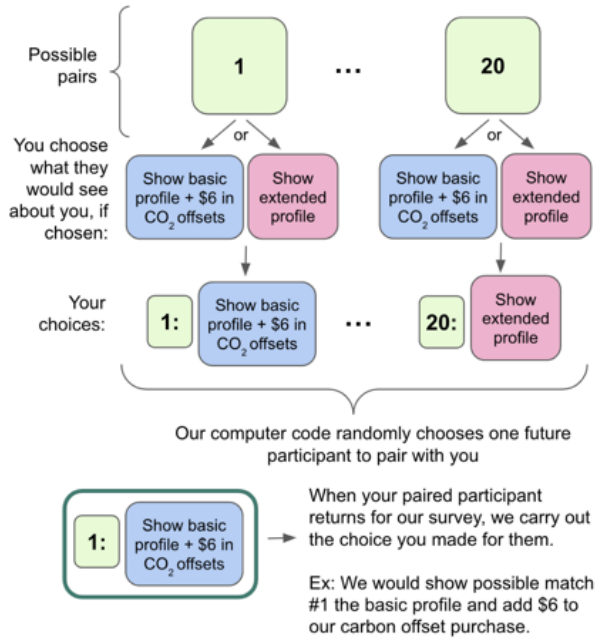

(Note that it includes an invitation to join you in acting, since they haven't yet decided whether to contact Congress or not.)

**Slide 9.**

During today's survey, we'll ask you to **choose ahead of time** which option you'd like us to carry out for **each possible participant**, if it turns out you're randomly paired with them and they return for the follow-up survey.

Later, we will **randomly pair you with one of your 20 possible matches**. When that participant returns for a follow-up survey, we'll then show them the profile about you that you chose today and make any carbon offset donations.

Here's a diagram laying that out:




**Slide 10.**

One last thing to note about the set-up:

The participant you're matched with would see your profile alone, not alongside profiles for any other earlier participants.

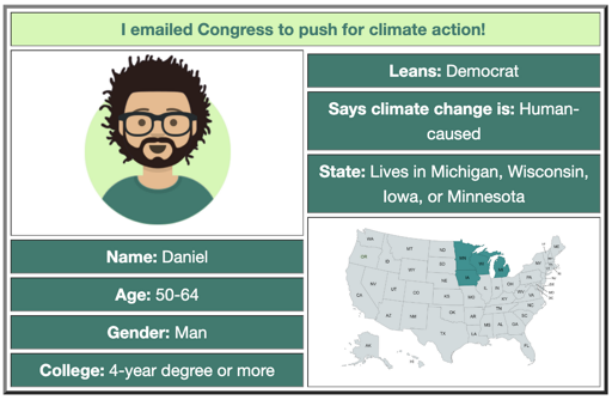
### E.1.2 Making choices with and without climate-belief information

Participants make binary choices between basic and extended profiles for 20 possible matches. These choices look as follows.

Figure B55: Binary choices with and without climate beliefs

*With climate-belief information:*

If you're paired with an **"Already decided"** participant and you decide to show them that you contacted Congress, they'll see the extended profile shown below:




*Without climate-belief information:*

During today's survey, we'll ask you to **choose ahead of time** which option you'd like us to carry out for **each possible participant**, if it turns out you're randomly paired with them and they return for the follow-up survey.

Later, we will **randomly pair you with one of your 20 possible matches**. When that participant returns for a follow-up survey, we'll then show them the profile about you that you chose today and make any carbon offset donations.

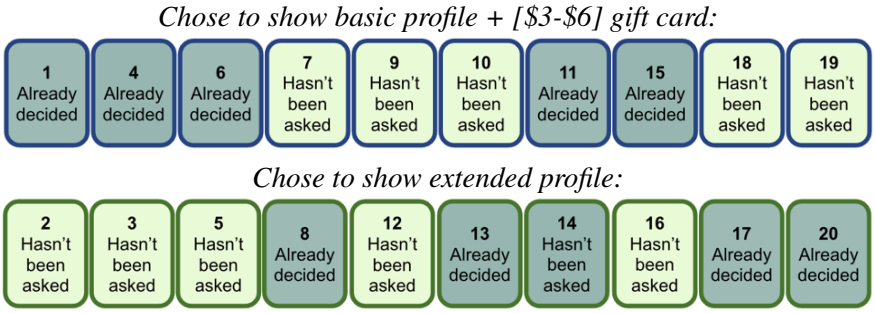
### E.1.3 Comprehension questions

After going through the full WTP set-up once, we then next tell participants that we will go through it again and ask a series of comprehension questions as we go. These questions will be incentivized: we will randomly choose two participants to receive $5 for each incentivize question that they answer correctly. We walk through this comprehension section as follows.

- **Step 1.** We remind participants that we will show them 20 other study participants that we will re-contact for follow-up surveys, and to whom they may be randomly paired. Some of these participants would see their profile before they choose whether to contact Congress or not ("Hasn't been asked"), while others would see their profile after they choose ("Already decided").
- **Step 2.** We remind participants that for each of these 20 options, we will ask them to choose which of two options you would like us to carry out in case they're randomly paired with you and return for their survey. We then ask participants to choose the two options that they can choose between for each possible match, from the following options:
  - We show the match their basic demographic profile and add [CORRECT AMOUNT] to our team's purchase of carbon offsets
  - We show the match their basic demographic profile and add [INCORRECT AMOUNT] to our team's purchase of carbon offsets
  - We show the match their extended profile saying that they contacted Congress
  - We show the match nothing about them

  We then correct participants' answers and reiterate the two choices available to them.

- We ask participants to imagine that they made the choices shown in the image below: they chose to show participants #1, 4, 6, 7, 9, 10, 11, 15, 18, and 19 their basic profile and to show participants #2, 3, 5, 8, 12, 13, 14, 16, 17, and 20 the extended profile, if randomly paired.




- **Step 3.** Next, we remind participants that a computer program will randomly choose one of the 20 study participants to pair with them when that match returns for a follow-up survey.

- We ask participants to imagine that the computer program randomly paired them with participant #10 and ask them three questions in that case.

  1. In that case, what information about you would participant #10 see about you when they return for their follow-up survey?
     - A profile including just your demographics
     - A profile that just says you contacted Congress, with no demographics
     - A profile that includes your demographics and says you contacted Congress
  2. And what amount of money would we donate to carbon offsets on your behalf, when participant #10 returns? [Multiple choice among $0, the correct amount, and 2 incorrect amounts]
  3. Would participant #10 be choosing whether or not to contact Congress after they saw your profile? [Yes / No]

  We then correct participants' answers and reiterate that participant #10 would see their basic demographic profile (along with displaying this profile again), that we would add the correct amount to our carbon offset purchase then they return, and that participant #10 would be choosing whether to contaact Congress after seeing their demographic profile. We then repeat this process for participant #8.

- Finally, we ask participants to imagine that the computer program paired them with participant #5 instead of participant #8; in our imagined scenario, they had chosen to show both of these matches the extended profile saying that they contacted Congress. We then ask whether there is any difference between the extended profile that would be shown to participant #8 and that shown to participant #5, if they were randomly chosen.

We then correct their answer and reiterate that the invitation shown to participant #5 would also include an invitation to join in action, since they would see it before deciding whether to email Congress or not.

## E.2 WTP recruitment and completion

**Sample recruitment:** We recruit WTP participants from "inframarginal climate activists," or those who email Congress without knowing that anyone will be told that they did so. We recruit most of these participants from the Wave-1 A0 control group. However, this group did not yield enough participants to reach our pre-registered sample size, so we also recruited a top-up sample who took the equivalent of the Wave-1 A0 survey and were invited to join the WTP experiment if they emailed Congress, but who we do not include in the experimental sample for the Wave-1 action experiment. In both of these source samples, we invite participants who emailed Congress to take an extra 15 to 20-minute survey section in exchange for being entered into an additional gift-card lottery. We frame this survey section as an opportunity for them to choose how we should spend our time and research funding to have impact on climate change.

In total, 3,616 were assigned to the A0 control group in the Wave-1 action experiment and 1,205 participants were recruited in the top-up sample who completed an equivalent survey. Of these total 4,821 participants, 1,657 (34%) said that they emailed Congress during the survey. While we were unable to confirm in real-time that they'd done so, 85% of these match with email records. We invited these 1,657 participants to take the additional WTP survey, and 1,519 (92%) began the survey.

**Randomizing participants to WTP version:** When participants begin the WTP survey, we randomize them between two versions of the WTP survey. We randomize 1,350 participants to the main WTP version, where participants choose between two options for each possible match: passing on a basic demographic profile and delegating a carbon-offset donation of $3 to $6 from our research funds or passing on an extended profile saying that they emailed Congress. We randomize an additional 166 participants to a "money" version of the WTP experiment that we describe in detail in the next section (Appendix Section E.3). Our main analysis restricts to participants assigned to the main WTP variation.

**Survey completion:** Of the 1,350 participants who begin the main WTP version, 1,239 go through the full description of the WTP set-up and begin answering comprehension questions (described in Appendix Section E.1.3). 1,109 participants reach the point of randomization to either see profiles of matches that show or hide their beliefs about climate change, and 1,091 participants finish answering the final comprehension question after this randomization. The next step in the survey is for participants to start making binary choices for their possible matches. 1,058 participants start making these choices, and 1,023 finish making all 20 choices. These participants compose our main sample for analysis.

## E.3 Money option

### E.3.1 Set-up and differential attrition

While participants in the main WTP version choose between two options for each possible match, we randomize some participants to a "Money" version of the experiment in which they choose between three options:

*Option 1. Basic profile and carbon offsets:* We show the returning match the WTP participant's basic demographic profile and add a fixed amount, randomized at $3, $4, $5, or $6 across participants, to a carbon offset purchase that our research team would make.

*Option 2. Extended profile:* We show the returning match an extended profile saying that the WTP participant emailed Congress and, in some cases, inviting them to join in action.

*Option 3. Basic profile and take-home gift card:* We show the returning match the WTP participant's basic demographic profile and send them a gift card valued at the same amount as the carbon-offset donation.

In total, we randomized 166 participants to the Money group. All of these participants see profiles of possible matches taht include their climate beliefs. Participants assigned to the Money group are about 8 pp (11%) less likely to finish all 20 binary choices than those in the main WTP variant (column 1, Appendix Table A20). Due to this differential attrition, we restrict to participants in the main WTP variant throughout our main analysis of the WTP experiment. However, Section 8.3 shows that all of our results are fully robust to including all participants.

#### E.3.2 Evidence that WTP participants value carbon offsets

One key question in interpreting our main WTP results is whether WTP participants in fact value carbon offsets. Participants' choices when they have the option to choose take-home gift cards suggest that they do. Appendix Figure A29a plots the shares of finished participants in the Money group who choose offsets or gift cards in 0 through 20 choices. While participants chose the basic profile and offset donation in an average of 10.5 choices, they chose the basic profile and gift card in an average of 1.2 choices; about 80% of participants never chose the take-home gift card. Thus, most participants value one dollar of carbon offsets more highly than one dollar in a gift card.

There are two remaining concerns. First, we might worry that participants avoid choosing the gift cards due to the perceived social desirability of taking a pro-climate action. To mitigate this risk, we explicitly frame all of the options as ways to take action on climate change: in presenting the gift-card option, we suggest that participants could use it to buy an eco-friendly product or free up other money to donate to an environmental organization. Second, we might worry that participants' valuation of gift cards may in turn be low because of the hassle cost of redeeming them. However, we find that participants are just as unlikely to choose gift cards of $5 or $6 relative to offset donations of the same amounts—where gift-card hassle costs are less likely to be prohibitively high—than across all amounts between $3 and $6 (Appendix Figure A29b).

## F Appendix on mechanism experiments

### F.1 The belief sample

#### F.1.1 Democrats under-estimate the role of non-political similarity

We estimate the role of non-political similarity in participants' influence beliefs in a variant of equation 5, where we interact $Profile_{ij}$ with indicators that senders and recipients match on gender, age group, educational attainment, and state group.

Appendix Table A30 present our estimates for the role of non-political similarity in participants' estimates of $\Delta P$, and Figure B57 plots these similarity interactions both in participants' influence beliefs and, in comparison, in Wave-2 regressions of profiles' actual influence. All of these coefficients have large standard errors, but the point estimates reveal a consistent pattern. While the Wave-2 results suggest that invitations are substantially more effective when senders and recipients share traits like age, educational attainment, and area of residence, Democrats' influence beliefs do not reflect these interactions. Indeed, politics are the only dimension on which Democrats in the belief sample do not underestimate the role of similarity.

Figure B56: Differential influence beliefs by Wave-2 traits

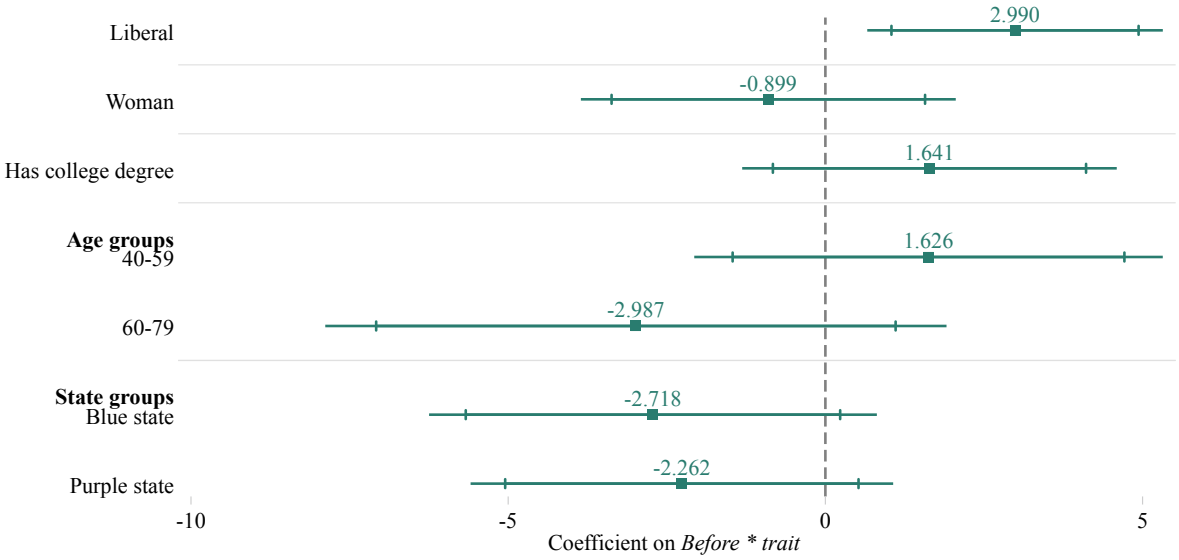
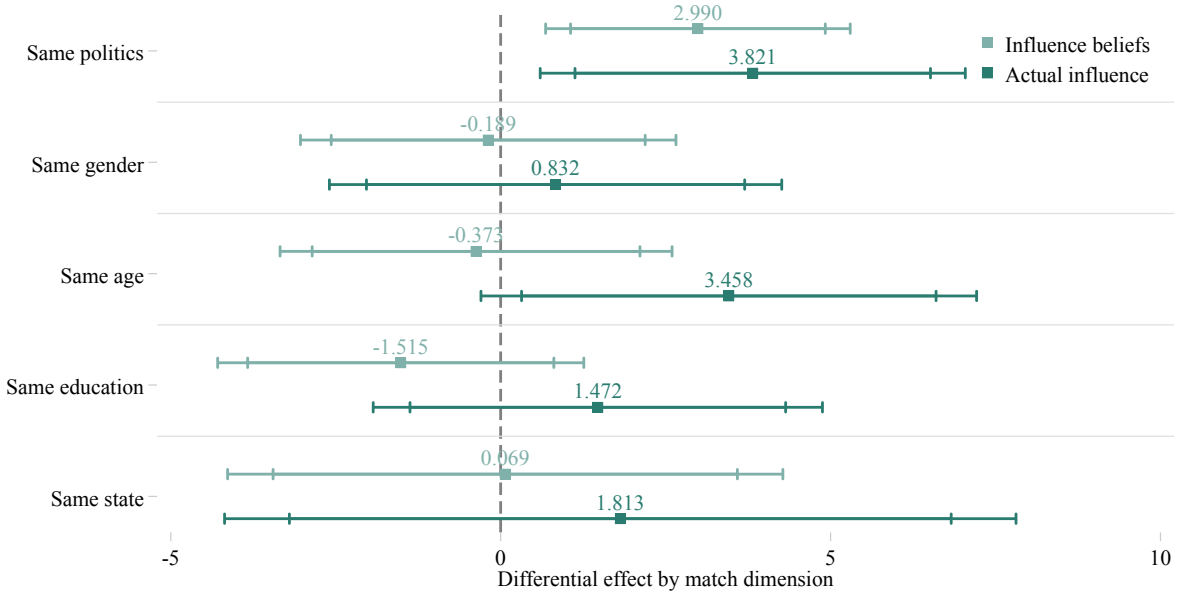



Figure B57: How non-political similarity affects invitations' estimated versus actual influence



## F.2 The certainty experiments

### F.2.1 Text of the explanation to certainty participants

The figure below shows the survey slides in which we explain the set-up of the certainty experiment. We only included the third slide (including the reminder preview of the email form) in round two.

Figure B58: Explanation of the certainty experiment

**Slide 1.**

Earlier in this project, we **recruited many Americans** who took one of our surveys, but who didn't have a chance to email Congress through our form.

Over the next few months, we can arrange for some of these past participants to **email Congress** about climate policy through our form.

Over the next few slides, we'll show you demographic profiles for **[10/14] groups of Americans** who we previously recruited**.** For each profile you see, we'll ask you to choose between two options:

- We enlist a past participant like this to **email Congress about climate policy** through our form. (This means that a letter would be sent **for sure**, not just a possibility of a letter being sent. We can easily recruit someone like each of these groups.)
- We donate **[$8-$16] to carbon offsets**.

We'll provide more detail about these options in the next few slides.

---

**Slide 2.**

First, what are **carbon offsets**?

- Buying carbon offsets means paying to reduce greenhouse gas emissions somewhere in the world.
- We will buy carbon offsets through a company called **Clear,** which is certified to invest in verified emissions-reduction projects.

How much will [**$8-$16] in offsets** accomplish?

- Adding [$8-$16] to our offset purchase will offset the equivalent of **driving [865-1730] miles** in the average 2WD SUV on the market today.
- That's roughly the distance to drive from [e.g. Chicago to Houston].

---

**Slide 3. (Only in Round 2)**

Like we said, the **other option** is for us to re-enlist a past participant to **email Congress** through the form previewed below.

Here are some quick reminders about what the form entails. First, the email will have an **un-editable** subject line **supporting climate policy**.

Then, the body of the message has several blanks where we ask survey-takers to **fill in details about who they are and why they care about climate change**.

Recall that you'd be choosing between carbon offsets and a letter sent **with certainty**---we would make sure that someone like the demographic profile shown actually emailed Congress.

(Show email preview, Figure B40c)

**Slide 4.**

Next, will your choices **matter**?

**Yes**, they will.

We will randomly choose 20 people and implement one of their choices. If one of your choices is selected, we'll do **whatever you picked** in that choice: We'll either donate $10 to carbon offsets, or we'll arrange for a past participant like that to email Congress.

As you make your choices, **please don't worry** about some options being harder or more expensive for us to carry out. We would like to carry out whichever choice you prefer.

On the next page, you can start making your choices. Since there's a lot of information in each profile, the page will only advance once **5 seconds** have passed.

### F.2.2 Other differences between rounds 1 and 2

In addition to slightly revising our description of the choice set-up in the second round, we made several changes to the experimental set-up. First, participants in the second round of the certainty experiment made choices over 14 demographic groups, while those in the first round only made 10 choices. Second, all participants in the second round made choices between letters and $10 in carbon offsets, while those in the first round were randomized to $8, $10, $12, $14, or $16 in carbon offsets.

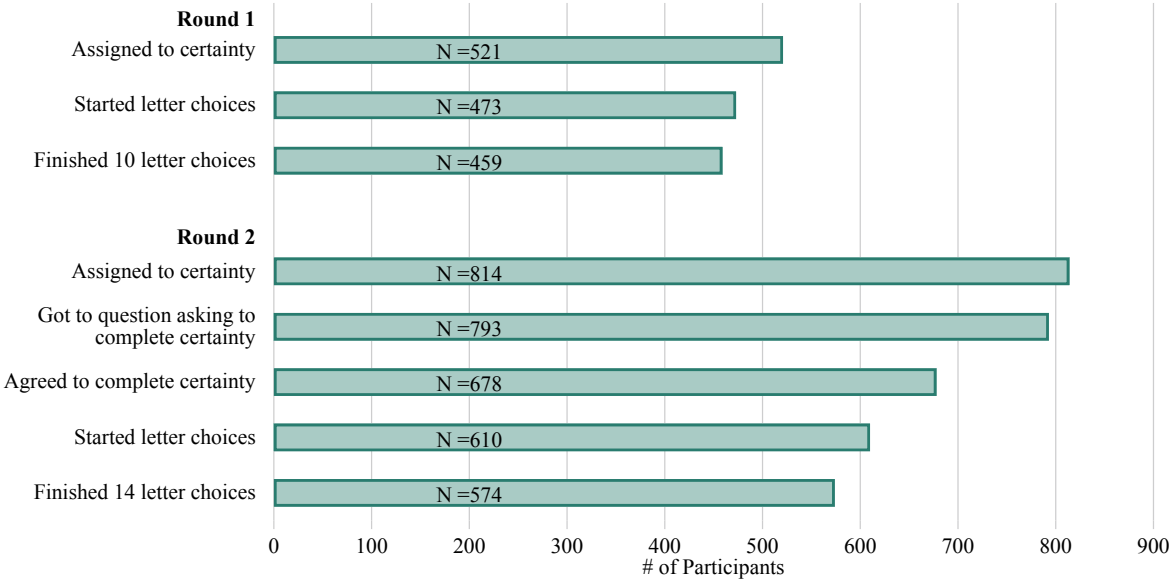
Next, we changed our strategy for constructing demographic groups of email-writers in the second round of the experiment. In the first round, we generated a sample of possible match groups from the full profiles (including avatars and names) of past study participants who had started but were ineligible for the Wave-1 survey because they were not members of the Democratic party. We first selected a set of 40 conservatives and then matched them on all visible demographics to a sample of liberals. We then randomized the resulting 80 profiles into 100 different groups of 10 demographic groups, including 5 conservatives and 5 liberals in each one. While liberals and conservatives in the possible demographic match groups are thus balanced on the other demographic traits shown in the profiles—age, gender, educational attainment, and state—conservatives and liberals could have differed in names and avatars chosen, and the sample on average had non-political demographic traits more representative of conservatives than liberals in our experimental samples.

In the second round, in contrast, we fully randomize political party with respect to other demographic-group traits. Among the set of past study participants who had started but were ineligible for the Wave-1 survey, and were not redirected to the Wave-2 survey, we identify groups by gender, age group (15-year bins), whether they have a college degree, and the 13 state groups where we have both Democratic and Republican leaners. There are 87 such groups. Among these, we try to create a list of demographic groups where it is relatively easy to recruit people in both parties. Within groups by age group and gender, we then select the 8 groups with the most Republicans in our data collected so far and then drop an additional 4 group of men from frequent state groups. We split the remaining 56 groups into 4 groups of 14 demographic profile, and randomize the set of profiles within each set into four orders. Finally, we cross-randomize political party with respect to these demographics by pairing each of these demographic orders with 100 orders of 7 liberals and 7 conservatives, creating a total of 1600 possible match lists of demographic groups in the letter-certainty experiment. Across these groups, political party of each profile is exogenous with respect to other demographic traits for each match. When participants complete the letter-certainty experiment, we randomly pair them to one of these lists of demographic-political groups.

### F.2.3 Certainty recruitment

We recruit participants for two rounds of the certainty experiment. We implement the first round among participants recruited alongside the Wave-1 action-experiment sample and assigned to the belief-elicitation sample described in Section 7.1 above; participants complete the certainty-valuation choices before completing the belief-elicitation exercise. In total, 521 participants are randomized to begin the first-round certainty experiment, 473 make at least one choice, and 459 complete all 10 binary choices.

Figure B59: Recruitment to the letter-certainty experiment




In contrast, we recruit participants for the second round of the certainty experiment via social media after finishing main Wave-1 recruitment. In the second round, we explicitly ask participants to choose whether to complete an additional survey section for the certainty analysis. Of 814 participants randomly assigned to be offered the certainty survey, 793 got to the question in which we invite them to take it and 678 agreed to do so. Of these participants, 610 make at least one binary choice, and 574 complete all 14 binary choices.

## F.3 Follow-up WTP experiment

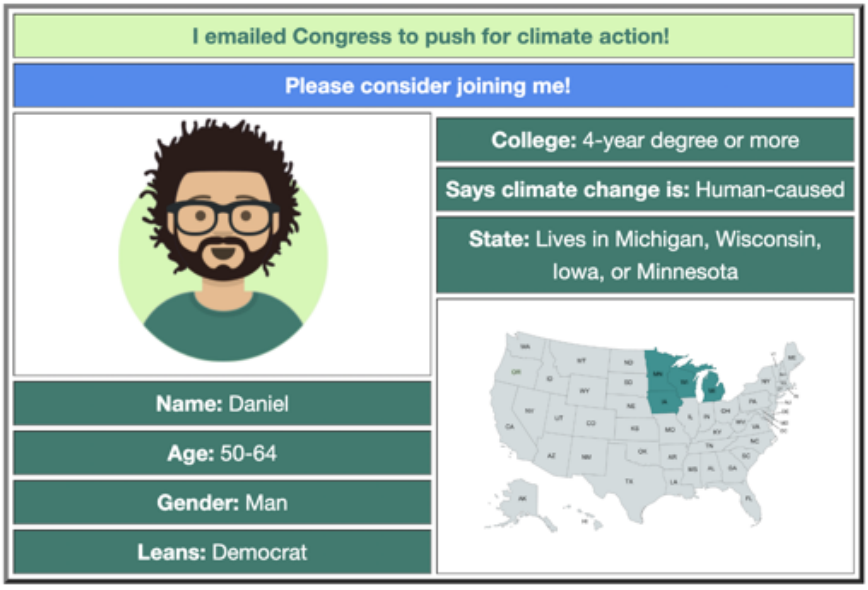
### F.3.1 Explanation to participants

The vast majority of the round-two WTP experiment exactly matches the main WTP set-up. Participants wh oare randomly assigned to have the option to hide their own political leanings also see the following explanation slide, in between slides 9 and 10 of the main WTP explanation (Appendix Figure B53).

Figure B60: Explanation of the option to hide your WTP participants' leanings

We have **two last important notes** about the set-up.

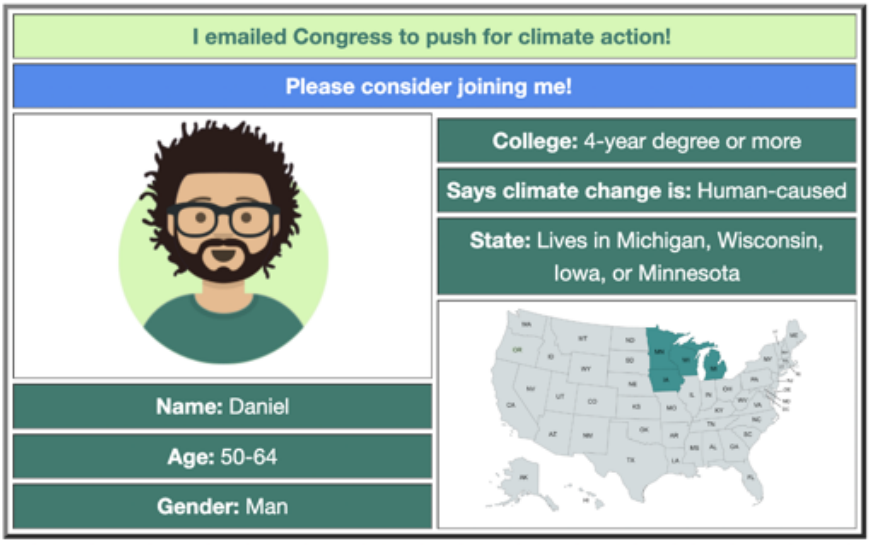
First, all of the sample profiles we've shown you so far have included your own political leanings, like this one:




However, some people might prefer to pass on profiles that don't show this information.

For each possible match, you can **choose** whether to **show your political leanings** or to **remove that line** from the profile you've decided to pass on.

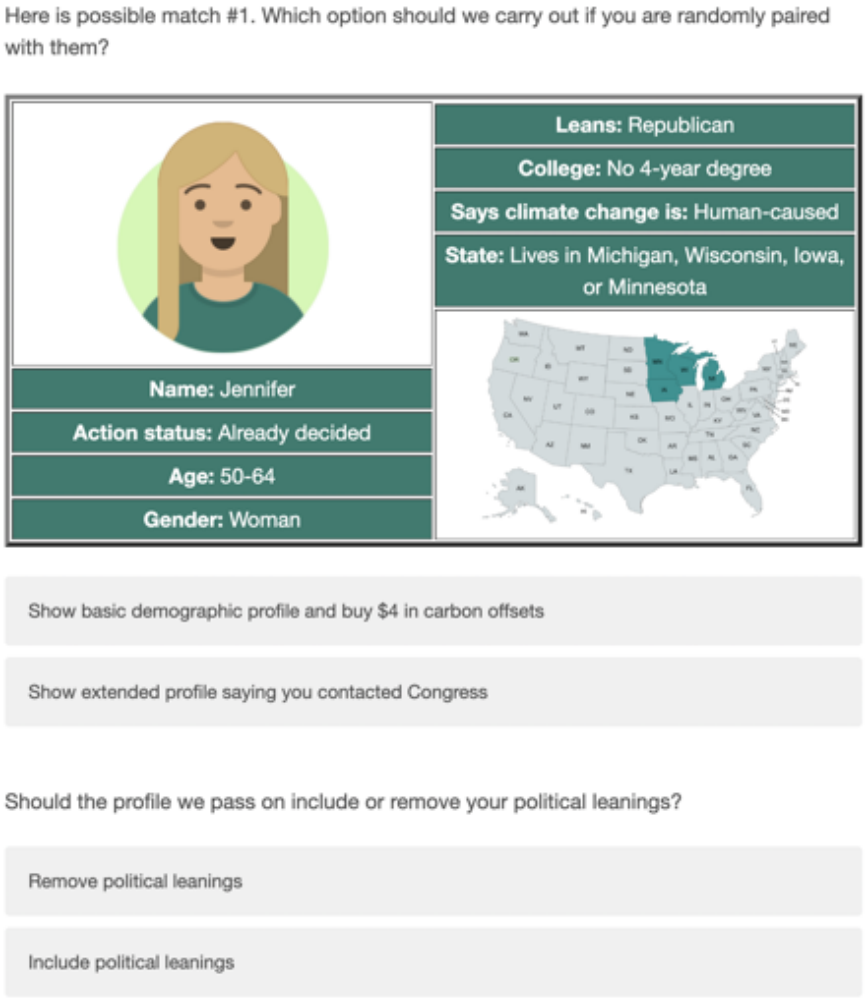
If you removed your political leanings from the extended profile shown above, for example, it would look like this:



### F.3.2 Making binary choices to hide politics

Participants assigned to have the option to hide their own political leanings make that choice for each possible match on the same page where they choose whether to show that match their basic or extended profile.

Figure B61: Making binary choices with the option to hide politics



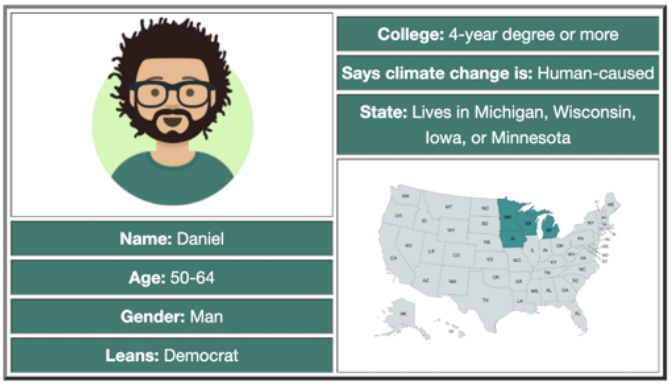
### F.3.3 Comprehension check for option to hide politics

Figure B62: Additional comprehension check for those with the option to hide politics

*Comprehension check text:*

We have one last comprehension question to ask you before we get to the choices.

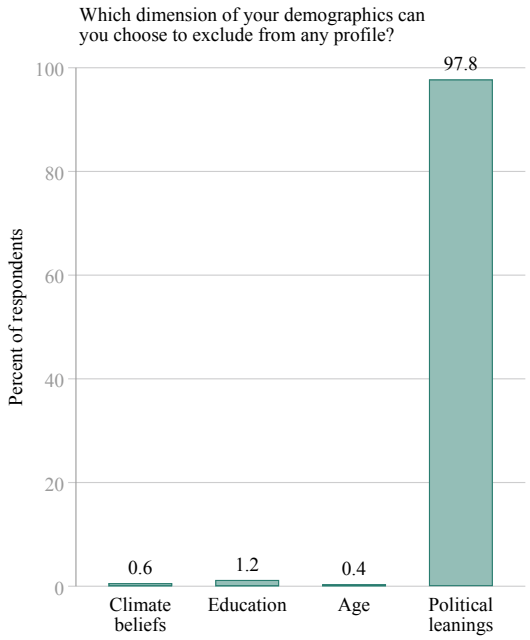
Remember that, as a default, the basic demographic and extended profiles you can choose to pass on will include all of the demographic information below:




**Comprehension check 9:** What is the one dimension of your demographics that you can choose to exclude from any of the basic or extended profiles you will pass on?

- Your political leanings
- Your age
- Whether you believe climate change is mostly human-caused
- Whether you have a 4-year college degree

*Performance on comprehension check:*




### F.3.4 Recruitment for round-two WTP

We recruit participants for the round-two WTP experiment with advertising on Facebook. In total, 4,492 participants qualified for the full set of sample restrictions for Democratic influencers and were randomized to be eligible for the round-two WTP experiment. Of these, 1,612 said that they emailed Congress (36%). We invited all of these participants to complete the additional WTP survey, and 1,420 (88%) started the round-two WTP survey. In total, 995 participants (70%) completed all 20 binary choices; as in the main WTP experiment, we will restrict our analysis to this sample of participants.