

REACHING ACROSS THE AISLE: POLARIZATION AND GRASSROOTS CLIMATE MOBILIZATION*

Lucy Page and Hannah Ruebeck

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Abstract

Mobilizing citizen climate lobbying among Republicans may be a key tool in building political will for bipartisan climate policy in Congress. However, record-high issue polarization—partisan gaps in beliefs—and affective polarization—animosity towards counter-partisans—may hold back efforts to expand the left-leaning citizen climate movement across party lines. We run a series of online experiments with 20,000 participants testing how polarization shapes the spread of citizen movements. When we randomly pair Democrats with Americans across the political spectrum, they are 27% more likely to invite other Democrats than Republicans to email Congress about climate change, even when all of them believe climate change is human-caused. We find three explanations for this outreach gap. First, Democrats correctly believe that their invitation will have half as much impact on Republicans’ action. Second, these strategic beliefs are driven by anticipated affective polarization, or the idea that Republicans will react badly to outreach from Democrats. Finally, Democrats’ own affective polarization matters: they prefer to reach out to co-partisans even when cross-party outreach is as effective.

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*Page: Assistant Professor, University of Pittsburgh, lep147@pitt.edu (corresponding author). Ruebeck: Assistant Professor, Hamilton College, hruebeck@hamilton.edu. We are grateful for generous advising and comments by Frank Schilbach, Esther Duflo, Ben Olken, and Abhijit Banerjee. We also thank Rohini Pande, Daron Acemoglu, Kelsey Jack, Noam Yuchtman, Ro’ee Levy, Colin Sullivan, Lisa Ho and many seminar participants for helpful comments. This project was supported by the JPAL King Climate Action Initiative, the George and Obie Shultz Fund at MIT, and Esther Duflo and Frank Schilbach’s research funds. We were 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>. We received IRB approval from the Massachusetts Institute of Technology Committee on the Use of Humans as Experimental Subjects (Protocol #2212000839). The *Online Supplement* can be found at <https://sites.google.com/view/lucy-page/research>.

1 Introduction

Addressing climate change requires drastic cuts to global greenhouse emissions (IPCC, 2023). In the US, most voters support decarbonization: about 70% support a revenue-neutral carbon tax, tax credits for efficient appliances, and methane restrictions on oil and gas, for example (Leiserowitz et al., 2025). While support is higher on the political left, a substantial 40–55% of Republicans support each of these policies. Despite this majority and cross-party citizen support, climate voting in Congress splits starkly along party lines. In 2024, Democrats and Republicans voted for pro-environmental legislation 93% versus only 3% of the time, respectively (LCV, 2025). Facing this gridlock, progress on climate policy remains slow and vulnerable across elections.

The citizen climate movement plays a key role in building or holding back bipartisan coalitions for climate policy in Congress. Constituent lobbying shapes legislators’ priorities and perceptions of constituent beliefs (Bergan, 2009; Bergan and Cole, 2015), especially within party (Broockman and Ryan, 2016; Lax et al., 2019), and legislators underestimate constituents’ support for climate policy and other liberal priorities (Broockman and Skovron, 2018; Hertel-Fernandez et al., 2019). Then, mobilizing citizen lobbying by Republicans may be critical to building conservative support for climate policy in Congress. Because political action spreads through networks (González, 2020; Bursztyn et al., 2021), Democrats—the core of the mainstream US climate movement—are key agents in spreading this coalition across party lines. However, even if Democrats expect advocacy by Republicans to be differentially impactful, intense political polarization could hold back cross-party outreach. First, Democrats may strategically target co-partisans due to “issue polarization”—partisan gaps in policy preferences—if they expect to more successfully mobilize Democrats. Second, they may *prefer* cooperating with co-partisans due to record-high “affective polarization,” or dislike of the opposite party (Iyengar and Krupenkin, 2018; Boxell et al., 2022).

In this paper, we run a series of online experiments with 20,000 participants studying how political polarization shapes the spread of citizen coalitions. We sequentially home in on three questions. First, do Democrats in the mainstream climate movement try to recruit others for climate advocacy? Second, if so, do they differentially try to recruit co-partisans or to build a broader citizen coalition? Finally, what strategic beliefs or preferences, such as affective polarization, hold back movement-building across party lines? While we focus on the US climate movement, we expect both the potential impacts of bipartisan citizen movements and the frictions to building them to generalize across US policy issues.¹

We construct a simple online network where a sample of Democrats can invite Americans across the political spectrum to email Congress about climate change. We recruit two waves of

¹Little policy change happens in the US without some cross-party support in Congress. Control of the US government is typically split—a single party controlled the presidency, Senate, and House for only 16 of the last 52 years—and passing legislation usually requires minority-party votes even under single-party control (Curry and Lee, 2020).

participants: a Wave 1 of Democratic “influencers” and a later Wave 2 of “targets,” some of whom are Democrats and some of whom are Republicans, but all of whom believe that climate change is mostly human-caused. Across several experimental designs, we give the Wave-1 Democrats opportunities to email Congress about climate change through a form embedded in our online survey; at the same time, we randomly pair some with Wave-2 targets to whom they can send costly, semi-anonymous invitations (Figure 1a) to join them in emailing Congress. Examining Democrats’ invitation choices provides a detailed picture of whether and to whom they seek to expand the climate movement. We later recruit Wave 2 and randomly and truthfully pass on Wave-1 invitations, letting us test the true impacts of invitations on action for both Democrats and Republicans.

This set-up offers several key methodological advantages. First, it allows us to exogenously connect real people across demographic and political lines, as well as to carefully vary our experimental choice set-ups to isolate the role of affective polarization and strategic beliefs. At the same time, this online setting does not sacrifice realism in our measures of political action. Much citizen advocacy happens online, and our primary outcome is whether participants send real emails to Congress via a form that is indistinguishable from those hosted by advocacy groups. Moreover, attempts to mobilize others for political action also largely happen online: of those in our sample who say they’ve invited someone to join in political climate advocacy in the last five years, 80% have done so via direct online messages or social-media posts. Finally, our online survey allows us to supplement revealed-preference findings with detailed analysis of text-based responses to better understand key mechanisms (Ferrario and Stantcheva, 2022; Stantcheva, 2023).

In the *Wave-1 experiment* (N = 8,937), we first show that Democrats try to expand the climate movement overall. Before Wave-1 participants choose whether to email Congress, we randomize them across three arms: the “Invitation,” “Control,” and “Tell-after” groups. In the Invitation group, participants are told that if they email Congress, we will pass on invitations (Figure 1a) from them to future Wave-2 participants to join in action. In the Control group, participants decide whether to email with no mention of others knowing that they did so. While comparing email rates between these groups gives the policy-relevant impacts of opportunities to invite others, it does not isolate influence motives, or attempts to recruit Wave-2 participants to email Congress: Invitation participants may also be more likely to email Congress because they derive self- or social-image benefits from telling others that they did so. To test for influence motives, we randomize other Wave-1 participants to the Tell-after group, which holds fixed these self- or social-image concerns: we will tell future Wave-2 participants if they email Congress, but only *after* those Wave-2 participants make their own email choices.

Then, any gap in email rates between the Tell-after and Invitation groups shows the existence of influence motives: attempts to mobilize others to email Congress during our survey. In contrast, any gap in email rates between Control and Tell-after participants reflects a combination of self-

image effects, social-image effects, and attempts to mobilize Wave-2 participants' action after the survey.² In total, 47% of the Invitation group emails Congress, compared to 31% of the Control group and 44% of the Tell-after group. We reject equality between the Invitation and Tell-after groups with $p = 0.042$. Thus, Democrats are more likely to email Congress when doing so can inspire others to do the same.

While Democrats work to build the climate movement overall, they are much more likely to try to recruit other Democrats than to reach across party lines. We test *whom* Democrats seek to engage in climate advocacy in the *Target-choice (TC) experiment* ($N = 1,023$), embedded among those who email Congress in the Wave-1 Control group. Here, we ask participants to make 20 binary choices between telling particular other survey-takers that they emailed Congress and delegating carbon-offset donations from our research funds; we will randomly implement one of their choices. To identify influence motives, we randomize whether each of the 20 possible matches would see that the TC participant emailed Congress *before* or *after* themselves deciding whether to do so. In turn, the series of 20 binary choices gives us power to test *whom* TC participants differentially try to recruit. As in the Wave-1 experiment, TC participants try to mobilize others: they are 48pp more likely to tell a match that they emailed Congress—giving up an offset donation—when they can do so *before* the match chooses whether to email Congress. However, they differentially try to recruit co-partisans: this *before–after* outreach gap is 11.5pp (27%) larger for Democratic rather than Republican matches, even when TC participants know that all matches believe that climate change is human-caused. This partisan gap dwarfs recruitment gaps by any other trait.

Guided by a conceptual framework, we then use a series of mechanism experiments to decompose the strategic beliefs and affective preferences that underlie this gap. Two key strategic beliefs may matter: Democrats may expect their invitations to have differential impacts on Democrats' and Republicans' likelihood of emailing Congress or may expect emails from Democrats or Republicans to be differentially effective in pushing for climate policy. In addition to these strategic factors, the partisan recruitment gap may be preference-based, arising from affective polarization: Democrats may derive differential emotional returns from trying to engage co- versus counter-partisans in the climate movement, even in this largely anonymous setting.

We find evidence that Democrats' cross-party outreach gap is both strategic and preference-based. First, Democrats correctly expect cross-party outreach to be less successful. In a sample ($N = 194$) recruited alongside Wave 1, Democrats estimate that invitations will make other Democrats 6pp more likely to email Congress, while making conservatives only 3pp more likely to do so; we reject equality with $p = 0.011$. These beliefs closely match the true impacts of invitations in

²These influence motives appear to be salient. In qualitative responses, 17% of Tell-after participants said that the treatment affected them because it allowed them to motivate others, about 66% of Tell-after participants who said it affected them overall.

Wave 2. When we truthfully and randomly pass on Wave-1 invitations to Wave-2 targets, we find that seeing an invitation makes other Democrats (N = 5,027) and Republicans (N = 2,954) 5.8pp and 2.1pp more likely to email Congress, respectively. We cannot reject that Democrats' influence beliefs are correct on average.³

In two additional mechanism experiments, we then show that affective polarization contributes to the outreach gap—even in this online network—in two key ways. First, Democrats strategically expect to have less impact on Republicans' email choices because of *second-order* or *anticipated* affective polarization: they expect Republicans to dislike them. In a second round of the TC experiment (N = 995), we randomly allow some participants to hide their own political leanings from the profiles they can choose to pass on. While Democrats show their political leanings to 91% of fellow Democrats, they show their politics to only 34% and 44% Republicans seeing their profile before and after deciding whether to email Congress, respectively. In quantitative belief elicitations, Democrats estimate that hiding their political leanings would make invitations to Republicans more than three times as effective, closing the gap in their ability to mobilize co- and counter-partisans. Participants' free-text responses strongly point to the role of anticipated affective polarization in this shift: 60% spontaneously say that Republicans would have a negative *emotional* reaction to inviters' political affiliation.

Second, Democrats remain much more likely to try to recruit co-partisans even when they expect invitations to co- and counter-partisans to have equal strategic returns; this remaining outreach gap suggests that Democrats have non-strategic, affective preferences for reaching out along party lines. We see this pattern by combining results from the second-round TC experiment and the “email-valuation experiment,” described below. In the second-round TC experiment, Democrats differentially try to recruit co-partisans even when hiding their politics from Republicans—and thus when their elicited beliefs suggest they expect to be as successful in mobilizing emails from Democrats and Republicans. In theory, this persistent outreach gap could still be strategic if Democrats expect *emails per se* sent by co-partisans to more effectively push for climate policy than those from Republicans. However, the email-valuation experiment contradicts this possibility. Here, we ask 574 Democrats to choose whether our research team should donate to carbon offsets or recruit emails to Congress with certainty from particular demographic groups.⁴ When Democrats can obtain emails with certainty and do not themselves interact with potential recruits, they are equally likely to choose emails from co- and counter-partisans over offsets; on average, they also rate emails from Democrats and Republicans as having equal impact on the passage of

³While forming correct beliefs about the role of partisanship, Democrats have wrong beliefs about how non-political traits shape the effects of invitations: they significantly underestimate the differential impact of invitations by recipient gender, age, and demographic similarity to senders.

⁴We tell participants that if they choose an email over an offset donation for a particular demographic group, we commit to finding someone like that group to email Congress through our form.

climate policy. Then, the sticky outreach gaps in the round-two TC experiment cannot be explained by differential strategic impacts. Rather, they suggest by elimination that Democrats have non-strategic, affective preferences to reach out to co-partisans, even in this online setting.

Our results suggest that unified, bipartisan grassroots movements are unlikely to form: even when we directly connect Democrats with potential allies across the political aisle, they are much more likely to try to recruit co-partisans, creating a force towards increasing homogeneity. Partisan outreach gaps are likely even starker in other settings—when working via segregated social networks or with more scope for direct interaction, where affective polarization may play an even larger role. Mobilizing bipartisan citizen advocacy on polarized issues like climate change may then require one of two approaches: building parallel liberal and conservative movements at the grassroots level, or directly targeting first- and second-order affective polarization. For example, recent work in political science and economics suggests that cross-party conversations can durably reduce affective polarization ([Santoro and Broockman, 2022](#); [Fang et al., 2025](#)).

Our paper contributes to several bodies of economic research. First, we add to the long and growing literature on the political-economy constraints to efficient environmental policy (e.g. [Oates and Portney, 2003](#); [Battaglini and Harstad, 2016](#); [Besley and Persson, 2023](#); [Dechezleprêtre et al., 2025](#); [Hsiao and Kuipers, 2025](#)). While recent work studies how to increase citizen support for climate policy ([Dechezleprêtre et al., 2025](#)), we show that there are frictions to converting policy support into citizen movements for legislative action, even when policy support exists. Our work on the role of citizens as lobbyists for policy change complements recent work on the role of citizens in enforcing environmental policy ([Colmer et al., 2024](#); [Buntaine et al., 2024](#)).

We also contribute to a growing literature on the spread of political movements (e.g. [García-Jimeno et al., 2022](#); [Qin et al., 2024](#)), where recent work shows that individuals’ political participation has externalities—sometimes positive, sometimes negative—on others’ action ([Perez-Truglia and Cruces, 2017](#); [Cantoni et al., 2019](#); [González, 2020](#); [Bursztyn et al., 2021](#); [Hager et al., 2022, 2023](#)). Our paper is the first to show that citizens internalize positive political spillovers and, in fact, intentionally try to spread political movements.⁵ A related literature experimentally tests interventions to mobilize citizens for political action. We add to the subset of this work exploring tools to engage citizens in collective action *between* election cycles ([Han, 2016](#)), while most prior work focuses on voting (e.g. [Green et al., 2013](#); [Pons, 2018](#)). Past work shows that social-image returns play a key role in voter turnout ([Dellavigna et al., 2017](#); [Gerber et al., 2017](#)), and we identify influence motives as an additional mechanism making publicizing political action a powerful mobilization tool.

⁵We also document diffusion from a particular form of interaction: sending invitations to email Congress. In experimentally introducing and evaluating a tool to facilitate diffusion, our work is similar to [Bond et al. \(2012\)](#) and [Jones et al. \(2017\)](#). Our paper is unique in experimentally creating interactions between people not connected by existing social networks, letting us causally estimate how spillovers are mediated by political and non-political similarity.

In doing so, we also add 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; Mengel and Ganguli, 2024). Esguerra et al. (2023) and Mengel and Ganguli (2024) use similar experimental designs in contemporaneous work to show that Germans are more likely to register for COVID-19 vaccination and that UK residents are more likely to donate to carbon offsets, respectively, when doing so may encourage others to do the same. While Esguerra et al. (2023) find that participants have no impact on others’ vaccination, participants in our setting and in Mengel and Ganguli’s experiment can in fact influence others’ behavior, a feedback cycle that may be necessary to sustain influence motives in the long run. Together, these three papers suggest that opportunities to motivate others can effectively promote pro-social behavior. We are the first to decompose the drivers of influence motives and to study them in political action.

Finally, we add to the literature on how rising partisanship affects democratic outcomes in the US, finding that own and anticipated affective polarization play key roles in restricting cross-party cooperation among citizens. While many in political science and economics warn that affective and issue polarization may reduce support for democratic norms and bipartisan cooperation in Congress (e.g. Finkel et al. 2020; Kingzette et al. 2021; Boxell et al. 2022), the empirical evidence for these claims has been limited and mixed (Broockman et al., 2023; Voelkel et al., 2023). We also provide some of the first evidence on the role of strategic beliefs in explaining the partisan engagement gaps that have been documented in a wide range of non-political domains (e.g. Gift and Gift, 2015; Huber and Malhotra, 2017; McConnell et al., 2018). With a few exceptions (Dimant, 2023; Zhang and Rand, 2023), most prior work implicitly attributes these gaps to preference-based affective polarization.

The paper proceeds as follows. First, Section 2 describes the experimental context, and Section 3 lays out a framework for influence motives. Sections 4 and 5 experimentally test whether and with whom Democrats try to expand the climate movement, and Section 6 examines the mechanisms underlying Democrats’ differential outreach along party lines. Section 7 concludes.

2 Experimental context: An online network for climate action

2.1 Basic network structure: Wave-1 influencers and Wave-2 targets

To test how Democrats try to expand the US climate movement, we construct a 20,000-person online network in which a large sample of Democrats can invite Americans across the political spectrum to join them in emailing Congress about climate change. This network randomly connects two waves of participants: a Wave 1 of Democratic “influencers” and a Wave 2 of “targets,” who lean towards either the Democratic or Republican parties. Network participants are semi-anonymous, interacting only via demographic profiles (Figure 1).

Across several experimental designs, we first recruit Wave-1 Democrats and give them the opportunity to email Congress about climate change via a form embedded in our online survey (Section 2.1.2). We then randomly give some Wave-1 participants opportunities to send costly invitations (Figure 1a) asking Wave-2 participants to join in emailing Congress. These invitations show the sender’s demographics, say that they emailed Congress, and encourage the recipient to do the same. The experiments in Sections 4, 5, and 6 will use variants on this basic structure to provide a detailed picture of how Democrats try to expand the US climate coalition.

2.1.1 Democratic influencers and bipartisan targets

We design Waves 1 and 2 to approximate key dynamics in the US climate movement. We restrict Wave-1 influencers to be “typical” climate activists: members of the Democratic party who believe that climate change is mostly human-caused (Leiserowitz et al., 2025). We recruit these Democrats via ads on social media for a survey about climate change (Appendix Figure A1), yielding a sample that mirrors the mainstream liberal climate movement in the US (Table 1). In particular, Wave 1 is quite politically engaged—73% report that they’ve contacted elected representatives in the last two years—as well as highly educated and wealthy: over 80% have a 4-year college degree or more, and 51% have annual household income over \$100,000.⁶ While our sample therefore cannot represent the entire environmental movement, it well approximates a mainstream movement that remains predominately white, affluent, and educated.⁷ Section 4.3 details Wave-1 recruitment.

In contrast, we allow Wave-2 participants to belong to or lean towards either the Democratic or Republican parties, but require all to believe that climate change is mostly human-caused. This set-up lets us test whether members of a heavily partisan mainstream political coalition, like the climate movement (Atske, 2020; Uhlmann, 2020), seek to broaden it with *potential allies* across the aisle. Note that we refer to Wave-2 participants who lean towards the Republican or Democratic parties, regardless of party membership, as Republicans and Democrats throughout the paper. We recruit much of the Wave-2 sample from an online panel service with no mention of climate change, yielding a sample that is less politically engaged and closer to national demographic averages (Table 1). Section 6.1.2 details Wave-2 recruitment.

⁶For experimental feasibility, the Wave-1 sample is also fully white. Our experimental design involves randomly matching Wave-1 and Wave-2 demographic cells defined by 15-year age bins, gender, educational attainment, and 13 clusters of political similarity and geographically close states (Appendix C.2). Feasibly implementing this network structure requires ensuring that all demographic cells in Waves 1 or 2 are sufficiently large. We were not able to recruit enough non-white participants in experimental pilots, so we restrict Waves 1 and 2 to white participants. For the same reason, we restrict to those who live in the contiguous US, are between 20 and 79, and identify as a man or woman. Appendix A.1 details participant screening.

⁷The mainstream US environmental movement has historically been dominated by white and educated Americans (Taylor, 2014; Purdy, 2015; Jones, 2020), despite evidence that racial minorities and lower-income communities disproportionately bear environmental harms (Banzhaf et al., 2019; Jbaily et al., 2022), may report higher environmental concern (Pearson et al., 2018), and have long led the active environmental-justice movement (Mohai et al., 2009).

2.1.2 *Real-stakes climate advocacy*

Throughout the study, we measure climate advocacy by whether participants email their national Senators and House Representative about climate change via a form embedded in our online survey (Figure 2). Whether participants email Congress via this form is an ideal measure of climate action. First, the email form has high external validity; we license it from a software company that provides nearly identical forms to advocacy organizations across the US. Second, sending customized emails is a meaningful form of political action. Politicians systematically misperceive constituents' opinions (Broockman and Skovron, 2018), and citizen lobbying can change politicians' voting behavior (Bergan, 2009; Bergan and Cole, 2015). Finally, emailing Congress in our setting takes effort and may be relatively robust to experimenter demand effects: participants who write spend an average of 8.6 minutes composing their emails.

While the email body is fully customizable, we fix the subject line to support climate action, curbing the possibility that Democrats avoid mobilizing Republicans because they expect their emails to oppose climate policy. Participants know that no email sent via our form can fully oppose climate action, letting us test whether Democrats choose to pursue *allied* action across party lines.⁸

2.2 Motivating descriptive evidence

Before experimentally observing whether and how Democrats try to build a climate coalition, we document a series of qualitative patterns in a non-experimental sample of Democrats (N = 197).⁹

Democrats want bipartisanship on climate: 89% of our sample agrees that building support for climate policy among both Democratic and Republican politicians is crucial for reducing emissions, and 77% agree that the US can only pass future climate legislation if lawmakers in both parties support it (Panels A and B, Appendix Figure A2). Moreover, they see bipartisan citizen advocacy as key to building these coalitions in Congress: 84% agree that lobbying by Republicans, rather than Democrats, could more effectively increase Republican lawmakers' support for climate policy, and 97% say that a bipartisan climate movement would more effectively advance US climate policy than a Democratic movement (Panels C and D, Appendix Figure A2). Democrats see a role for themselves in building this bipartisan movement, with 82% agreeing that Democrats should try to get more Republicans involved in climate advocacy (Appendix Figure A3).

But few have themselves tried to recruit Republicans: While 58% of Democrats in our sample say that they've invited someone to join in climate advocacy in the last 5 years, they've largely

⁸Consistent with this, one mechanism experiment (Section 6.3.2) elicits Democrats' beliefs for how emails to Congress via our form from different demographic groups would affect whether legislators support a hypothetical climate bill; 95% said an email from a Republican via our form would make legislators more or equally likely to support the bill.

⁹We recruit the motivating sample via the same advertising and sample restrictions as the Wave-1 experimental sample, and these samples are statistically indistinguishable on most baseline traits (columns 3 and 4, Appendix Table A1). The primary exception is that motivating-sample participants are more likely to be women.

invited co-partisans (Panel A, Appendix Figure A4). This gap could simply reflect that Democrats have tighter social ties to other Democrats (Panel B, Appendix Figure A4), but it could also stem from more subtle preferences or strategic beliefs about reaching across party lines. In this experiment, we isolate the role of these beliefs and preferences by abstracting from endogenous social networks: we will observe how Democrats choose to spread political movements when we randomly connect them with others across the political spectrum.

3 Conceptual framework

This section outlines a simple conceptual framework that guides our study design.

3.1 Identifying efforts to build the climate movement

We measure Democrats’ attempts to spread the climate movement via their costly choices to send invitations (Figure 1a) encouraging future Wave-2 participants to join in emailing Congress. Throughout our experiments, we face a central identification challenge: participants may choose to send invitations not just because of influence motives—attempts to engage others in climate action—but also because they derive self- or social-image benefits from simply telling others that they emailed Congress.

Consider a Wave-1 participant who has just emailed Congress and can pass on an invitation asking Wave-2 participants to do the same. We outline the payoffs to inviting others as follows:

$$V(\text{Invite}) = V(\text{Try Influence Email}) + V(\text{Try Influence Later}) + V(\text{Image}) - C \quad (1)$$

These payoffs include both the value of trying to mobilize others—either to email Congress during the survey, $V(\text{Try Influence Email})$, or to engage in pro-climate action after the survey, $V(\text{Try Influence Later})$ —and image benefits from others knowing that they emailed Congress, $V(\text{Image})$.¹⁰ Invitations could also cost time or the utility or disutility of interacting with others, C .

In several experiments, we isolate influence motives from image concerns by varying *when* participants can tell targets that they emailed Congress. Compare the value of inviting others to email Congress (Equation 1) with the value of telling someone that one emailed Congress *after* they decide whether to do the same:

$$V(\text{Tell After}) = V(\text{Try Influence Later}) + V(\text{Image}) - C \quad (2)$$

¹⁰Note that we do not assume that *all* image benefits of invitations are additively separable from the the value of trying to mobilize others, only the image returns from others knowing that one emailed Congress. We describe below that $V(\text{Try Influence Email})$ may include self- or social-image returns to being seen as trying to mobilize others, for example. The identification strategy we outline below assumes only that the social-image returns to telling others that one emailed Congress before or after they make their own choice (separately from trying to affect their action) are equal. We comment on the validity of this assumption when presenting experimental results (see footnote 18).

Telling someone that one emailed Congress after the target’s own choice would still activate any social- or self-image returns to them knowing that you emailed Congress, the value of trying to influence their action after the survey, and any interaction costs. The only difference in these payoffs is the value of trying to affect whether recipients email Congress during the survey itself, $V(\text{Try Influence Email})$.¹¹

Guided by this logic, we test whether Democrats value trying to mobilize others by testing whether they are more likely to tell others that they emailed Congress when they can do so *before* targets decide whether to act—when telling them can affect their choice—rather than *after*.

3.2 Decomposing influence motives

What beliefs and preferences underlie these influence motives, and how might they differ across party lines? We decompose $V(\text{Try Influence Email})$ as follows:

$$V(\text{Try Influence Email}) = \Delta P(\text{Email}) * [V(\text{Email Impact}) + A(\text{Target Involved})] + A(\text{Try Influence Email}) \quad (3)$$

If participants only value their impacts on climate policy, the returns to trying to mobilize others would be the product of $\Delta P(\text{Email})$ —the sender’s beliefs about her invitation’s impact on the probability that the target emails Congress—and $V(\text{Email Impact})$ —how highly she values the expected impact of the target’s email.

In addition to these strategic returns, participants may derive affective or warm-glow returns from trying to engage others in climate action. First, participants may enjoy or value seeing themselves or being seen by others as trying to recruit action, $A(\text{Try Influence Email})$. Second, the total value of successfully recruiting the target—the terms scaled by $\Delta P(\text{Email})$ —may also include warm-glow returns to knowing that one is cooperating with them, $A(\text{Target Involved})$.¹²

3.3 Drivers of partisan gaps in influence motives

What beliefs or preferences could then underlie gaps in Democrats’ attempts to recruit others along versus across party lines?

¹¹Contemporaneous work by [Esguerra et al. \(2023\)](#) and [Mengel and Ganguli \(2024\)](#), as well as earlier work by [Karlan and McConnell \(2014\)](#), use similar experimental variation to isolate influence motives.

¹²This framework makes several assumptions. First, it assumes additive separability between the affective returns to the act of trying to recruit others, $A(\text{Try Influence Email})$, and the impacts of outreach, as well as between the affective returns to cooperation, $A(\text{Target Involved})$, and beliefs about email impact, $V(\text{Email Impact})$. These simplifications are unrealistic but will not threaten our conclusions in Section 6, where we decompose partisan gaps in influence attempts (see footnote 48). Finally, we assume that Democratic influencers value the *absolute* impacts of their invitations on targets’ action, rather than their proportional impact. Proportional effects may matter if influencers derive differential affective benefits from engaging an “unlikely” advocate in the climate movement, which would be captured in $A(\text{Target Involved})$ or $A(\text{Try Influence Email})$.

Strategic drivers of partisan gaps: Such gaps could arise from strategic beliefs. First, Democrats may believe that their invitations would have more or less impact on whether Democrats versus Republicans email Congress ($\Delta P(\text{Email}|D) \neq \Delta P(\text{Email}|R)$). For example, Democrats may expect co-partisans to better trust their persuasive motives or their ability to judge the importance of climate policy or effects of advocacy, to differentially perceive their invitation as a signal of group-compatible behavior, or to have stronger tastes to cooperate with them. Second, participants may believe that emails from Democrats or Republicans would be differentially impactful in achieving short- or long-term climate policy goals ($V(\text{Email Impact}|D) \neq V(\text{Email Impact}|R)$), perhaps because they would advocate for different policies, be differentially informative signals of citizen preferences, or influence co-partisan lawmakers.

Preference-based drivers of partisan gaps: In addition to these strategic factors, Democrats may differentially try to mobilize co- versus counter-partisans due to *preference* gaps arising from affective polarization. Democrats in the Wave-1 experimental sample are highly affectively polarized (Appendix Figure A5): they rate the Democratic Party an average of 63 degrees warmer than the Republican Party on the standard “feelings thermometer” measure of affective polarization (Iyengar et al., 2019), and most report strongly preferring that friends be Democrats. In our framework, Democrats may derive differential emotional returns from cooperating with co- versus counter-partisans in the climate movement ($A(\text{Target Involved}|D) \neq A(\text{Target Involved}|R)$) or from reaching out to Democrats or Republicans ($A(\text{Try Influence During}|D) \neq A(\text{Try Influence During}|R)$).¹³

Note that this decomposition does not capture Democrats’ distaste for simply interacting with counter-partisans, which would enter our framework as differential invitation costs, C , in Equation 1. However, this distaste is likely relatively small in our setting, which has limited cross-participant interaction; we do not test for these differential interaction costs in particular, instead decomposing the returns to mobilizing others for climate action *conditional* on interacting with them. Our results therefore provide a lower bound for the role of Democrats’ own affective polarization in holding back cross-party outreach.

Role of affective polarization in strategic beliefs: While affective polarization may directly create gaps in the affective returns to within- or cross-party invitations, *second-order* or *anticipated* affective polarization may also drive gaps in Democrats’ strategic beliefs about their ability to influence others, $\Delta P(\text{Email})$. In particular, Democrats may believe that $\Delta P(\text{Email}|D) > \Delta P(\text{Email}|R)$ in part because they expect counter-partisans to dislike and be unwilling to cooperate with them.

¹³Political scientists warn that affective polarization may directly reduce cooperative norms and preferences like these (Levendusky, 2018; Finkel et al., 2020). While the empirical evidence that affective polarization reduces tastes for bipartisan cooperation in Congress is mixed (Broockman et al., 2023; Voelkel et al., 2023), Broockman et al. (2023) find strong effects of affective polarization on desired social distance from counter-partisans, and prior work in political science finds that Americans favor co-partisans even in domains with no direct interaction (Iyengar and Westwood, 2015; Dimant, 2023).

3.4 A roadmap to the experiments

This framework guides our study design, which consists of several connected experiments (Figure 3). First, the *Wave-1 experiment* (Section 4) and an embedded sub-experiment, the *Target-choice experiment* (Section 5), use the timing logic described in Section 3.1 to show that Democrats are motivated to engage others in climate action and differentially try to engage co-partisans.

Guided by the framework in Sections 3.2 and 3.3, we then run a series of mechanism experiments decomposing the strategic beliefs and affective preferences underlying why Democrats differentially reach out along party lines (Section 6). Finally, we randomly and truthfully pass on invitations to Democrats and Republicans in the *Wave-2 experiment* (Section 6.1), testing invitations’ true impacts on email rates, $\Delta P^*(Email|D,R)$, and the accuracy of influencers’ beliefs.

4 Wave-1 experiment: Do Democrats try to recruit climate action?

Before examining *whom* Democrats try to engage in climate action, we first show in the “Wave-1 experiment” that Democrats try to recruit others overall: they are more likely to email Congress when doing so can inspire others to join them.

4.1 Wave-1 experimental structure

Appendix Figure B1 summarizes the design of the Wave-1 experiment. The full survey scripts for all experiments in this paper are available in the Online Supplement.¹⁴

Pairing with future Wave-2 participants: Wave-1 participants first build a basic demographic profile of themselves (Figure 1b). Next, we tell them that they will be paired with up to 10 future Wave-2 participants in a particular demographic group, who will see the Wave-1 participant’s demographic profile when they take our future survey.¹⁵ We ensure that participants attend to the demographics of their paired Wave-2 participants by asking them to rate and explain how similar they feel to these future participants.

Treatment variations to identify influence motives: Next, all Wave-1 participants see a preview of the upcoming opportunity to email Congress. Before they decide whether to do so or not, we randomize participants to three treatment variations guided by the framework in Section 3.

In the “**Control**” group, participants are told nothing about whether future participants will know if they email Congress. In the “**Invitation**” group, we tell participants that if they email

¹⁴The Online Supplement can be found at <https://sites.google.com/view/lucy-page/research>.

¹⁵Wave-1 participants in a given demographic cell—defined by the interaction of gender, 4-year college attainment, 15-year age groups, and state groups (Appendix C.2)—are randomly paired with Democrats or Republicans in a randomly matched Wave-2 demographic cell. Wave-1 participants see a profile showing their paired Wave-2 matches, including their political party and that they believe climate change is human-caused (Appendix Figure B2).

Congress, we will send their paired Wave-2 participants an invitation saying that the Wave-1 participant emailed Congress and urging the recipient to join in action (Figure 1a); Wave-2 targets would see this invitation **before** deciding whether to email Congress. In contrast, in the “**Tell-after**” group, we tell participants that if they email Congress, we will show their paired Wave-2 participants a profile saying that the Wave-1 participant emailed Congress (Figure 1c), but only **after** their Wave-2 matches decide whether to email Congress themselves. If Invitation or Tell-after participants do not email Congress, their Wave-2 matches see nothing about whether they or others did so. Appendix Figures B3 and B4 show these treatments.

Here, the key design feature is that the only difference between the Tell-after and Invitation groups is that Invitation participants’ choice to email Congress can affect whether their paired Wave-2 participants do the same; comparing email rates between these groups then identifies whether Democrats try to engage others in climate advocacy. In contrast, any gaps in email rates between the Tell-after and Control groups may reflect any self- or social-image benefits of others knowing that you emailed Congress, as well as any efforts to affect Wave-2 participants’ behavior after the survey itself.

Can email Congress: After several comprehension questions, Wave-1 participants choose whether to email Congress. Interested participants opt into the email process, choose whether to continue after learning that the email form requires a mailing address, and finally write and send their emails.

4.2 Wave-1 empirical specifications

We estimate the following specification in the full Wave-1 sample:

$$Email_i = \alpha + \beta_1 Tell_i + \beta_2 Invite + \Phi X_i + \varepsilon_i \quad (4)$$

where $Email_i$ indicates that participant i emailed Congress, $Tell_i$ indicates that participant i is in the Invitation or Tell-after groups, $Invite_i$ indicates that participant i is in the Invitation group, and X_i is a vector of controls. Here, β_1 captures the effects of knowing that future participants will be told if one emails Congress, while β_2 tests for influence motives: are Democrats more or less likely to email Congress when doing so can affect whether others do the same?

Outcome variables: Our primary Wave-1 outcome variable is whether participants match to an email sent to Congress via our embedded form. We match participants to email records by combinations of first name, email address, treatment status, state, and the time of survey completion. We successfully match 94% of all recorded emails sent via the embedded Wave-1 forms to participants, with no differential success by treatment group (see Appendix B.2 for details). We also estimate effects on whether participants initially opt in to the process of emailing Congress; 73% of those who initially opt into this process ultimately match to an email record.¹⁶

¹⁶8% of those who start the email process opt out after learning that the form will ask for their address. 20% of those

Control variables: X_i includes participants’ demographics, baseline beliefs about climate and politics, and political engagement. Appendix B.2 details these controls, and Appendix B.4 shows that our results are robust to excluding them.

4.3 Wave-1 recruitment, sample summary, and experimental fidelity

Recruitment: We recruited Wave-1 participants with ads on Facebook, Instagram, and Twitter for a survey on climate change; we then screened for members of the Democratic party who believe that climate change is mostly human-caused, along with the demographic requirements described in Section 2.1. We ran the Wave-1 experiment and all other experiments in this project between March and October 2023. In total, 29,596 participants consented to the Wave-1 survey, 12,540 of whom met our screening criteria. We randomized 8,937 participants into the Wave-1 sample and 2,004 participants into mechanism experiments described below. Appendix B.3 details Wave-1 recruitment.

Attrition: While the Control group advances immediately from randomization to deciding whether to email Congress, the Invitation and Tell-after groups see treatment information and comprehension questions with an average duration of 2.6 minutes before deciding. Thus, we directly observe whether only 87% of the Invitation and Tell-after groups email Congress, with no differential attrition between them, compared to 99% of the Control group (column 1, Appendix Table B1).

Our main analysis assumes that participants who leave the survey between learning about the opportunity to email Congress and deciding whether to do so would not have emailed Congress had they continued, letting us define the email outcome for all randomized participants. All results are robust to restricting to participants for whom we observe active email choices (Appendix B.4).

Sample summary and balance: As we described in Section 2.1, the Wave-1 sample reflects a mainstream environmental movement that remains predominately white, affluent, and educated (Table 1). The Wave-1 sample is largely balanced on baseline characteristics across treatment groups (Appendix Table B2), and our results are robust to including baseline controls for the small imbalances that remain (Appendix B.4).

Set-up comprehension and attention: Most Wave-1 participants correctly understand whether their action can influence others. At the end of the survey, Invitation participants are 76pp and 51pp more likely than Tell-after participants to say that future participants will see that they emailed Congress *before* making their own choice and that their choice to email Congress could affect future participants’ email choices, respectively (Appendix Figure B5).

Participants also attend to their Wave-2 matches’ demographics, so they may differentially seek to influence matches with certain traits. In multiple-choice questions at the end of the Wave-1

who still say they want to email Congress at this point don’t match to an email record, either because they don’t send an email once they reach the form itself or because their email used a different name or email address.

survey, 75% or more of both Invitation and Tell-after participants correctly identify their Wave-2 matches' party leanings, gender, age, education, and state group (Appendix Figure B6a).

4.4 Wave-1 results: Democrats value mobilizing others

We find in the Wave-1 experiment that Democrats do indeed try to mobilize others to email Congress, with suggestive evidence that they may differentially try to mobilize co-partisans.

4.4.1 Evidence for influence motives

Table 2 presents the impacts of the Wave-1 treatments on whether participants opt into the email process (columns 1 through 3) and ultimately send an email to Congress (columns 4 through 6). Columns 1 and 4 show effects for all Wave-1 participants, while the others restrict to those paired with Wave-2 Democrats or Republicans.

The Invitation effect: Results for both outcomes suggest that Democrats try to recruit others to email Congress. The Tell-after treatment alone has large effects, making Democrats 12pp (26%) more likely to start the email process and 13pp (43%) more likely to match to an email record (columns 1 and 4). However, Invitation participants are an additional 3.3pp more likely than Tell-after participants to start the email process and 2.7pp more likely to send an email, about 7% and 9% of the relevant control means, respectively. Thus, Wave-1 Democrats recognize and internalize their externalities on others' political action: they are more likely to email Congress when doing so can inspire others to do the same.

Several forms of evidence help to validate that the differential Invitation effect captures influence motives. First, Invitation participants are 8.4pp (49%) more likely than Tell-after participants to report in free-text responses that our treatments affected them because they could motivate others (Appendix Table B3).¹⁷ Second, we find no evidence that the β_3 effect reflects key alternate mechanisms: that the Invitation treatment changes participants' beliefs about overall rates of action or induces differential social-image benefits from others knowing that one emailed Congress.¹⁸

¹⁷A treatment-blind academic hired by the authors hand-coded 1,800 Tell-after and Invitation participants' free-text responses for if and why knowing that others would see that they emailed Congress affected their action. The only other notable difference in answers between treatment groups is that Invitation participants were 5.3pp less likely to say that they treatment did not affect them because they don't care what others think about them. The Online Supplement includes the full coding schemes for these and all other free-text variables.

¹⁸If Invitation participants expect other participants to also have opportunities to influence others' action, they may differentially revise their beliefs about aggregate email rates in our survey; these beliefs may then affect their action if they perceive a different descriptive norm or see political action as a game of strategic substitutes or complements (Miller and Prentice, 2016; Gerber et al., 2018; Cantoni et al., 2019; Hager et al., 2022, 2023; Andre et al., 2024). However, the Invitation treatment has no differential effect on participants' incentivized beliefs on the share of participants who chose to email Congress via our form (column 4, Appendix Table B1). Another natural concern is that the Invitation treatment induces differential self- or social-image benefits that are not derived from trying to mobilize others if, for example, it provides cover to boast that one emailed Congress. This risk is low, as Tell-After participants also have strong cover for acting on social image: free-text responses suggest that many in the Tell-After

Influence motives in the Tell-After effect: While the Invitation versus Tell-After effect shows that influence motives exist, this gap likely under-estimates the total effects of influence motives on email rates, as the Tell-after effects also capture attempts to affect targets’ action after the survey. Indeed, 17% of Tell-after participants say that the treatment affected them because it allowed them to motivate others, about 66% of Tell-after participants who say it affected them overall (Appendix Table B3). Moreover, 54% of Tell-after participants correctly say that their decision to email Congress could in theory affect Wave-2 participants’ behavior after the survey, with most others saying they don’t know, and 56% say it would be at least somewhat likely to do so (Appendix Figure B7).

The large Tell-After effects likely also derive in part from self- or social-image concerns, which are strong drivers of action in other political and non-political domains (Bursztyn and Jensen, 2017; Dellavigna et al., 2017). When asked whether the treatment affected their email choice, about 10% of Tell-after participants say that it activated social-image concerns or made them feel accountable, about 30% of those who say it affected them overall (Appendix Table B3).

Robustness: Our main Wave-1 results are robust to excluding participants who misunderstand the experimental set-up or guess the study purpose,¹⁹ to changing controls, and to correcting for the small differences in experimenter demand effects between the Invitation and Tell-after groups (Appendix B.4).

4.4.2 *Democrats may act to influence Democrats more than Republicans*

The Wave-1 experiment provides suggestive, though underpowered, evidence that Democrats differentially act to mobilize Wave-2 Democrats rather than Republicans. Here, we test for differential β_2 coefficients in Equation 4 when Wave-1 Democrats are randomly paired with Wave-2 Democrats or Republicans. Participants are 4.9 pp (11%) more likely to opt into the email process and 3.5 pp (12%) more likely to send an email when doing so could influence whether Wave-2 Democrats do the same (columns 2 and 5, Table 2), while being only 1.7 pp (4%) and 2.0 pp (6%) more likely to start to or ultimately email Congress, respectively, when doing so could influence Republicans (columns 3 and 6). We cannot reject that these coefficients are equal across party, but they suggest that Democrats may differentially act to build the climate movement within, rather than across, party lines. The next experiment explores these partisan gaps in detail.

group see telling others that they emailed Congress as an opportunity to show the strength of the climate movement or mobilize them after the survey (Appendix Table B3), and any individual’s choice to email Congress could always be attributed to intrinsic rather than image drivers. Participants in the Invitation group do not differentially say that treatment affected their behavior via social-image concerns in free-text responses (Appendix Table B3).

¹⁹In responses hand-coded by a treatment-blind academic, only 2-3% of Tell-After and Invitation participants guess that the study purpose was to test whether people try to influence others (Appendix Table B4). Most guess that the purpose was to measure climate actions and beliefs, to mobilize political action, or to estimate spillovers from invitations on others’ action. The Online Supplement details the coding scheme.

5 The Target-choice (TC) experiment: Whom do Democrats try to recruit?

While the Wave-1 experiment shows that Democrats try to mobilize climate advocacy overall, it lacks power to test *whom* they try to recruit. Thus, in a sub-experiment embedded in Wave 1, we generate this power by asking participants to make a series of binary choices between inviting *particular* other survey-takers to email Congress and delegating donations to carbon offsets from our research funds. This “Target-choice (TC) experiment” shows that Democrats are much more likely to try to recruit co-partisans than Republicans, even when they know that all believe climate change is human-caused. This partisan outreach gap dwarfs differential outreach by any other trait.

5.1 Target-choice experimental structure

The TC sample: We embed the TC experiment in the Control group of the Wave-1 experiment, inviting all Control participants who emailed Congress during the survey to take an additional 15-minute survey section (see Figure 3 for the project roadmap). The TC sample then captures “inframarginal” climate activists—those who email Congress when they do not have opportunities to tell or mobilize others.

Binary invitation choices: We pair each TC participant with a group of 20 real past study-takers—varying in their politics and other demographics—each of whom we will recontact to take a second survey during which they can choose to email Congress.²⁰ Each TC participant will be randomly matched with one these 20 others, who will see a profile of the TC participant when they return for our follow-up survey. For each possible match, participants choose which of two profiles of them that match would see, if paired:

- *Option 1. “Basic profile” and carbon offsets:* We will show the returning match the TC participant’s basic demographic profile and add some amount (\$3 to \$6, randomized across participants) to a carbon-offset purchase by our research team.²¹
- *Option 2. “Extended profile:”* We will show the returning match an extended profile that both includes the TC participant’s demographics and says that they emailed Congress.

Thus, participants trade off between offset donations and telling others that they emailed Congress. They make these choices while seeing demographic profiles for each possible match, so they may

²⁰These past study-takers were recruited before starting the Wave-1 experiment or from those who started but were ineligible for the Wave-1 experiment (e.g. they were Republicans or leaned towards but were not members of the Democratic party). We do not analyze these possible matches’ behavior.

²¹We benchmark these dollar amounts as offsetting approximate emissions from driving a new 2WD SUV between cities near the participants’ state of residence, using conversions published by Clear (<https://clear.eco/>), the company from which we purchase offsets. While carbon offsets may avert less than their stated emissions reductions (West et al., 2020; Calel et al., 2025), participants value offsets in this context (see footnote 23).

choose differentially by match traits. We describe the basic and extended profiles in more detail below, and Appendix Figures C1 through C3 show how this set-up appears to participants.

Identifying influence motives by randomizing profile timing: As in the Wave-1 experiment, we identify influence motives using variation in *when* possible matches would see that the TC participant emailed Congress. Within participant, we randomize whether each possible match would see their profile *before* or *after* themselves choosing whether to email Congress, motivating this variation by saying that matches would see their profiles at different times based on the “structure of the survey they’re signed up to complete.” An attribute labeled “Action status” in matches’ profiles (Appendix Figure C3) communicates this timing: those who would see one’s profile *before* versus *after* themselves deciding whether to email Congress are labeled as “Hasn’t been asked” and “Already decided,” respectively.

We then identify attempts to mobilize others by testing whether TC participants are more likely to choose extended profiles over offsets when targets would see their profile *before* rather than *after* deciding themselves whether to email Congress.

The appearance of basic and extended profiles: For each of TC participants’ 20 choices, the “basic profile” they could pass on is identical to the demographic profile that they created at the start of the Wave-1 experiment (Figure 1b). In contrast, “extended profiles” differ by whether a possible match would see them before or after themselves choosing whether to email Congress. A match seeing an extended profile *before* deciding to email Congress would see the profile shown in Figure 1a—including an invitation to join in action—while one seeing an extended profile *after* deciding would see the profile shown in Figure 1c. These are the same profiles passed on in the Invitation and Tell-after treatments in the Wave-1 action experiment (Section 4).

Testing differential targeting by match traits: TC participants’ possible matches are real people who vary in a range of traits appearing in their profiles (Appendix Figure C3), allowing us to identify whether Democrats differentially try to mobilize climate advocacy by politics, gender, age, whether or not matches have a four-year college degree, and state or region of residence. We particularly focus on whether TC participants differentially try to recruit co- or counter-partisans.²²

By state politics: Another key test of interest is whether TC participants strategically try to recruit climate action in states where it could be more impactful. We construct 13 groups of geographically close states that most participants in an online sample of Democrats classify into one of three climate-politics categories: “blue states,” where legislators would likely support a climate bill even

²²One concern is that a possible match will only see an extended profile if they return for a follow-up survey, while carbon-offset donations could be made regardless of attrition. If TC participants expect higher attrition for some demographic groups, these beliefs could drive spurious heterogeneity in participants’ choices across matches. We eliminate this bias by telling TC 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.

if few constituents advocated for it; “red states,” where legislators would be unlikely to support a climate bill even if constituents advocated for it; and “purple states,” where legislators could be convinced to vote for a climate bill if enough constituents supported it (Appendix C.2). Matches’ profiles show the state-group in which they live, letting us both test whether Democrats differentially recruit emails in states where they may be particularly impactful and estimate all partisan outreach gaps conditional on targets’ state politics.

The role of matches’ climate beliefs: We randomize TC participants to see profiles of possible matches that either show that all matches believe climate change is human-caused or hide these beliefs (Appendix Figure C3). This cross-participant variation lets us test how inferences about climate beliefs drive any partisan gap in Democrats’ attempts to recruit others; in all other experiments in this paper, we show that all recruitment targets believe climate change is human-caused.

5.2 Target-choice empirical specifications

We estimate partisan gaps in TC participants’ mobilization attempts as follows:

$$Extended_{ij} = \alpha_i + \beta_1 Before_{ij} + \beta_2 Before_{ij} * Dem_{ij} + \beta_3 Dem_{ij} + \Theta P_{ij} + \varepsilon_{ij} \quad (5)$$

where $Extended_{ij}$ indicates that participant i chose to show possible match j an extended profile (saying that they emailed Congress) over carbon offsets, $Before_{ij}$ indicates that possible match j would see participant i ’s profile before deciding whether to email Congress, and Dem_{ij} indicates that match j leans towards the Democratic party. β_1 captures whether TC participants are more or less likely to tell a Republican match that they emailed Congress when they can do so before, rather than after, that match’s email choice, and β_2 tests whether this “influence” gap differs when paired with Democrats. Finally, β_3 tests whether participants are more likely to tell co-partisans that they emailed Congress when it cannot affect their in-survey action.

Control variables: Each TC participant makes 20 binary choices, so our main regression specifications control for participant fixed effects, α_i . We also control for P_{ij} , a vector of other match features. P_{ij} only includes fixed effects for choice number in our main specifications, but our results are robust to controlling for all other demographics in match j ’s profile (Appendix C.4).

5.3 Target-choice sample recruitment, summary, and fidelity

Attrition: In total, 1,350 participants started the TC survey after emailing Congress from the Wave-1 Control group (see Appendix C.3 for recruitment details). Just before participants made choices for each of their 20 possible matches, we randomized the 1,109 remaining participants to see match profiles that either show or hide matches’ climate beliefs. A total of 1,023 participants (76% of those who start the experiment and 92% of those randomized) completed profile choices

for all 20 possible matches and compose our main sample, with no differential attrition by whether participants see matches' climate beliefs (column 1, Appendix Table C1).

Sample description and balance: The TC sample captures “inframarginal” climate activists—those who email Congress from the Wave-1 control group. Consistent with this selection, the TC sample is more concerned about climate change and has higher baseline political engagement than the full Wave-1 Control group, as well as being somewhat younger, better educated, and more likely to be women (columns 1-3, Appendix Table C2). The TC sample is largely balanced between those who see or do not see information on matches' climate beliefs (columns 4-6, Appendix Table C2). Participant fixed effects control for any remaining baseline differences.

Set-up comprehension: We invest heavily in ensuring that participants understand the TC set-up. After describing the set-up once, we review it while asking a series of 8 incentivized comprehension questions and correcting participants' answers. We detail these questions in the Online Supplement. Participants initially answer an average of 5.8 questions correctly (Appendix Figure C4), and our results are robust to restricting to participants who initially answer at least half correctly (Appendix C.4).

5.4 Target-choice results: Democrats differentially try to mobilize co-partisans

TC participants try to mobilize political action: As in the Wave-1 experiment, TC participants are strongly motivated to recruit others to email Congress. In particular, participants are much more likely to choose an extended profile over carbon offsets when matches would see their profile *before* rather than *after* choosing whether to email Congress, and thus when doing so could affect matches' political action (Table 3). While participants choose extended profiles over carbon offsets for about 23% of matches who would see their profile after deciding whether to email Congress, they are 42 pp and 48 pp more likely to do so, without and with information on matches' climate beliefs, respectively, when matches would see their profile before choosing whether to email Congress (columns 1 and 3, Table 3).^{23,24}

Large partisan gap in outreach: We find that while TC participants do try to mobilize both Democrats and Republicans to email Congress, they are much more likely to try to recruit co-partisans. When climate beliefs are hidden (column 2, Table 3), knowing that a Republican match

²³Whether we should interpret these effects as evidence of strong or weak influence motives depends on whether TC participants value offsets. We randomize 132 participants to a variant of the TC experiment in which they also have the option to pass on a basic profile and take home a gift card in the same amount as the carbon-offset donation for each possible match (Appendix C.5). Participants choose the basic profile and offset donation in an average of 10.5 choices, while choosing the gift-card in only 1.2 choices; about 80% of participants never choose the gift card. Most participants thus value offsets more highly than the same amount in an online gift-card.

²⁴We cannot map this effect to a numerical willingness to pay in offset dollars to try mobilizing others: while we randomized carbon-offset amounts in an effort to do so, participants' profile choices are inelastic with respect to these amounts (Appendix Figure C5).

would see their profile *before* deciding whether to email Congress makes TC participants 30 pp more likely to choose an extended profile. However, this influence gap is 24 pp (81%) higher when matches are Democrats. The partisan outreach gap falls but stays substantial even when participants know that all matches believe climate change is human-caused: Democrats remain 11.5 pp (27%) more likely to try to recruit co-partisans (column 4, Table 3).

Alongside these partisan gaps in influence attempts, Democrats are more likely to tell Democratic matches that they emailed Congress even when it cannot affect those matches' email choices. When climate beliefs are hidden or revealed, respectively, TC participants are 13.5 pp (82%) and 3.7pp (18%) more likely to show extended profiles to Democrats than Republicans who would see them *after* their own email choice. As in the Wave-1 experiment, these partisan gaps could arise from differential social-image concerns or efforts to affect matches' action after the survey.

Targeting by non-political traits and similarity: The large gap in Democrats' attempts to mobilize action along versus across party lines dwarfs recruitment gaps by all other match traits (Panel A, Figure 4). When TC participants lack information on matches' climate beliefs, they more strongly try to recruit women, those who live in blue or purple states, and those with 4-year college degrees. These gaps are much smaller than Democrats' differential efforts to mobilize other Democrats, however, and they shrink substantially or disappear when participants know that all matches believe climate change is human-caused. Notably, there is no evidence that Democrats differentially try to mobilize advocacy in red or purple states, where they might perceive this advocacy to be more impactful, when climate beliefs are revealed.²⁵

Targeting by shared politics also dwarfs differential recruitment by participants' non-political similarity with possible matches (Panel B, Figure 4). When TC participants lack information on matches' climate beliefs, they are 3pp more likely to try to recruit matches with whom they share an additional non-political trait, driven especially by differential attempts to recruit those with the same educational attainment or from the same state group. However, these effects are much smaller than differential influence attempts by political match, and they fall and become statistically insignificant when participants know matches' climate beliefs. These patterns suggest that TC participants use matches' demographics as signals of their likely beliefs about climate change, ultimately targeting outreach based on climate beliefs and political affiliation.

Robustness: The TC results are robust to changing controls and excluding participants who misunderstand the experimental set-up, guess the study purpose,²⁶ or express concern that Republicans'

²⁵We also find little evidence that TC participants differentially target Democrats versus Republicans by state politics (Appendix Table C3). If anything, Democrats may be especially unlikely to try recruiting Republicans in red states, where we might expect their advocacy to be especially impactful.

²⁶In responses hand-coded by a treatment-blind academic, 23% of TC participants guess that the experiment aimed to test whether people try to influence others, but only 25% of those (6% of the full sample) guess that we were interested in the role of politics in these motives (Appendix Table C4). The Online Supplement details this coding.

emails would oppose climate policy (Appendix C.4).²⁷

6 Mechanism experiments: Decomposing partisan influence gaps

Why do Democrats differentially try to mobilize other Democrats for climate action, even as—as seen in Section 2.2—they agree that a bipartisan climate movement would be more effective?

Here, we return to Section 3’s conceptual framework to decompose the beliefs and preferences that underlie Democrats’ reluctance to reach across party lines. Again, these gaps could be strategic, driven by Democrats’ beliefs that they have differential influence on co- or counter-partisans ($\Delta P(Email)$) or beliefs that emails to Congress from Democrats or Republicans could be differentially impactful ($V(Email\ Impact)$). On the other hand, outreach gaps could arise from affective polarization, if Democrats get differential affective returns from cooperating with a co- or counter-partisan ($A(Target\ Involved)$) or over the act of themselves reaching out to try recruiting a co- or counter-partisan ($A(Try\ Influence\ Email)$).

We find that both strategic beliefs and affective polarization play key roles. On the one hand, there are large partisan gaps in Democrats’ strategic $\Delta P(Email)$ beliefs: Democrats accurately expect their invitations to have about twice as much impact on Democrats’ than Republicans’ likelihood of emailing Congress. On the other hand, we find suggestive evidence that affective polarization matters in two key ways. First, *anticipated* affective polarization shapes Democrats’ strategic influence beliefs: they estimate that hiding their own politics from invitations to Republicans would make them three times as effective. Second, we find suggestive evidence for partisan gaps in $A(Try\ Influence\ Email)$, Democrats’ affective preferences for themselves trying to mobilize co- versus counter-partisans.

6.1 Large and accurate partisan gaps in influence beliefs: $\Delta P(Email)$

We first consider the role of Democrats’ beliefs about the impacts of invitations on the likelihood that Wave-2 Republicans and Democrats email Congress, $\Delta P(Email)$. We estimate the true impacts of invitations in the “Wave-2 experiment,” then elicit Democrats’ incentivized beliefs about these effects. We find that Wave-1 Democrats *correctly* anticipate that their invitations will have about twice as much impact on action by other Democrats than by Republicans.

6.1.1 The Wave-2 experiment: Estimating true $\Delta P(Email)$ effects

The Wave-2 experiment randomly assigns half of a large sample of Democrats and Republicans to see invitations from Wave-1 participants to join them in emailing Congress, testing the impacts of these invitations on whether Wave-2 participants email Congress via our embedded form.

²⁷Only 3% of participants express this concern in responses hand-coded by a blinded academic. Recall that we aim to test whether Democrats pursue allied cross-party action as much as possible. To that end, for example, we set the subject line of the email form to strongly support climate policy (Section 2.1.2).

See basic demographic profile of Wave-1 match: Wave-2 participants first build a basic demographic profile (Figure 1b). We next show them the comparable profile for a Wave-1 participant, saying that they’ve been randomly paired to give them a sense of who else is involved in our study (see Appendix Figure D1a). To encourage Wave-2 participants to attend to their match, we ask them to rate and explain how similar they are to this recent participant.

Treatment. Show participants 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. We then implement the invitation treatment, randomizing half of Wave-2 participants to see an invitation to email Congress from the same Wave-1 participant whose demographic profile they saw earlier (see Appendix Figure D1b); alongside, the survey states that when their Wave-1 match took our survey, they chose to contact Congress via our form and to pass on this profile.²⁸ To ensure that Wave-2 participants who see invitations do not infer that their own action will be shown to others, treated participants next see a slide stating that we will not tell any other participants whether they email Congress or not. All participants then decide whether to email Congress.

6.1.2 Wave-2 sample recruitment, summary statistics, and experimental fidelity

Recruitment: We recruited Wave-2 participants via ads on Facebook, Twitter, and Instagram; by redirecting Republicans or Democratic-leaning Independents from Wave-1 recruitment; and from Qualtrics, which aggregates market-research panels and online samples. All participants must believe that climate change is mostly human-caused and meet the demographic restrictions described in Section 2.1. In total, 8,685 participants were randomized to a Wave-2 treatment, of whom 3,002 lean Republican and 5,683 lean Democratic. We recruited nearly all Republicans in our sample from Qualtrics, while recruiting about 85% of Democrats from social media. Appendix D.2.1 details Wave-2 recruitment. We will show that our main Wave-2 results are statistically indistinguishable across sample sources.

Attrition: 92% of those randomized to a Wave-2 treatment advance through choosing whether to email Congress, with no differential attrition by treatment (column 1, Appendix Table D1). We define the experimental sample as the 7,981 participants for whom we observe email choices.²⁹

Comparing Wave-2 Democrats and Republicans: Democrats and Republicans in the Wave-2 sample differ notably both in demographics and in political engagement and beliefs (columns 1-3,

²⁸To 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 survey-takers in the Wave-2 participant’s demographic group. The Online Supplement describes this matching process in detail.

²⁹Note that this differs from the Wave-1 experiment, where we define the experimental sample as all those randomized to a treatment arm and then assume that those who attrit after randomization (and seeing the upcoming email opportunity) would not have emailed Congress. We make that assumption in Wave 1 to deal with differential attrition across treatment arms, which is a not a problem in Wave 2. Both experiments’ results are robust to either sample definition (Appendix Sections B.4 and D.2.2).

Appendix Table D2). Wave-2 Democrats are more educated, older, wealthier, and more likely to live in blue or purple states than Wave-2 Republicans, as well as reporting higher baseline political engagement, concern about climate change, and preference for friends of their own party.³⁰ We will show that the differential impacts of Democrats’ invitations on Wave-2 Democrats and Republicans cannot be explained by basic demographic gaps between these groups.

Balance: The Wave-2 sample is balanced across treatment arms both among Democrats and Republicans (columns 4-9, Appendix Table D2).

Set-up comprehension and attention: Wave-2 comprehension is high (Appendix Figure D2). Treated participants are 60pp more likely to state that their paired Wave-1 participant emailed Congress when they took our survey, and even when treatment participants state that they don’t know whether their Wave-1 match emailed Congress, they guess that they did so with higher probability than do control participants.

Wave-2 participants also attend to matches’ demographics, so they may react differentially to invitations by Wave-1 traits. At the end of the survey, participants correctly identify matches’ party, gender, age, and state group over 85% of the time (Appendix Figure B6b).

6.1.3 Wave-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:

$$Email_i = \alpha + \beta Treatment_i + \Phi X_i + \varepsilon_i \quad (6)$$

where $Email_i$ indicates that participant i emailed 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, β captures the true impacts of invitations on the likelihood that participants email Congress, $\Delta P^*(Email)$ in our conceptual framework. We estimate this regression separately for Wave-2 Democrats and Republicans.

As in the Wave-1 experiment, our primary outcome is whether participants match to a record for an email sent via our form. We successfully merge 91% of all emails sent via the embedded

³⁰While some of these gaps, such as in educational attainment, mirror demographic gaps across parties in national samples (ANES, 2021), others diverge. For example, Democratic- and Republican-leaning report about equal political engagement in national samples (Oliphant, 2018). These differential gaps in our survey in part reflect the differential selection induced among Democrats and Republicans 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 Democrats and Republicans in the Wave-2 sample also arise in part from the fact that we largely recruited Democrats from ads about climate change on social media and recruited Republicans from Qualtrics. When we restrict to participants recruited from Qualtrics (Appendix Table D3), Democrats more closely match Wave-2 Republicans on income, education, and baseline political engagement. However, they remain more highly educated, wealthy, concerned about climate change, politically engaged, and affectively polarized.

forms to Wave-2 participants, with no differential success by treatment group (Appendix B.2.1). We also present impacts on whether participants initially opt into the email process. Our controls match those in Wave 1 (Section 4.2), in addition to indicators for recruitment source and whether participants identify as members of (rather than Independents who lean towards) their respective political parties.

6.1.4 Wave-2 results: Estimating $\Delta P^*(Email)$

Invitations have larger absolute impacts on Democrats: Invitations make Wave-2 Democrats about 5.8pp more likely to email Congress, a 23% increase over a control mean of 25% (left figure, Panel A of Figure 5). In contrast, invitations make Wave-2 Republicans only about 2.1pp more likely to email Congress, a 25% increase over the 8% of Wave-2 Republicans who email Congress in the control group.³¹ We reject ($p = 0.023$) that invitations have the same absolute impact on whether Democrats and Republicans match to email records.^{32,33}

Invitations' impacts vary with other recipient and match traits (Panel B of Figure 5): In addition to having larger impact on Democrats, Wave-1 invitations have larger impacts on action for Wave-2 women and those aged 60 to 79 ($p = 0.027, 0.067$). Notably, Wave-2 participants seem to also respond more to invitations from senders who are like them in non-political ways: invitations' impacts rise by about 1.4pp for each non-political trait (e.g. age, gender, educational attainment, or state) that Wave-1 senders and Wave-2 recipients share ($p = 0.14$). This effect is driven primarily by the role of shared age group and state (Appendix Figure D3).

Robustness: The Wave-2 results are unchanged when we exclude participants who misunderstand the experimental set-up or guess the study purpose,³⁴ change control variables, and control for experimenter demand effects (Appendix D.2.2).

6.1.5 The belief sample: Estimating Democrats' beliefs about $\Delta P(Email)$

What do Wave-1 Democrats believe about $\Delta P(Email)$, the impact of invitations on Wave-2 action?

³¹Appendix Table D4 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 Republicans 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 other Wave-2 analysis pools respondents recruited from the two sources.

³²Proportionally, Wave-1 Democrats' invitations have very similar impacts on climate advocacy by Democrats and Republicans. While the proportional effects on action could shape the emotional returns to recruiting co- or counter-partisans to act, the *absolute* impacts of invitations on emails are what determine the strategic value of inviting others to join in climate action (see footnote 12).

³³The Online Supplement explores the mechanisms by which invitations increase action. These effects are best explained by invitations' impacts on beliefs about the share of other study participants who emailed Congress, consistent with the idea that invitations work by changing perceived norms.

³⁴In responses hand-coded by a treatment-blind academic, only 7% of the Wave-2 treatment group guess that the study purpose is to estimate spillovers from invitations on other's action (Appendix Table D5). Most guess that the purpose is to measure climate actions and beliefs or to mobilize action. See the Online Supplement for coding details.

The belief sample: We estimate Democrats’ beliefs for $\Delta P(Email)$ in a “belief sample” that we recruit alongside Wave 1. Participants complete the Wave-1 experimental survey through seeing the upcoming opportunity to email Congress and are then randomized to complete these belief elicitation rather than participate in the main experiment.³⁵

Estimating beliefs: To measure $\Delta P(Email)$ beliefs, we ask participants to estimate how many out of 100 Democratic and 100 Republican participants in a certain Wave-2 demographic cells would email Congress in each of two cases: if they did or did not see invitations from a particular demographic group of matched Wave-1 Democrats. In total, 397 participants complete all four belief elicitation, of whom we incentivized 194 for accuracy (Appendix Figure D4).³⁶ Appendix Figures D5 through D7 show the belief-elicitation process. We analyze incentivized participants throughout this section, estimating the following regressions:

$$ProbEmail_{ijp} = \alpha_i + \beta Invited_{ijp} + \varepsilon_{ijp} \quad (7)$$

where $ProbEmail_{ijp}$ gives participant i ’s guess of how many of 100 participants in group j would email Congress in elicitation p , and $Invited_{ijp}$ indicates that elicitation p is a case in which group j would see invitations to act. α_i are individual fixed effects. β captures Democrats’ average estimates for invitations’ impacts on Wave-2 participants’ likelihood of emailing Congress, $\Delta P(Email)$. We estimate this regression separately on elicited beliefs about action by Wave-2 Democrats and Republicans, then test for equality in β across the two parties.

6.1.6 Results: Democrats correctly estimate ΔP by politics, but neglect non-political traits.

Democrats’ average $\Delta P(Email)$ beliefs are strikingly close to the true impacts of invitations on Democrats and Republicans. The belief sample estimates that Wave-1 invitations would make other Democrats about 6.2pp more likely to email Congress, while making Republicans only about

³⁵Before undertaking these belief elicitation, this sample completed the first iteration of the email-valuation experiment described in Section 6.3.2. The final belief sample matches Wave-1 on climate beliefs and political engagement, while differing on some demographics (Appendix Table A1). Like the main Wave-1 sample, the belief sample is less politically engaged and concerned about climate change than the Target-choice sample. Note that the belief sample differs slightly from the main Wave-1 sample, despite randomization, because of attrition during the email-valuation experiment and belief elicitation. Appendix Figure D4 summarizes belief-sample treatment assignment and attrition.

³⁶We elicit participants’ beliefs for the impacts of invitations between demographic groups that were actually randomly paired between Waves 1 and 2, letting us measure the truth for each participant guess. 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. We incentivized only half to test for the role of motivated reasoning in partisan gaps in influence beliefs (Bullock and Lenz, 2019; Zhang and Rand, 2023).

3.2pp more likely to do so (right figure, Panel A of Figure 5).^{37,38} While Democrats overestimate overall rates of action, we cannot reject that these average $\Delta P(\text{Email})$ beliefs equal the true impacts of invitations on action for both Wave-2 Democrats and Republicans ($p = 0.81$ and $p = 0.48$, respectively).³⁹ In other words, Democrats correctly anticipate that invitations will have about twice as much absolute impact on whether co- versus counter-partisans email Congress. We also cannot reject that Democrats’ beliefs are correct on average when we re-weight the Wave-2 sample to match the demographics of the recipients about whom the belief sample makes predictions, though these estimates are less precise (Appendix Figure D10). This accuracy stands in stark contrast to a large literature documenting misperceptions of others, including along partisan lines (Bursztyn and Yang, 2022).

While belief-sample Democrats correctly predict partisan influence gaps, they attend less to or misperceive the offsetting role of non-political recipient traits that also drive invitation effects (Panel B, Figure 5). In particular, we can reject at the 10% level that Democrats correctly estimate the differential impact of invitations on recipients who are women, live in purple states, are over age 60, or match the sender on an additional non-political trait.

6.2 Second-order affective polarization shapes $\Delta P(\text{Email})$ influence beliefs

While Democrats’ outreach gaps may be in part strategic, driven by their correct beliefs that cross-party outreach is less effective ($\Delta P(\text{Email}|\text{D}) > \Delta P(\text{Email}|\text{R})$), we find in our next mechanism experiment that these strategic influence beliefs are crucially shaped by a context of extreme affective polarization. In particular, Democrats expect to have less influence on counter-partisan action because of *anticipated* affective polarization, or because they expect Republicans to react negatively to them as Democrats. We see this in a round-two Target-choice (TC) experiment with one key variation: we randomly give some Democrats the option to hide their party leanings from the profiles they pass on to each possible match.

6.2.1 Experimental structure and sample

The round-two TC experiment follows the same procedure as the main TC experiment described in Section 5.1, except that we randomize half of round-two TC 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. Participants with the option to hide politics choose whether to do so separately for

³⁷Besides calculating average values for $\Delta P(\text{Email})$ in Equation 7, we use each participant’s multiple reports to calculate their individual estimates of $\Delta P(\text{Email})$ for each target, though these estimates are noisy (Panel A, Appendix Figure D8). Participants are 16pp (22%) less likely to expect $\Delta P(\text{Email})$ to be positive and 14pp (78%) more likely to expect it to be negative for Republican than Democratic targets (Panel B, Appendix Figure D8). About 56% estimate that $\Delta P(\text{Email}|\text{D}) > \Delta P(\text{Email}|\text{R})$ in their matched Wave-2 group (Panel C, Appendix Figure D8).

³⁸We cannot reject that participants’ estimates for $\Delta P(\text{Email})$ are equal with and without accuracy incentives, but those without incentives estimate somewhat higher $\Delta P(\text{Email})$ for Republicans (Appendix Figure D9).

³⁹We replicate these belief estimates as part of the next mechanism experiment (Appendix Figure D15).

each possible match, alongside the choice to pass on a basic or extended profile. Appendix Figure D11 shows how we explain the choice to show or hide their politics to participants, and Appendix Figure D12 shows an example choice page. All participants in the round-two experiment see profiles showing that matches believe climate change is human-caused.

Recruitment and sample summary: In total, 995 participants complete all 20 binary choices and compose our main sample, with no differential attrition by treatment arm (column 3, Appendix Table C1). Appendix D.3 summarizes recruitment. Like the main TC sample, round-two participants are more concerned about climate change and politically engaged than the main Wave-1 sample, while they match the main TC sample on climate beliefs and political engagement (columns 9-10, Appendix Table A1). The sample is largely balanced across treatment arms (Appendix Table D6) and has high comprehension (Appendix Figure D13).

6.2.2 Results: The role of anticipated affective polarization

Democrats' choices in the round-two TC suggest that *anticipated* affective polarization plays a key role in the $\Delta P(\text{Email})$ influence beliefs that shape cross-party outreach.

Democrats hide their politics from Republican matches: While participants show their politics to about 91% of Democratic matches, regardless of *when* those matches would see their profile, they show their politics to only 34% and 44% of Republicans who would see their profiles before and after deciding whether to email Congress, respectively (Figure 6a). The fact that Democrats are more likely to hide their politics from Republicans overall could simply be explained by social-image concerns. However, their choices to differentially hide their politics when matches would see their profiles before choosing whether to email Congress suggest that Democrats strategically hide their politics in order to better mobilize Republicans' action.

Hiding politics closes gaps in influence beliefs ($\Delta P(\text{Email})$): Indeed, Democrats expect their invitations to have much higher impact on Republicans' action if they hide their own political leanings. At the end of the round-two TC survey, we randomize a subset of participants to estimate the probability that two of their possible matches would email Congress if they did or did not see an invitation; we randomize whether participants make these predictions for invitations that do or do not include their own political leanings. 96% complete all four elicitations and are included in our analysis; Appendix Figure D14 summarizes attrition.

Democrats with the option to hide their politics estimate that an invitation that hides their politics would make Republicans 11.7pp more likely to email Congress, while an invitation identifying them as a Democrat would increase Republicans' action by only 3.1pp (Figure 6b). On the other hand, they estimate that hiding their political leanings would somewhat decrease the impact of invitations on Democrats' action from 12.9pp to 9.7pp. In other words, Democrats expect Republicans

to respond less to their invitations precisely because they are Democrats. Indeed, they estimate that hiding their political leanings would close the gap in their ability to mobilize Democrats and Republicans.^{40,41}

Text responses point to anticipated affective polarization: In theory, Democrats might expect invitations that hide their politics to more strongly affect Republicans’ action because they expect Republicans to rationally respond more to the signal of an invitation from someone with whom they expect to share more prior beliefs. However, participants’ free-text explanations for whether and why they think people might respond differently to invitations that show or hide politics strongly point to second-order affective polarization. 71% say without prompting that they expect Republicans to react more negatively to invitations that identify the sender as a Democrat. Of these, 60% explicitly mention that Republicans would have a negative emotional reaction to the sender’s politics, while only 19% mention rational beliefs.⁴²

6.3 Suggestive evidence of partisan gaps in $A(\text{Try Influence Email})$

Not only does second-order affective polarization shape Democrats’ influence beliefs, but we find suggestive evidence that Democrats’ *own* affective polarization also holds back cross-party outreach. In particular, we find suggestive evidence for partisan gaps in $A(\text{Try Influence Email}|\text{D,R})$, the affective returns to themselves reaching out to try to recruit others along versus across party lines. The starting point for this claim comes again from the round-two TC experiment, where we find that Democrats continue differentially reaching out to co-partisans even when hiding politics from invitations eliminates partisan gaps in $\Delta P(\text{Email})$ beliefs.

6.3.1 Results: Hiding politics from invitations does not close partisan outreach gaps

Despite the fact that many TC participants choose to hide their politics from invitations to Republicans and believe that doing so would close the partisan influence gap, those with the option to hide politics are still much more likely to try to recruit Democrats (Table 4). When TC participants must show their politics, as in our main TC experiment, the *Before* effect—capturing attempts to

⁴⁰While these beliefs were not incentivized, we replicate these patterns in an incentivized sample recruited in June 2025 (Appendix Figure D15). Moreover, we show in the main belief sample (Section 6.1.5) that accuracy incentives do not change average belief estimates. Note that TC participants (who are more politically engaged and have just emailed Congress) report higher $\Delta P(\text{Email})$ influence beliefs than the average Democrat in our sample (i.e. those in this replication sample and the prior belief-elicitation sample, who are not selected on emailing Congress).

⁴¹We focus here on participants with the option to hide politics during the TC experiment because they are likely to have formulated more precise beliefs about the impacts of profiles with and without political leanings while making TC binary choices. Those without the option to hide politics also estimate that hiding politics would close the partisan influence gap (Appendix Figure D17e), but primarily by decreasing invitation effects on Democrats. Qualitative patterns in the incentivized replication sample (Appendix Figure D15) more closely match beliefs among participants with the option to hide politics (Figure 6b).

⁴²An academic hired by the researchers hand-coded the 237 responses here as falling in these categories. See the Online Supplement for detail on the coding scheme.

mobilize others—is 9.7pp (24%) larger for Democratic versus Republican matches (column 2). We cannot reject that this influence gap is equal to that measured in the main TC experiment. This gap falls only slightly when Democrats have the option to hide their politics from possible matches (column 4): Democrats remain 8.5pp (23%) more likely to try to recruit co-partisans, and we cannot reject equality between partisan outreach gaps with or without the option to hide politics.

In theory, the partisan-outreach gap could remain because Democrats continue showing their politics to about 40% of Republican matches, so the partisan gap in $\Delta P(\text{Email})$ is not fully closed.⁴³ However, the partisan outreach gap persists even among TC participants who always or almost always show their politics to Democratic matches and hide them from Republican matches, shown in columns 5 and 6 of Table 4, respectively. The $\Delta P(\text{Email})$ influence beliefs elicited from these subsets of participants suggest that they should therefore expect invitations to have equal impact on Democratic and Republican matches’ likelihood of emailing Congress (Appendix Figure D17), but they remain much more likely to reach out to fellow Democrats.

This pattern suggests, therefore, that partisan gaps in $\Delta P(\text{Email})$ influence beliefs do not fully explain Democrats’ reluctance to reach out across party lines. In our conceptual decomposition (Section 3), there must then be partisan gaps in Democrats’ preferences for *emails* from co- or counter-partisans—whether due to their beliefs about the effectiveness of emails in pushing for policy goals ($V(\text{Email Impact})$) or their affective preferences for cooperating with others in the climate movement ($A(\text{Target Involved})$)—or in their affective preferences for directly reaching out to mobilize others ($A(\text{Try Influence Email})$). In a final mechanism experiment, called the “email-valuation experiment,” we find no evidence for partisan gaps in either $V(\text{Email Impact})$ or $A(\text{Target Involved})$, so partisan gaps in $A(\text{Try Influence Email})$ are the best remaining explanation for these sticky partisan outreach gaps.

6.3.2 The email-valuation experiment: Testing $V(\text{Email Impact})$ and $A(\text{Target Involved})$

In our final mechanism experiment, which we call the “email-valuation experiment,” we test for gaps in how much Democrats value emails from co- and counter-partisans *when they can be obtained with certainty* by our research team. In the language of our conceptual framework (Section 3), we test for partisan gaps in Democrats’ overall valuation of emails sent, a combination of how much impact they expect the email to have, $V(\text{Email Impact})$, and any other preferences for cooperating with the letter writer, $A(\text{Target Involved})$. By combining estimates of any partisan gaps in expected email impacts, $V(\text{Email Impact})$, with any gaps in overall preferences for emails by

⁴³In Appendix D.3.2, we estimate that the remaining effective partisan gap in $\Delta P(\text{Email})$ influence beliefs among those with the option to hide politics is about 2.3pp. In contrast, the partisan gap in influence beliefs among TC participants without the option to hide politics is about 6.5pp (Appendix Figure D17e). Note that we do not use whether participants have the option to hide their politics as an instrument for influence beliefs, as it may also change any affective returns to trying to mobilize others, $A(\text{Try Influence Email})$.

Democrats and Republicans, we can then implicitly test for gaps in Democrats’ preferences for cooperating with co- and counter-partisans in the climate movement, $A(\textit{Target Involved})$.⁴⁴

Binary choices between letters and offsets: Here, we ask a sample of Democrats to make incentivized binary choices between carbon-offset donations and emails to Congress, sent with certainty, from a range of demographic groups. In particular, we show participants a series of profiles for 14 demographic groups, identified by political leanings, gender, college attainment, 15-year age bins, and the group of politically similar, nearby states in which they live, as in profiles throughout the experiment (e.g. Figure 1b). These profiles are evenly split between leaning towards the Republican and Democratic parties. For each demographic profile, participants then choose between two options, with explanations to participants shown in Appendix Figure D18:

- *Option 1. Carbon offset donation:* The research team will donate \$10 to carbon offsets.
- *Option 2. Enlist an email with certainty:* We will recruit a participant like the demographic group shown to email Congress about climate policy through our online form. We reiterate to participants that choosing this option means that we commit to enlisting an email-writer like that, and that it is easy for us to enlist someone like each group shown.

We incentivize these choices by saying that we will implement one choice for 30 participants.

Recruitment and sample summary: In total, 574 participants complete all choices in the email-valuation experiment (Appendix D.4 details recruitment).⁴⁵ The email-valuation sample was recruited after the main Wave-1 and TC samples, and participants are somewhat younger, more likely to be women, and more likely to have a college degree than the main Wave-1 experimental sample (columns 11-13, Appendix Table A1). This sample falls between the Wave-1 sample and the TC sample in climate beliefs, affective polarization, and political engagement.

Empirical specifications: We estimate the following regression in the email-valuation sample:

$$\textit{Email}_{ij} = \alpha + \beta_1 \textit{Dem}_{ij} + \beta_2 \textit{Woman}_{ij} + \beta_3 \textit{College}_{ij} + \sum_{a=2}^4 \gamma_a \textit{Age}_{ija} + \sum_{s=2}^3 \delta_s \textit{State}_{ijs} + \varepsilon_{ij} \quad (8)$$

where $\textit{Email}_{ij}$ indicates that participant i chose an email from demographic group j over a carbon-offset donation, and $\textit{Dem}_{ij}$, $\textit{Woman}_{ij}$, and $\textit{College}_{ij}$ indicate that members of demographic group

⁴⁴Note that in addition to setting $\Delta P(\textit{Email}|\textit{D}, \textit{R})$ equal to 1, we think of this experimental exercise as shutting down $(A(\textit{Try Influence Email}|\textit{D}, \textit{R}))$, Democrats’ affective returns to seeing themselves or being seen by others as trying to recruit climate action, because participants here are not themselves reaching out to co- and counter-partisans to recruit action. We describe the experimental set-up in more detail below.

⁴⁵In addition to this sample, we ran a first round of the email-valuation experiment with about 400 participants, as pre-registered. This first round suffered from several implementation errors, so we re-ran the experiment with an improved design. Appendix D.4 details the first round of the experiment and shows that our results throughout this section are robust to pooling data across both experiments.

j lean towards the Democratic party, identify as women, and have a 4-year college degree, respectively. $\{Age_{ija}\}$ are 15-year age bins, and $\{State_{ijs}\}$ are categories of state politics. The β , γ , and δ coefficients capture Democrats' relative preferences for emails to Congress from Americans with varying demographic traits and to legislators in states across the political spectrum.

6.3.3 Results: No evidence for partisan gaps in $A(Target\ Involved)$ or $V(Email\ Impact)$

No partisan gaps in email choices: Panel A of Figure 7 plots the coefficients estimated in Equation 8. When Democrats can obtain emails with certainty, they are equally likely to recruit emails over offsets from Democrats and Republicans. This suggests that that Democrats' do not differentially value *emails themselves* from co- versus counter-partisans, modeled in our simple framework as the sum of $A(Target\ Involved)$ and $V(Email\ Impact)$.

Indeed, there are no distinguishable gaps in choices over emails versus offsets for any demographic trait other than the states where email-writers live. Here, we see large gaps: Democrats are about 10pp less likely to choose an email over offsets from senders in blue states and 5pp more likely to choose an email from senders in purple states, both relative to red states. This pattern suggests 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, over blue states, where legislators may support climate policy regardless of citizen advocacy (Appendix C.2).⁴⁶

No partisan gaps in perceived email impacts ($V(Email\ Impact)$): Next, we test for partisan gaps in the two key components of Democrats' preferences for emails with certainty: $V(Email\ Impact)$ and $A(Target\ Involved)$. In theory, Democrats may be equally likely to choose emails from co- and counter-partisans either because they both perceive no differential impacts and derive no differential affective benefits by partisanship, or because of offsetting gaps in these two measures.

While our motivating survey suggests that Democrats expect emails from Republicans to more effectively promote climate policy than letters from Democrats in the aggregate, we find no evidence that Democrats in our sample attend to this differential impact when considering emails from individuals across the political spectrum. To elicit Democrats' beliefs for $V(Email\ Impact)$, we first ask them to imagine that a climate bill were introduced to Congress several months later. Next, we randomly show them one demographic profile and ask them to imagine that representatives read emails from 20 people in that group about the bill sent via our form. We then ask how the emails would affect the representatives' support for the bill, on a Likert scale from making them much less likely to much more likely to support it. We standardize these beliefs and regress them on the email-writer traits in Equation 8, plotting estimated coefficients in Panel B of Figure 7.

⁴⁶While we might expect participants to differentially prioritize emails from Democrats or Republicans by state politics, we find no partisan gaps in email choices in any state-politics category (Appendix Table D7).

We cannot reject that Democrats expect emails from individual Democrats or Republicans to be equally impactful overall. Combining the null partisan gap in overall email choices with the null partisan gap in perceived email impacts ($V(\textit{Email Impact})$), we find no evidence that Democrats have differential affective preferences to cooperate with co- versus counter-partisans in climate action ($A(\textit{Target Involved})$).⁴⁷

6.3.4 Wrapping up evidence

Put together, these patterns suggest that the sticky partisan outreach gaps in the round-two TC experiment cannot be explained by strategic beliefs. Rather, they seem to reflect partisan gaps in $A(\textit{Try Influence Email})$, Democrats’ affective preferences over themselves reaching out to mobilize others.⁴⁸ Consistent with this explanation, we find across both rounds of the TC experiment that the partisan outreach gap increases in participants’ own affective polarization, measured by their preference for being friends with other Democrats rather than Republicans (Appendix Figure D19). In other words, Democrats in the climate movement have *preferences* for within-party outreach that hold back the formation of broader coalitions, alongside accurate beliefs that within-party mobilization is more likely to succeed. These affective gaps arise even in our online setting with little scope for direct interaction and would likely be even starker in more real-world settings.

7 Conclusion

In an online experiment with over 20,000 participants, we construct an online network where Democrats, the base of the mainstream climate movement, have opportunities to invite Americans across states, demographic groups, and the political spectrum to join them in political climate advocacy. This set-up allows us to observe for the first time whether and how people try to expand political coalitions. We focus, in particular, on whether Democrats attempt to broaden the coalition by recruiting potential allies across the political aisle, or to recruit others like themselves.

We find that Democrats are motivated to engage others in climate action: they are more likely to email Congress themselves when doing so can inspire others to join them. Despite saying that a bipartisan climate movement would be more effective than a liberal-only movement, however, they

⁴⁷It is possible that Democratic influencers have different affective preferences to cooperate ($A(\textit{Target Involved})$) with Republican “compliers” in the email-valuation experiment (who email Congress when given an opportunity by our research team) than with Republican “compliers” in the TC experiment (who are induced to email Congress by an invitation). While these compliers are unlikely to be very different, any such discrepancy would just mean that we mistakenly attribute a partisan gap in $A(\textit{Target Involved})$ in the round-two TC experiment to partisan gaps in $A(\textit{Try Influence Email})$. Our broad conclusion about the role of affective polarization would be unchanged.

⁴⁸As we note in footnote 12, our framework’s assumption that these affective preferences are independent of invitation impact may be unrealistic. However, it is unlikely to threaten our conclusions. We find gaps in $A(\textit{Try Influence Email})$ even when Democrats expect invitations to have equal impact on Democratic and Republican recipients. If anything, these affective preference gaps would likely be larger when influencers expect to have more impact on co-partisans’ action.

are much more likely to try to recruit other Democrats than Republicans—even when they know that all of them believe climate change is human-caused. In a series of mechanism experiments, we then decompose the beliefs and preferences that could underlie these outreach gaps. In particular, we aim to distinguish the role of strategic beliefs—driven in part by “issue polarization,” the large partisan gaps in climate beliefs and policy preferences—from affective preferences against cooperating with counter-partisans, arising from “affective polarization.”

We find that partisan outreach gaps are driven both by strategic beliefs and today’s context of extreme affective polarization. On the one hand, Democrats correctly expect their outreach to be about twice as effective in mobilizing emails from Democrats than from Republicans, so outreach along party lines is more strategically efficient. However, affective polarization matters in two key ways. First, *anticipated* affective polarization drives Democrats’ differential influence beliefs: they estimate that hiding their politics from invitations to Republicans would close the gap in their ability to mobilize co- and counter-partisans. Second, Democrats remain much more likely to try to recruit co-partisans even when they expect to be equally successful in mobilizing emails from Democrats and Republicans; these persistent gaps are best explained by differential affective preferences to themselves reach out to co-partisans, independent of impact.

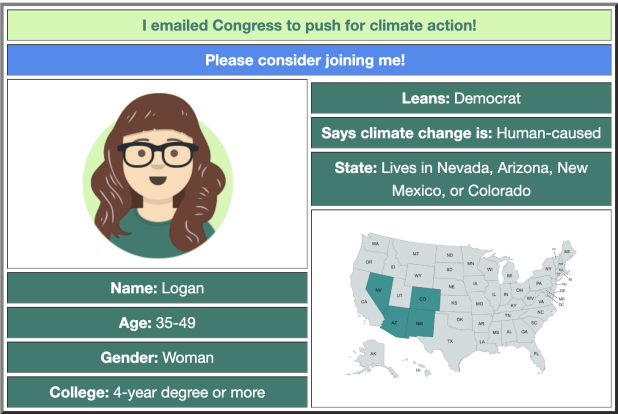
Altogether, our results suggest that bipartisan citizen coalitions are unlikely to organically form. Rather, outreach by movement members may push towards increasing partisan homogeneity. We find large partisan outreach gaps even when we directly connect Democrats with potential allies across the political aisle; these outreach gaps are likely even starker in real-world settings, when working via polarized social networks and with more scope for direct interaction, where affective polarization could play a larger role.

Mobilizing bipartisan citizen advocacy on climate or other issues may then require one of two approaches. First, it may require building parallel liberal and conservative movements at the grassroots level, with “like” mobilizing “like.” Indeed, we see these split movements in modern environmentalism today: mainstream, liberal climate organizations like the Sierra Club mobilize Democrats with platforms covering a range of liberal priorities, while other NGOs, such as the American Conservation Coalition, explicitly target conservatives. Another viable approach may be to facilitate grassroots cooperation in ways that directly target affective polarization. For example, a growing literature in political science suggests that cross-party conversations reduce both first- and second-order affective polarization, especially when they focus on non-political common ground (Santoro and Broockman, 2022; Fang et al., 2025). There may be substantial room for investment in either approach: currently, only 1% of philanthropic climate funding goes to increasing Republicans’ support for climate policy (Nisbet, 2018).

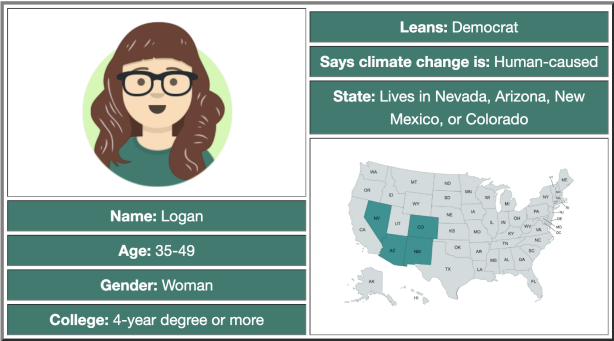
8 Main-text figures and tables

Figure 1: Profiles and invitations exchanged between network participants

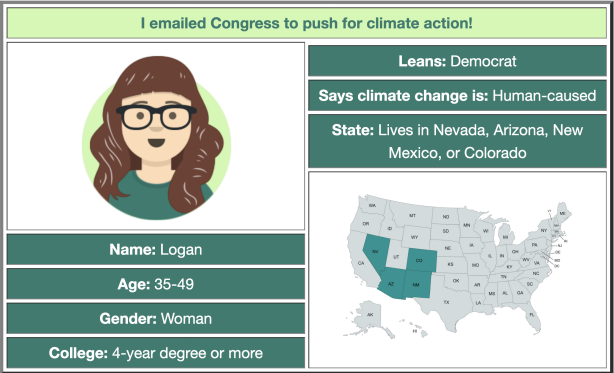
(a) Sample invitation to email Congress



(b) Sample demographic profile



(c) Sample profile saying emailed Congress



Note: Demographic profiles like those in subfigure (b) form the basis of all interactions between Wave-1 and Wave-2 participants. Across several experimental designs, Democratic influencers can send future participants either invitations to email Congress (subfigure (a)) or profiles that say that they emailed Congress, but which recipients would see after themselves deciding whether to email Congress or not (subfigure (c)). Given this timing, the profile in subfigure (c) does not include a statement inviting the recipient to join in emailing Congress.

Figure 2: The measure of climate action: Emailing Congress

Tell Congress You Care!

One way that people address climate change is by directly contacting their Congressional representatives to advocate for climate policy.

You can email Congress through this form, if you're interested!



Contact Information

Prefix (required)

First Name (required)

Last Name (required)

Email (required)

Mobile or Home Phone (required)

50 Memorial Drive, E52-300
Cambridge, MA 02142-1347

I would like to change my address

SEND

PREVIEW

Message



Sen. Elizabeth Warren
(D-MA)



Sen. Ed Markey
(D-MA)



Rep. Ayanna Pressley
(D-MA-07)

Subject: Strong climate policy in the US

Dear [[Recipient's Title and Name]]:

Please write 1 or 2 sentences describing who you are. (required)

Please add 1 or 2 sentences describing why you care about climate change. (required)

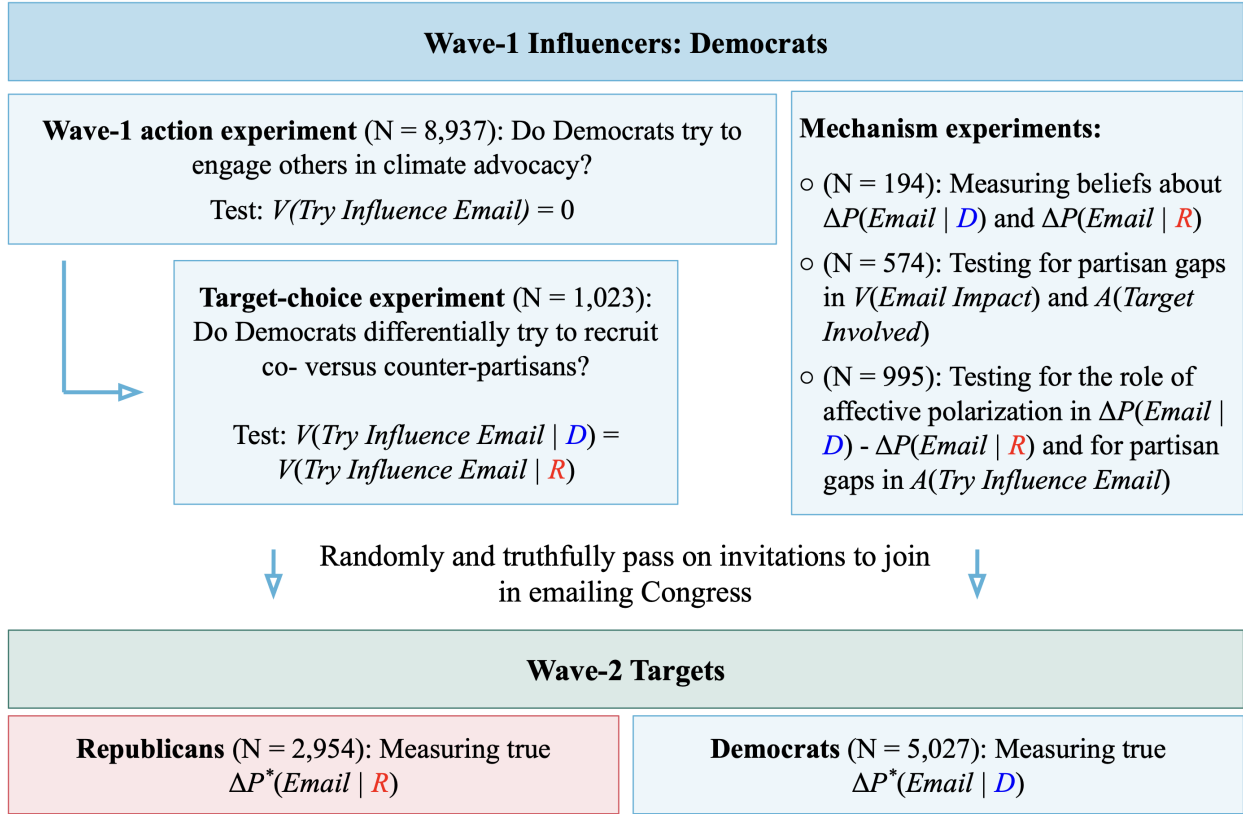
Please add anything else that you'd like to include in your message.

Sincerely,

[Your Full Name]

Note: This figure shows the form that we license and through which participants can email Congress about climate change. Note that the form is almost fully customizable, except that it includes a fixed subject line supporting climate action. We randomize this subject line across participants. The Online Supplement shows screenshots of the full process of emailing Congress.

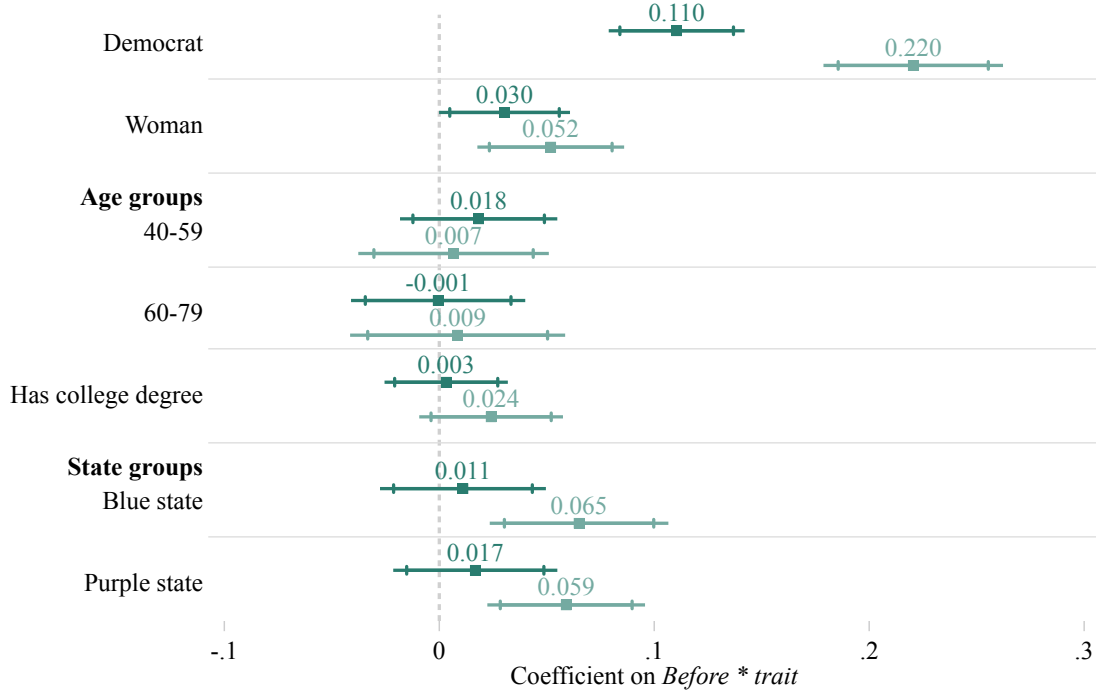
Figure 3: An overhead view of the study design



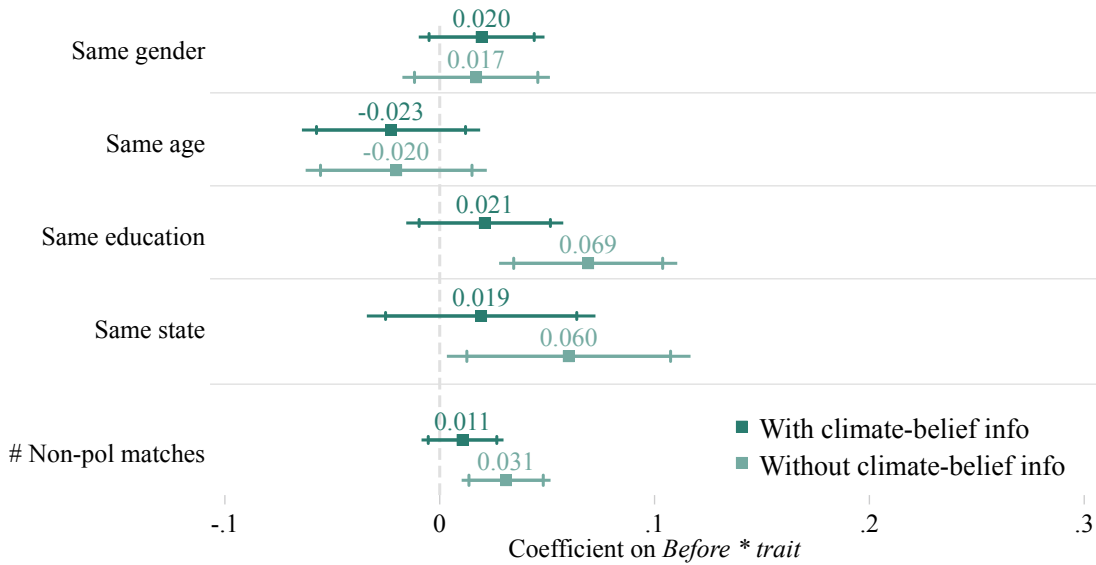
Note: This figure provides an overview of the interlocking experiments that contribute to this paper. The experiments rest on a network connecting two waves of participants: a Wave 1 of Democrats who have opportunities to email Congress and to invite Wave-2 targets to join them in doing so. Wave-2 participants are split between those who lean towards the Republican party and the Democratic party. A series of experiments among Wave-1 participants test whether they are motivated to recruit others for climate action, whether they differentially try to recruit Democrats or Republicans, and what beliefs or preferences underlie any such differential attempts. Among Wave-2 participants, we then test the true impacts of Wave-1 invitations on whether Democrats and Republicans email Congress.

Figure 4: Differential Target-choice influence attempts by other match features

Panel A. By matches' demographic traits



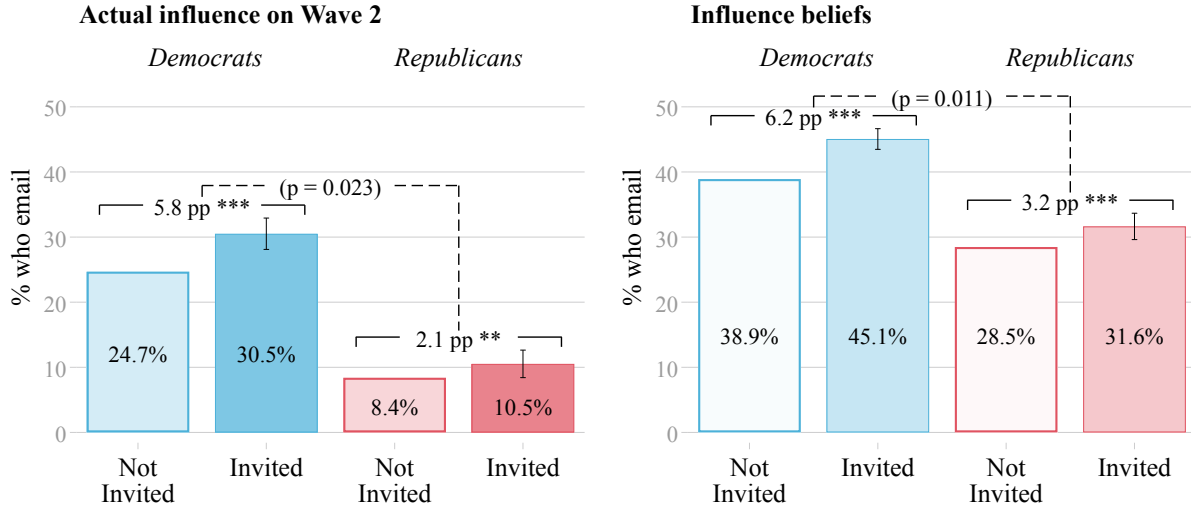
Panel B. By non-political similarity



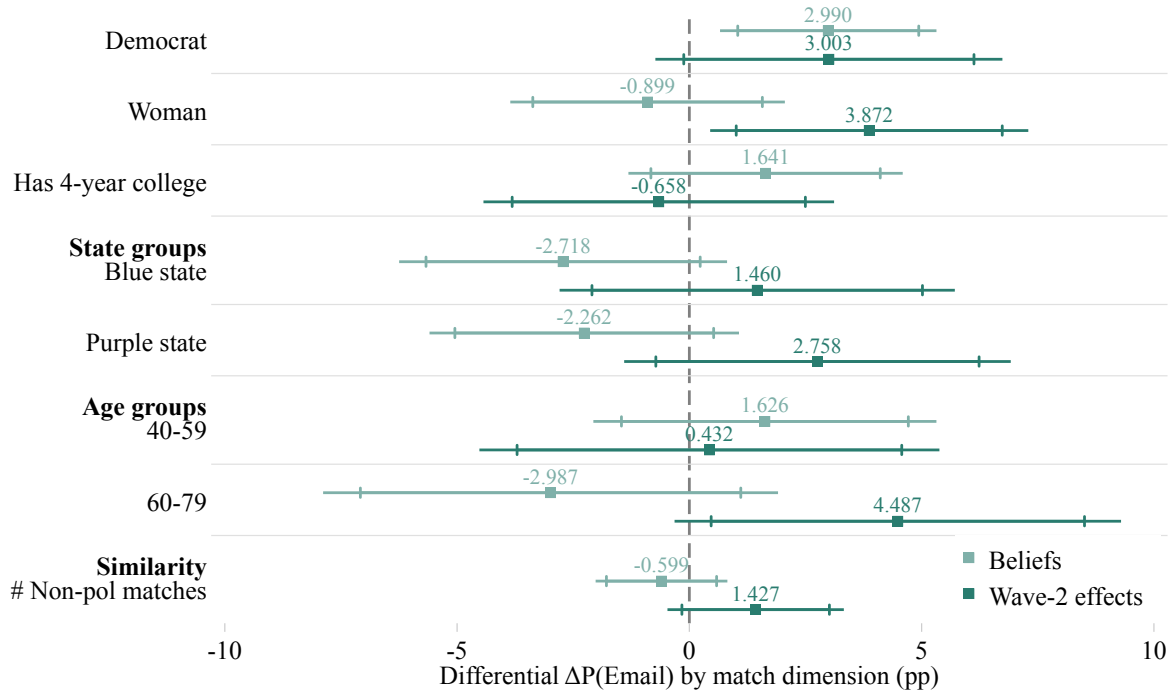
Note: Panel A tests whether TC participants differentially seek to mobilize possible matches with certain demographic traits, estimated in a variant of Equation 5 in which we jointly include indicators for each trait and their interactions with $Before_{ij}$. The omitted categories for age and state groups are ages 20-39 and red states, respectively. Note that we present matches' state of residence alongside their political affiliation, letting us separately test for targeting on match politics and the politics of legislators who would receive their email. Panel B tests whether TC participants differentially seek to mobilize possible matches with whom they share non-political traits, jointly estimating interactions with each dimension of similarity. The last row in this figure estimates a variant of Equation 5 in which we interact $Before_{ij}$ with the total number of non-political traits that a TC participant and a given possible match share, as well as an indicator that the match is a Democrat and its interaction with $Before_{ij}$. All regressions use the same sample restrictions, clustering of standard errors, and control variables as in Table 3. The capped and uncapped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively.

Figure 5: True and participant-estimated influence effects ($\Delta P(\text{Email})$)

Panel A. True and participant-estimated overall $\Delta P(\text{Email})$ by recipients' party affiliation

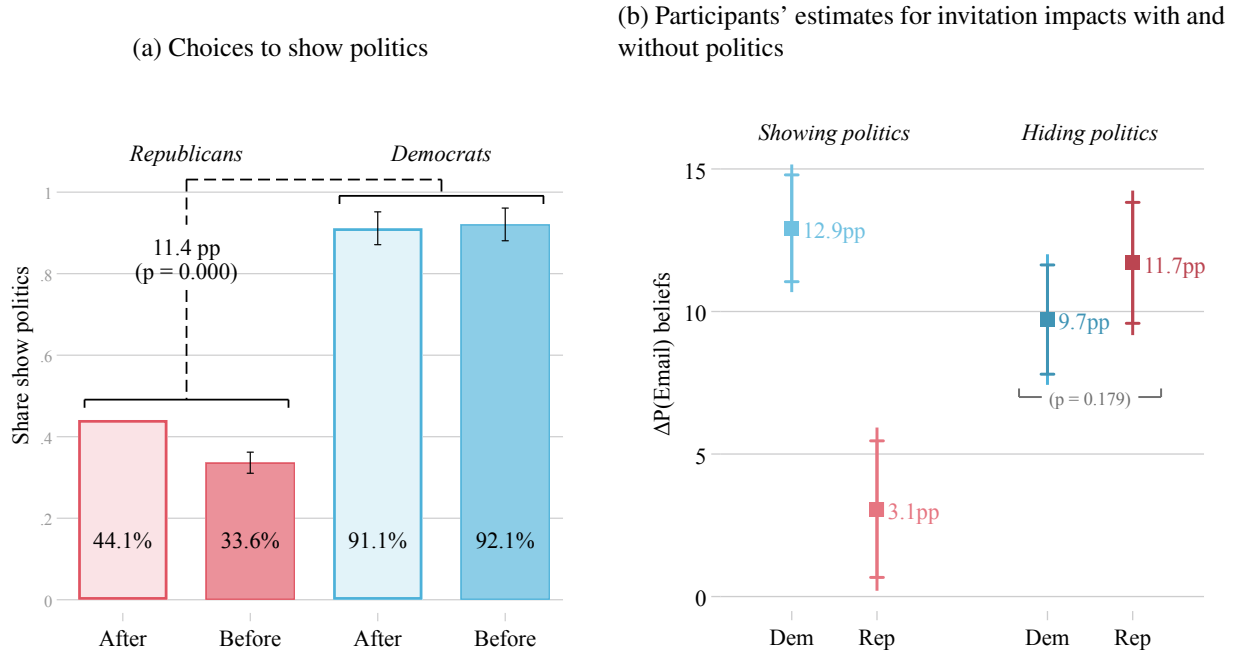


Panel B. Heterogeneity in true and participant-estimated $\Delta P(\text{Email})$ by other recipient and match traits



Note: Panel A shows the true (at left) and participant-estimated (at right) impacts of invitations on whether Wave-2 Democrats and Republicans email Congress. The figure at left plots email rates with and without invitations among Wave-2 Democrats ($N=5,027$) and Republicans ($N=2,954$; Equation 6). The figure at right plots the belief sample's ($N=194$) estimates for Wave-2 email rates with and without invitations (Equation 7). Both subfigures show 95% heteroskedasticity-robust confidence intervals for the true and participant-estimated effects of invitations in each party, and we indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively. We show p -values from tests that the true or participant-estimated effects of invitations are equal between Democratic and Republican targets. Appendix Figure D10 shows p -values comparing average beliefs with estimated Wave-2 effects. Panel B plots heterogeneity in true and participant-estimated influence effects by other recipient traits and non-political similarity. We estimate all of this heterogeneity jointly in versions of Equations 6 and 7 where we interact the invitation indicator with each recipient trait. The only exception is that we estimate the role of matching on an additional non-political trait in regressions including only the interactions of the invitation indicator with Wave-2 targets' political leanings and with the number of shared traits. Appendix Figure D3 shows the role of sender-recipient match on particular non-political traits. The capped and uncapped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively.

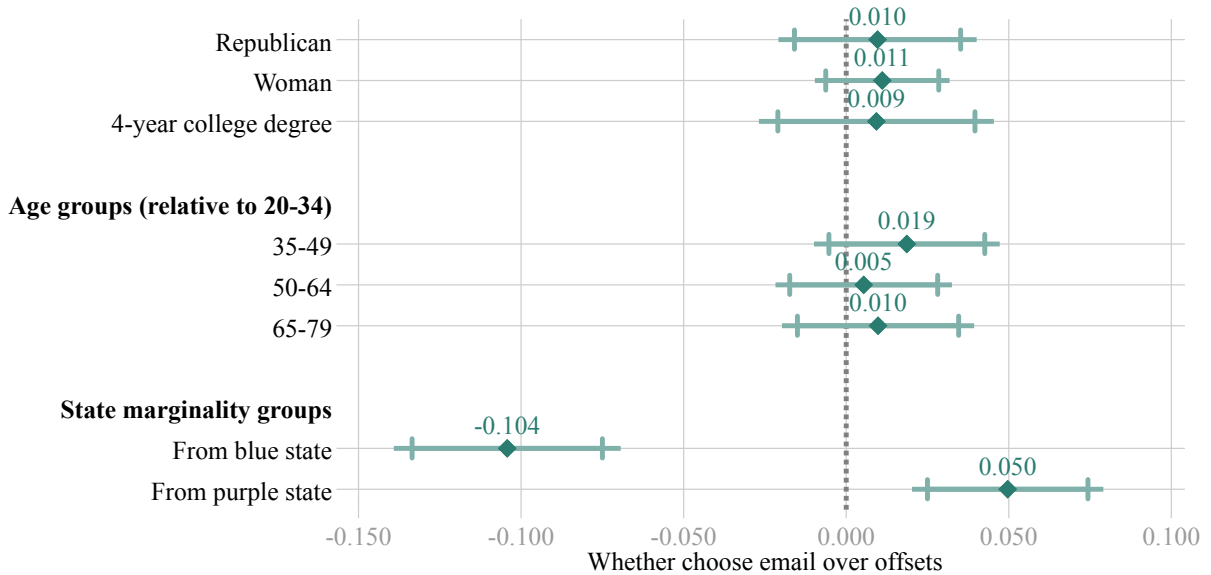
Figure 6: Hiding politics from invitations in the round-two Target-choice (TC) experiment



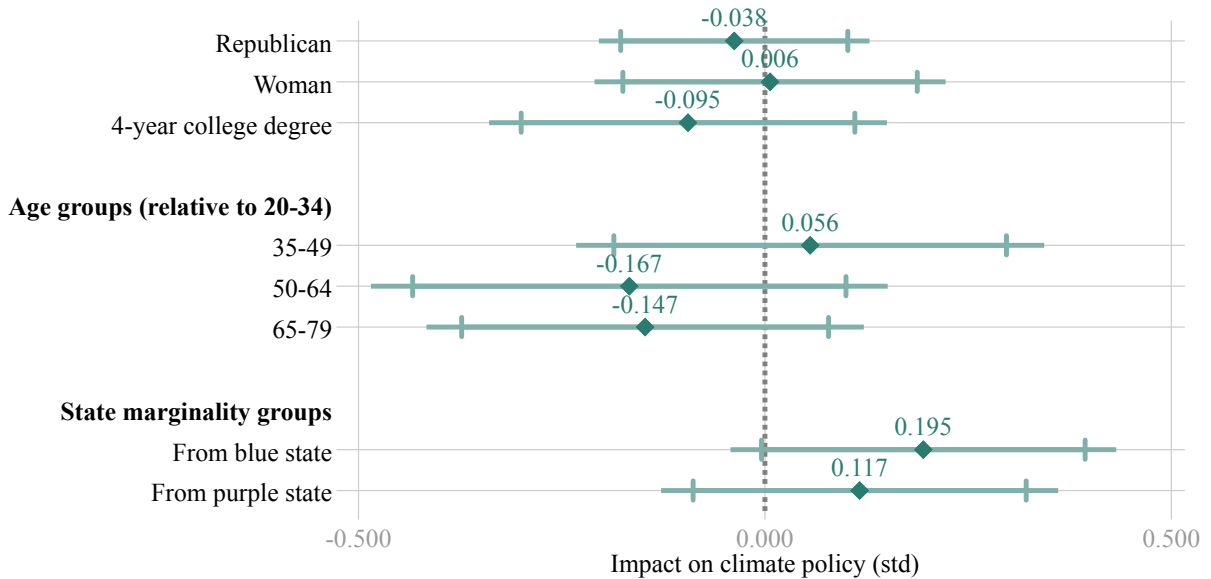
Note: Subfigure (a) summarizes round-two TC participants' choices to include their political leanings in profiles shown to possible matches; we restrict here to the 490 participants who are randomized to have the option to hide their politics and complete all 20 choices. We regress an indicator that a participant chooses to include their political leanings in a profile shown to a possible match on interacted indicators that the match is a Democrat and would see their profile before deciding whether to email Congress or not, TC participant fixed effects, and indicators for binary choice number from 1 through 20. We cluster standard errors by TC participant. Subfigure (b) plots participants' estimates for the impact of invitations that do or do not show that the sender is a Democrat on the probability that a Democratic or Republican recipient would email Congress. 503 participants overall were randomized to these belief elicitation, and we restrict here to those who had the option to choose to hide or show politics to their matches and completed all elicitation (N = 240). To estimate these beliefs, we ask participants to estimate the probability that two possible matches would email Congress if they did or did not see an invitation to do so from the TC participant; we randomize across participants whether this invitation would show that they are a Democrat. We then regress participants' guesses for the probability that the match would email Congress on a dummy that the match would see their invitation, separately by randomized match politics and whether invitations would show the senders' Democratic affiliation. The capped and uncapped bars plots 90% and 95% confidence intervals, respectively, with standard errors clustered by TC participant. The p -value shown tests that participants report equal average effects of an invitation that hides the sender's politics on Republican and Democratic recipients.

Figure 7: Results of the email-valuation experiment

Panel A. Determinants of choosing emails with certainty over carbon offsets



Panel B. The determinants of emails' impacts on the likelihood a climate bill would pass



Note: In Panel A, we regress whether participants choose an email enlisted with certainty from a given demographic group (instead of a \$10 carbon-offset donation) on email-writer demographic traits. This regression includes 8,036 choices across 574 participants. In addition to estimating the role of the demographics shown above, we also control for indicators for choice number. In both panels, 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. In Panel B, we ask participants to assume that 20 people in a given demographic group emailed Congress about a climate bill and that their representatives read their emails. We then ask participants to rate how these emails would affect whether those representatives support the climate bill, from 1 (Make much less likely to support) to 9 or 11 (Make much more likely to support). We standardize these outcomes across email-valuation participants, each of whom provides these responses for one randomly-chosen demographic group ($N = 557$). We control for the demographics shown and for choice-number fixed effects. The capped and uncapped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively.

Table 1: Summary of the Wave-1 and Wave-2 samples

	(1) Wave-1: Democrats	(2) Wave-2: Democrats	(3) Wave-2: Republicans	(4) 2023 ACS statistics
Woman	0.638	0.564	0.520	0.505
White	1.000	1.000	1.000	0.714
Hispanic	0.026	0.055	0.101	0.194
Has $\geq$ 4-year college degree	0.826	0.758	0.361	0.362
Age ranges:				
20-39	0.114	0.213	0.426	0.353
40-59	0.341	0.327	0.318	0.321
60-79	0.545	0.460	0.255	0.250
Income bins (USD):				
Less than 50,000	0.167	0.232	0.397	0.323
50,000-99,999	0.321	0.323	0.377	0.288
100,000-149,999	0.234	0.221	0.136	0.174
150,000-199,999	0.130	0.110	0.051	0.091
200,000 or more	0.148	0.114	0.039	0.124
Residence by state marginality:				
Red state	0.236	0.270	0.413	0.354
Blue state	0.441	0.418	0.257	0.343
Purple state	0.322	0.312	0.330	0.295
Climate beliefs:				
Climate worry (1-7)	6.421	6.305	4.953	
Desire for climate action (1-7)	6.734	6.632	5.217	
Perceived local impacts (1-7)	5.499	5.484	4.835	
Political engagement and beliefs:				
Member of resp. party	1.000	0.316	0.741	
Prev. contacted reps	0.731	0.268	0.219	
Prev. donated	0.819	0.688	0.242	
Prev. canvassed	0.081	0.064	0.040	
Prev. signed petition	0.828	0.749	0.390	
Prev. phonebanked	0.111	0.080	0.045	
Prefer friend of own party (1-7)	6.038	5.664	4.894	
Sample size	8937	5200	2960	

Note: This table summarizes the Wave-1 and Wave-2 experimental samples. We describe these samples in more detail in Sections 4.3 and 6.1.2. Column 1 presents mean values of each baseline trait among Wave-1 influencers, all of whom are members of the Democratic party. Columns 2 and 3 present mean values among Wave-2 targets who lean towards the Democratic and Republican parties, respectively. Column 4 presents statistics from the 1-year 2023 American Community Survey Tables S0101, S1501, S1901, and DP05. In these national figures, we show the share of Americans with at least a 4-year college degree among those over age 25, the share in each age range out of all adults over 18, and the share white including multi-racial individuals. Appendix B.2 describes each of the baseline traits shown in more detail. Appendix C.2 describes our classification of states as red, blue, or purple. Variables labeled as “(1-7)” are elicited on Likert scales from 1 through 7. Note that participants in both Waves 1 and 2 are restricted to believe that climate change is mostly human-caused, live within the contiguous US, be between 20 and 79, identify as a man or woman, and identify as white. Footnote 6 and Appendix A.1 detail why and how we screen participants by politics, climate beliefs, and these demographics.

Table 2: Email choices in the Wave-1 experiment

	(1)	(2)	(3)	(4)	(5)	(6)
	Started email process			Sent an email		
	<i>All Wave 2</i>	<i>Democrats</i>	<i>Republicans</i>	<i>All Wave 2</i>	<i>Democrats</i>	<i>Republicans</i>
Tell	0.121*** (0.012)	0.133*** (0.017)	0.106*** (0.018)	0.134*** (0.012)	0.143*** (0.017)	0.122*** (0.017)
Invite	0.033** (0.013)	0.049*** (0.018)	0.017 (0.019)	0.027** (0.013)	0.035* (0.019)	0.020 (0.019)
Control mean	0.461	0.452	0.470	0.310	0.304	0.315
N	8937	4494	4443	8937	4494	4443
<i>p-values:</i>		Tell (2) = (3): Invite (2) = (3):	0.281 0.220		Tell (5) = (6): Invite (5) = (6):	0.389 0.566

Note: This table reports impacts of the Wave-1 treatments on whether Wave-1 participants initially opt into the email process (columns 1-3) and match to an email record (columns 4-6). Columns 1 and 4 pool across all Wave-1 participants, while columns 2 and 5 versus 3 and 6 restrict to those paired with Wave-2 Democrats or Republicans, respectively. We define the sample as those who were randomized to a Wave-1 treatment, assuming that participants who left the survey before deciding whether to email or not, but after seeing the email preview, would not have done so. All regressions control for participants' recruitment timing, demographics (gender, age, state of residence, income category, educational attainment, and whether they identify as Hispanic), baseline climate beliefs (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). Appendix B.2 details these controls. 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 present *p*-values for tests of equality between the treatment effects for Wave-1 participants matched with Wave-2 Democrats and Republicans.

Table 3: Invitation choices in the Target-choice experiment

	(1)	(2)	(3)	(4)
	Showed extended profile			
	<i>No climate beliefs</i>	<i>Sees climate beliefs</i>		
Before	0.416*** (0.020)	0.296*** (0.021)	0.481*** (0.019)	0.424*** (0.020)
Democrat		0.135*** (0.016)		0.037*** (0.014)
Before * Democrat		0.239*** (0.019)		0.115*** (0.015)
Mean: After, Dem + Rep	0.233		0.225	
Mean: After, Rep		0.165		0.206
# Participants	475	475	548	548
# Choices	9500	9500	10960	10960

Note: Across columns, the outcome is whether participants choose to pass on an extended profile saying that they emailed Congress, rather than a basic demographic profile and carbon-offset donation. Columns 1 and 2 analyze TC participants who are assigned to see profiles of possible matches without information on their climate beliefs, while columns 3 and 4 analyze participants who see that all matches believe climate change is human-caused. We restrict to participants who make all 20 binary choices. The footer mean in columns 1 and 3 shows the share of choices in which TC participants choose to show an extended profile when matches would see their profile after deciding whether to email Congress, while the footer mean in columns 2 and 4 shows the share of cases in which TC participants choose extended profiles when matches are Republicans who would see the profile after their email choice. Regressions include fixed effects for TC participant and choice number. We show standard errors clustered by participant in parentheses and indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

Table 4: Invitation choices in the Round-2 Target-choice experiment

	(1)	(2)	(3)	(4)	(5)	(6)
	Showed extended profile					
	<i>Must show politics</i>		<i>Can hide politics</i>			
			Full sample	Hiders (Strict)	Hiders (Approx)	
Before	0.445*** (0.022)	0.397*** (0.022)	0.409*** (0.020)	0.366*** (0.022)	0.399*** (0.043)	0.409*** (0.029)
Democrat		0.044*** (0.014)		0.064*** (0.015)	0.129*** (0.035)	0.098*** (0.022)
Before * Democrat		0.097*** (0.015)		0.085*** (0.015)	0.080*** (0.030)	0.098*** (0.020)
Mean: After, Dem + Rep	0.266		0.293			
Mean: After, Rep		0.245		0.261	0.232	0.237
# Participants	505	505	490	490	124	282
# Choices	10100	10100	9800	9800	2480	5640
<i>p-values:</i>		Democrat (2) = (4):		0.310		
		Before * Democrat (2) = (4):		0.569		

Note: This table shows participants' invitation choices in the round-two TC experiment. Columns 1 through 4 mimic those in Table 3, except that here columns 1 and 2 show estimates for TC participants who cannot choose to hide their politics from profiles passed on to recipients, while columns 3 and 4 show estimates for participants with this option. Columns 5 then restricts to participants with the option to hide their politics who choose to do so for all 10 Republican matches and to show their politics to all 10 Democratic matches. Column 6 restricts to participants who almost always follow this strategy, as classified in Appendix Figure D16. Both samples have no partisan gap in their effective $\Delta P(\text{Email})$ influence beliefs. Across all columns, we restrict to participants who make binary choices for all 20 possible matches. All regressions include fixed effects for TC participant and choice number. We present standard errors clustered by participant in parentheses and indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

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Online Appendix: Reaching across the aisle: Polarization and grassroots climate mobilization

Lucy Page and Hannah Ruebeck

May 2025

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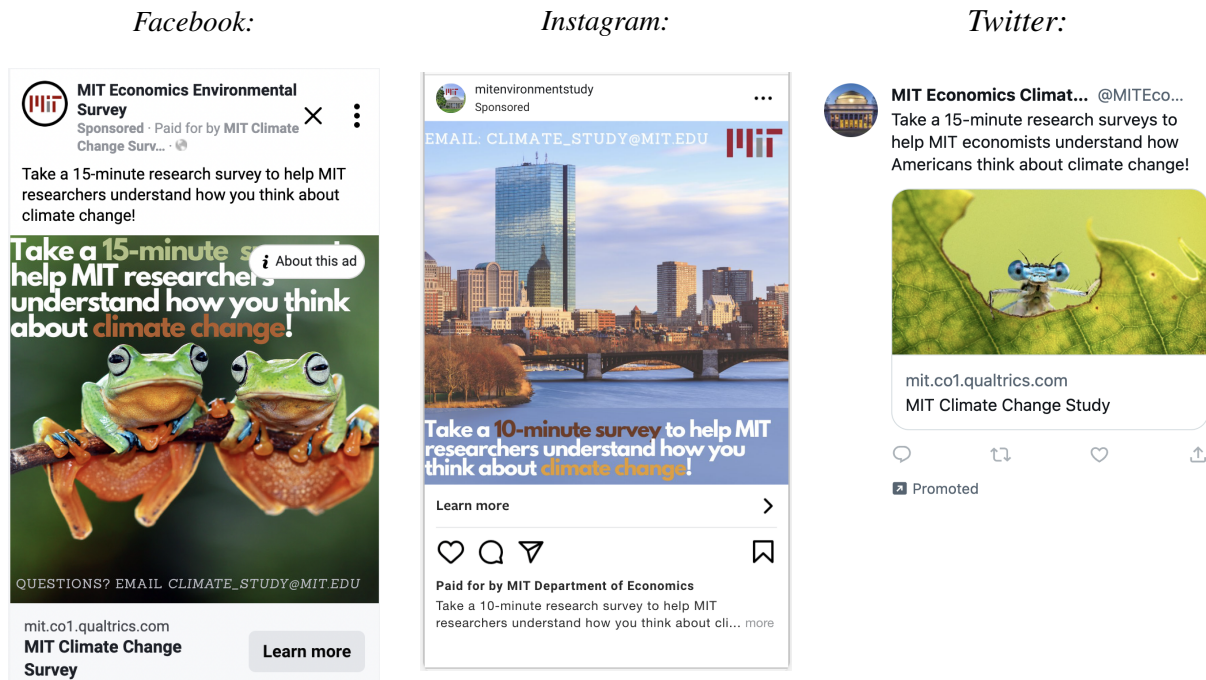
Other pre-registered analyses

Deviations from PAP

Screenshots of full surveys

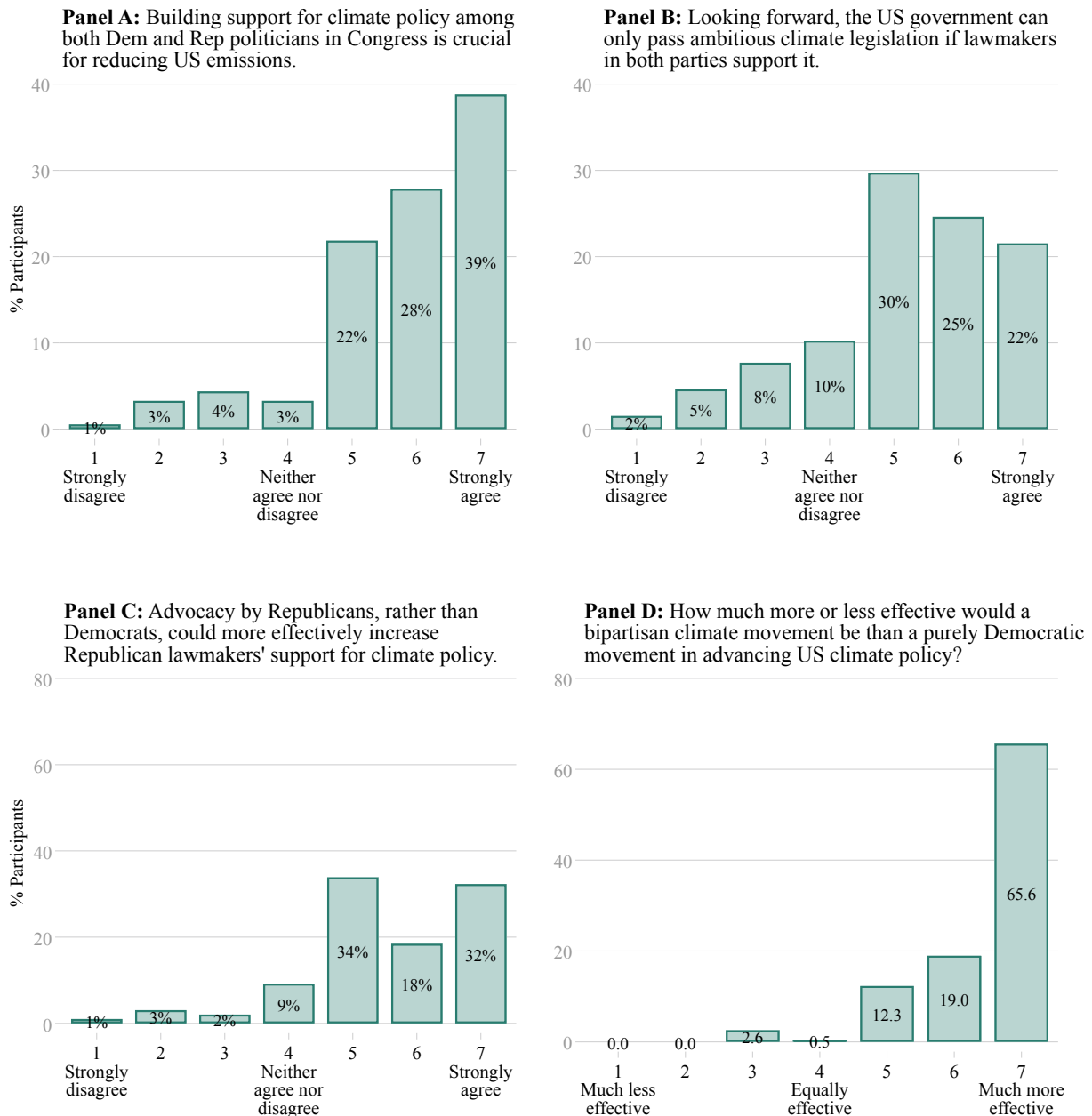
A Appendix to the experimental context and conceptual framework

Figure A1: Sample social-media advertisements



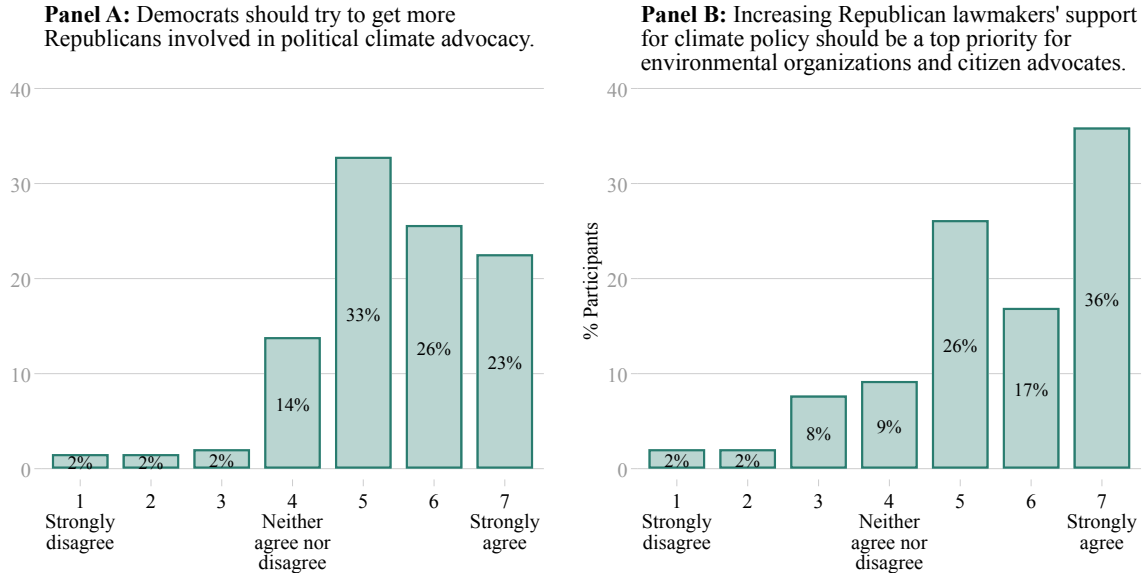
Note: This figure gives screenshots of sample survey advertisements on Facebook, Instagram, and Twitter. We used similar ads for all study recruitment, except for those Wave-2 participants who were recruited via Qualtrics. The stated survey length and other ad text varied slightly depending on the survey for which we were recruiting participants.

Figure A2: Democrats think bipartisan climate action would be more effective



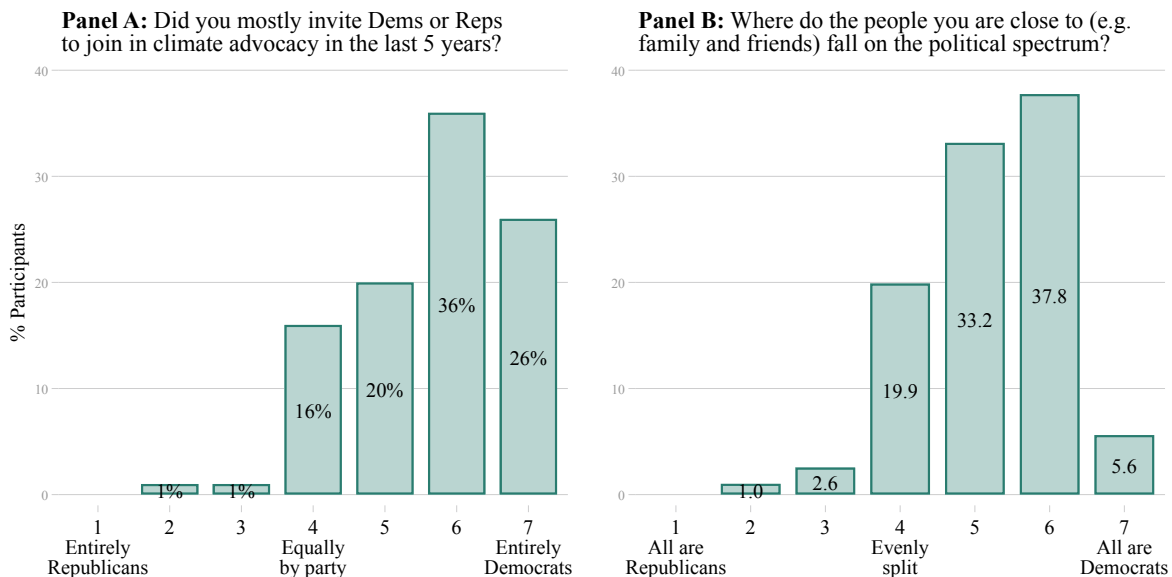
Note: This figure plots responses in a motivating sample of Democrats, who were recruited subject to the same eligibility restrictions as the Wave-1 experimental sample (Section 2.1). Panels A, B, and C plot responses to how much participants agree with the statements in the subfigure titles, while Panel D plots responses to the question in the subfigure title. The x-axes of each figure give the scales on which participants reported their responses. $N = 183$ in Panel A and $N = 195$ in Panels B, C, and D. (The sample is lower in Panel A because this question was mistakenly not marked as required.) Throughout these statements and those shown in Figures A3 and A4, we referred in the question text to citizens as “liberals” or “conservatives” rather than Democrats or Republicans, defining liberals and conservatives to respondents as those who belong to or lean towards the Democratic and Republican parties, respectively. We have changed the statements here for consistency with our other in-text descriptions. We define a bipartisan climate movement to participants as one that includes citizens who lean towards or belong to both the Democratic or Republican Parties.

Figure A3: Democrats say citizens and organizations should focus on engaging Republicans



Note: This figure plots the motivating sample's agreement with the statements in the subfigure titles. The x-axes of each figure give the same scales on which participants reported their responses. Participants saw a version of the question statement in Panel A that referred to "liberals" and "conservatives" rather than Democrats or Republicans, alongside definitions of liberals and conservatives as those who belong to or lean towards the Democratic and Republican parties, respectively. N = 195 in both figures.

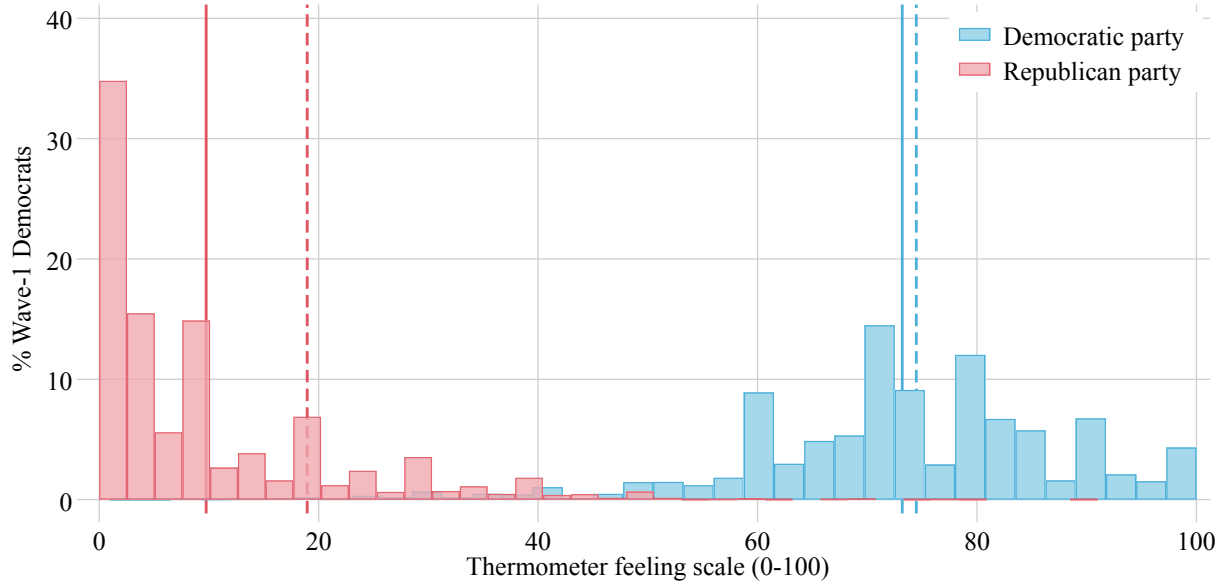
Figure A4: Few Democrats say they've previously invited Republicans



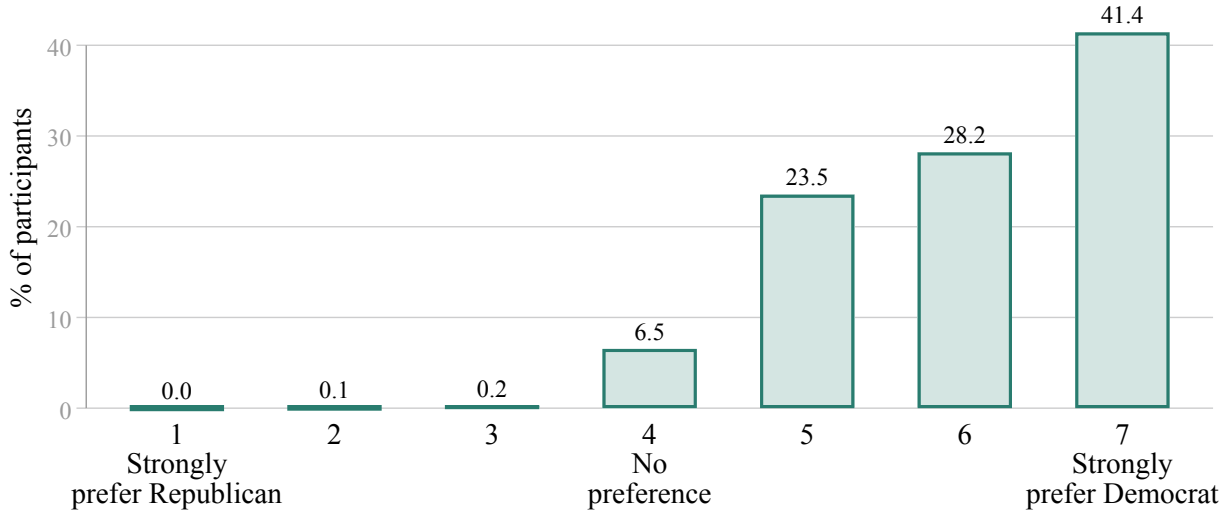
Note: This figure plots the motivating sample's responses to the questions in the subfigure titles. Panel A restricts to Democrats who said that they have invited other individuals to join in political climate advocacy in the last 5 years (N = 100). Panel B is in the full motivating sample (N = 196). The question statements in Panels A and B referred to "liberals" and "conservatives" rather than Democrats or Republicans, alongside definitions of liberals and conservatives as those who belong to or lean towards the Democratic and Republican parties, respectively.

Figure A5: Wave-1 Democrats' baseline affective polarization

Panel A. Affective polarization on the ANES “feelings thermometer”



Panel B. Democrats strongly prefer friends of their own party



Note: Panel A measures affective polarization using the “feelings thermometer” question from the American National Election Study (ANES), in which participants report how warmly they feel toward the Democratic party and the Republican party on scales from 0 degrees (cold) to 100 degrees (warm). We elicit these thermometer values from Democrats in the Wave-1 experimental sample who are assigned to either the Tell-after or Invitation groups (Section 4). We elicit these values after participants choose whether to email Congress or not, yielding $N = 4,321$ (81% of those randomized to these treatment arms). The solid lines show average warmth towards the Democratic and Republican parties in our sample, while the dashed lines show average warmth values among Democrats in the 2020 ANES sample (ANES, 2021). The ANES identifies Democrats as those who say that generally speaking, they think of themselves as Democrats, while our sample restricts to those who report being members of the Democratic party. Panel B measures affective polarization using participants’ responses to how much they would prefer being friends with a Democrat versus a Republican. We randomize which party is given at 1 versus at 7 on the 7-point scale. We elicit these baseline beliefs from the full Wave-1 Democratic sample, yielding $N = 8,937$.

Table A1: Comparing the motivating and mechanism-experiment samples to the Wave 1 and Target-choice samples

	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)		(9)		(10)		(11)		(12)		(13)	
	Wave-1	TC	Mean	Δ Wave1	Mean	Δ Wave1	Mean	Δ Wave1	Mean	Δ Wave1	Mean	Δ Wave1	Mean	Δ TC	Mean	Δ Wave1	Mean	Δ Wave1	Mean	Δ TC	Mean	Δ Wave1	Mean	Δ Wave1	Mean	Δ TC
Woman	0.632	0.761	0.853	0.221***	0.587	-0.045*	-0.174***	0.677	0.045***	-0.083***	0.841	0.209***	0.841	0.209***	0.841	0.209***	0.841	0.209***	0.841	0.209***	0.841	0.209***	0.841	0.209***	0.841	0.209***
Hispanic	0.027	0.039	0.015	-0.011	0.028	0.001	-0.011	0.025	-0.001	-0.014*	0.024	-0.002	0.024	-0.014*	0.024	-0.002	0.024	-0.002	0.024	-0.014*	0.024	-0.002	0.024	-0.002	0.024	-0.014*
Has $\geq$ 4-year college degree	0.828	0.862	0.817	-0.010	0.854	0.026	-0.008	0.845	0.018	-0.017	0.871	0.043***	0.871	-0.017	0.871	0.043***	0.871	0.043***	0.871	-0.017	0.871	0.043***	0.871	0.043***	0.871	-0.017
Age ranges:																										
20-39	0.119	0.146	0.096	-0.023	0.264	0.145***	0.119***	0.055	-0.064***	-0.090***	0.122	0.002	0.122	-0.090***	0.122	0.002	0.122	0.002	0.122	-0.090***	0.122	0.002	0.122	0.002	0.122	-0.090***
40-59	0.337	0.354	0.396	0.059	0.385	0.049*	0.032	0.264	-0.072***	-0.090***	0.460	0.123***	0.460	-0.090***	0.460	0.123***	0.460	0.123***	0.460	-0.090***	0.460	0.123***	0.460	0.123***	0.460	-0.090***
60-79	0.544	0.500	0.508	-0.036	0.350	-0.194***	-0.150***	0.680	0.136***	0.180***	0.418	-0.126***	0.418	0.180***	0.418	-0.126***	0.418	-0.126***	0.418	0.180***	0.418	-0.126***	0.418	-0.126***	0.418	-0.082***
Income bins (USD):																										
Less than 50,000	0.170	0.166	0.193	0.023	0.113	-0.057***	-0.053***	0.172	0.002	0.006	0.167	-0.003	0.167	0.006	0.167	-0.003	0.167	-0.003	0.167	0.006	0.167	-0.003	0.167	-0.003	0.167	0.006
50,000-99,999	0.325	0.338	0.371	0.045	0.229	-0.096***	-0.109***	0.350	0.024	0.012	0.310	-0.015	0.310	0.012	0.310	-0.015	0.310	-0.015	0.310	0.012	0.310	-0.015	0.310	-0.015	0.310	-0.028
100,000-149,999	0.231	0.238	0.147	-0.083***	0.300	0.069***	0.062**	0.234	0.004	-0.003	0.216	-0.015	0.216	-0.003	0.216	-0.015	0.216	-0.015	0.216	-0.003	0.216	-0.015	0.216	-0.015	0.216	-0.022
150,000-199,999	0.126	0.118	0.117	-0.009	0.181	0.055***	0.063***	0.117	-0.010	-0.002	0.132	0.006	0.132	-0.002	0.132	0.006	0.132	0.006	0.132	-0.002	0.132	0.006	0.132	0.006	0.132	0.014
200,000 or more	0.147	0.140	0.173	0.025	0.176	0.029	0.037*	0.128	-0.020*	-0.012	0.174	0.027	0.174	-0.012	0.174	0.027	0.174	0.027	0.174	-0.012	0.174	0.027	0.174	0.027	0.174	0.034*
State marginality:																										
Red state	0.231	0.220	0.213	-0.017	0.227	-0.004	0.007	0.243	0.013	0.023	0.240	0.010	0.240	0.023	0.240	0.010	0.240	0.010	0.240	0.023	0.240	0.010	0.240	0.010	0.240	0.020
Blue state	0.438	0.457	0.386	-0.052	0.481	0.043*	0.025	0.454	0.016	-0.002	0.439	0.001	0.439	-0.002	0.439	0.001	0.439	0.001	0.439	-0.002	0.439	0.001	0.439	0.001	0.439	-0.017
Purple state	0.331	0.324	0.401	0.070*	0.292	-0.039	-0.031	0.303	-0.029*	-0.021	0.321	-0.011	0.321	-0.021	0.321	-0.011	0.321	-0.011	0.321	-0.021	0.321	-0.011	0.321	-0.011	0.321	-0.003
Climate beliefs:																										
Climate worry (1-7)	6.430	6.547	6.513	0.083	6.395	-0.034	-0.152***	6.605	0.175***	0.058*	6.528	0.098***	6.528	0.058*	6.528	0.098***	6.528	0.098***	6.528	0.058*	6.528	0.098***	6.528	0.098***	6.528	-0.020
Desire for climate action (1-7)	6.735	6.823	6.782	0.047	6.725	-0.009	-0.098***	6.809	0.075***	-0.014	6.774	0.039*	6.774	-0.014	6.774	0.039*	6.774	0.039*	6.774	-0.014	6.774	0.039*	6.774	0.039*	6.774	-0.050*
Perceived local impacts (1-7)	5.496	5.576	5.426	-0.070	5.479	-0.018	-0.097*	5.610	0.114***	0.034	5.495	-0.001	5.495	0.034	5.495	-0.001	5.495	-0.001	5.495	0.034	5.495	-0.001	5.495	-0.001	5.495	-0.081
Political engage. and beliefs:																										
Prev. contacted reps	0.733	0.803	0.777	0.044	0.703	-0.030	-0.100***	0.806	0.073***	0.003	0.758	0.025	0.758	0.003	0.758	0.025	0.758	0.025	0.758	0.003	0.758	0.025	0.758	0.025	0.758	-0.045**
Prev. donated	0.816	0.863	0.822	0.007	0.798	-0.017	-0.065***	0.841	0.026**	-0.022	0.829	0.014	0.829	-0.022	0.829	0.014	0.829	0.014	0.829	-0.022	0.829	0.014	0.829	0.014	0.829	-0.034*
Prev. canvassed	0.081	0.090	0.132	0.051**	0.091	0.009	0.001	0.107	0.025**	0.017	0.105	0.023	0.105	0.017	0.105	0.023	0.105	0.023	0.105	0.017	0.105	0.023	0.105	0.023	0.105	0.015
Prev. signed petition	0.835	0.880	0.878	0.044*	0.816	-0.019	-0.064***	0.855	0.021	-0.024	0.838	0.003	0.838	-0.024	0.838	0.003	0.838	0.003	0.838	-0.024	0.838	0.003	0.838	0.003	0.838	-0.042**
Prev. phonebanked	0.100	0.134	0.127	0.027	0.091	-0.010	-0.043**	0.143	0.042***	0.009	0.139	0.039***	0.139	0.009	0.139	0.039***	0.139	0.039***	0.139	0.009	0.139	0.039***	0.139	0.039***	0.139	0.005
Degree prefer Dem friends (1-7)	6.040	6.072	6.020	-0.020	6.033	-0.008	-0.040	6.005	-0.035	-0.067	6.070	0.029	6.070	-0.067	6.070	0.029	6.070	0.029	6.070	-0.067	6.070	0.029	6.070	0.029	6.070	-0.003
Sample size	3616	1023	197		397			995			574															

Note: This figure compares the baseline traits of the main Wave-1 experimental sample (Section 4) and Target-choice (TC) sample (Section 5) with the motivating sample (Section 2.2), the influence-belief sample (Section 6.1.5), the round-two Target-choice sample (Section 6.2), and the email-valuation sample (Section 6.3.2). Appendix B.2 describes each of these baseline traits in more detail. Variables labeled as “(1-7)” are elicited on Likert scales from 1 through 7. Columns 1, 2, 3, 5, 8, and 11 show means in each sample. Columns 4, 6, 9, and 12 present differences in means between each mechanism sample and the Wave-1 experimental sample (column 1), while columns 7, 10, and 13 compare each mechanism sample with the Target-choice sample (column 2). Each column indicates statistical significance of these differences in means at the 10%, 5%, and 1% levels by *, **, and ***, respectively, all calculated with heteroskedasticity-robust standard errors.

A.1 Screening participant eligibility

After screening out any participant who does not live in the US or is below age 18, we elicit baseline screening questions at the start of the Wave-1 and Wave-2 surveys. The full survey scripts are available in the Online Supplement.

Demographic screeners:

Age: Participants select their age from the following categories: {18-19, 20-24, 25-29, ..., 75-79, 80-84, 85+}. We restrict the sample to those between 20 and 79.

State: Participants select their current state of residence from a dropdown list of the 50 states and Washington D.C. We restrict to those in the contiguous United States to facilitate grouping participants in clusters of nearby and politically similar states.

Gender: Participants select their gender from {Man, Woman, Non-binary, Other (with open-response field)}. We restrict the sample to those who identify as men or women.

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

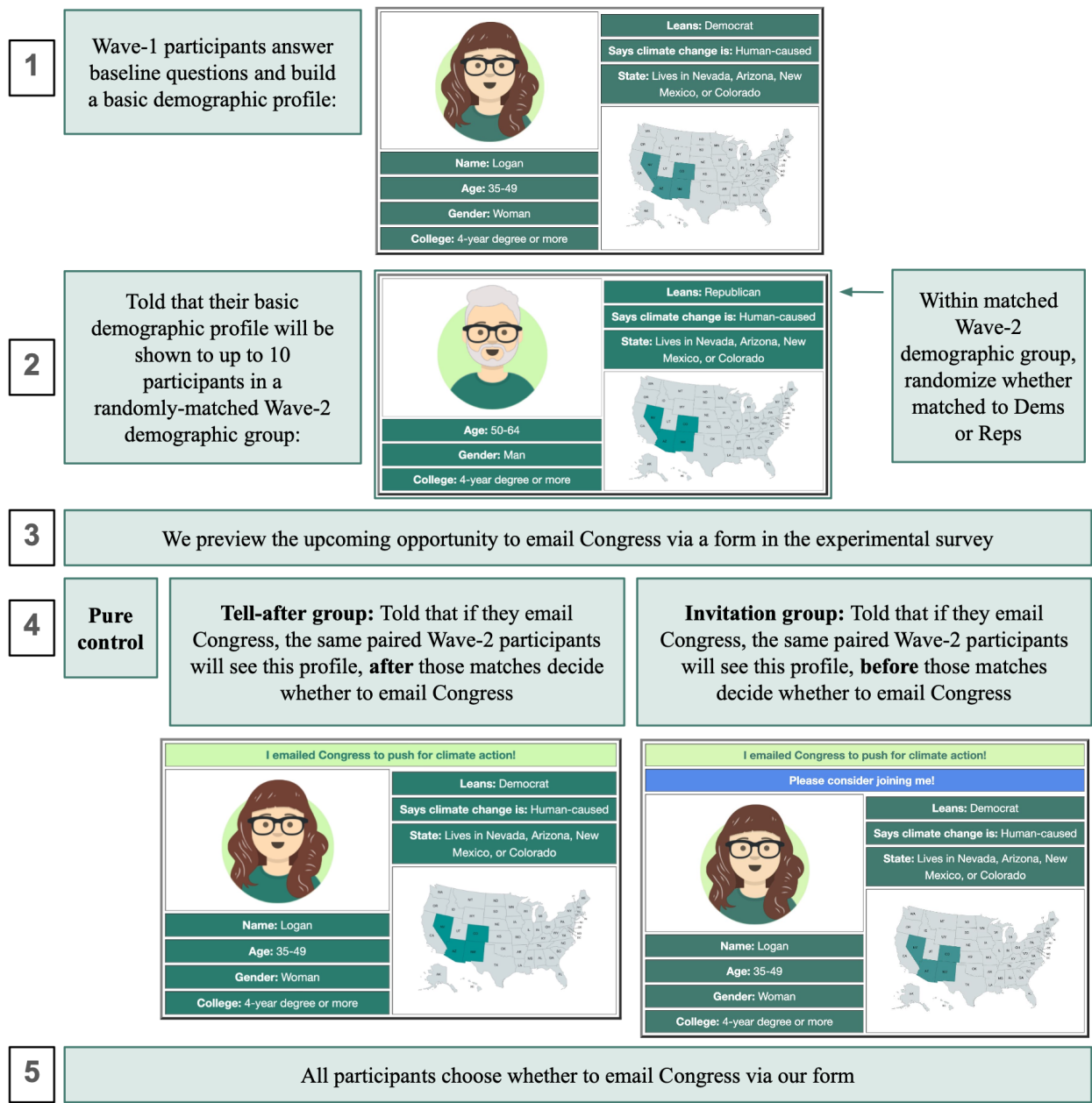
Climate-belief screener: We ask participants to choose whether they think: (1) Climate change is caused mostly by human activities; (2) Climate change is caused mostly by natural changes in the environment; (3) Neither, since climate change isn't happening; or (4) Other (with open-response field). We restrict to those who say that climate change is caused by human activities.

Political-affiliation screener: We ask participants whether, generally speaking, they would say they lean towards the Democratic Party, the Republican Party, or neither. If they choose 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? We then restrict Wave-1 participants to members of the Democratic Party. We restrict Wave-2 participants to those who lean towards either the Democratic or Republican Parties.

B Appendix to the Wave-1 experiment

B.1 Wave-1 additional exhibits

Figure B1: Flowchart of Wave-1 experimental structure

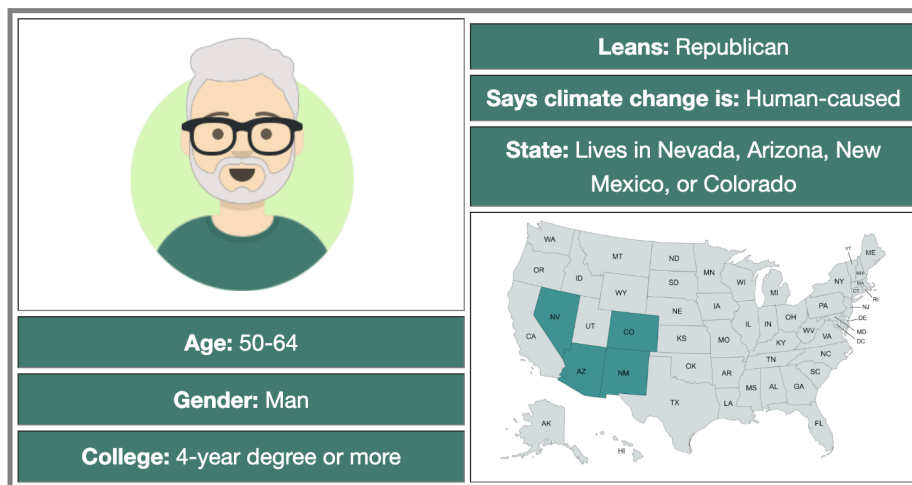


Note: This figure lays out the structure of the Wave-1 action experiment, which we describe in detail in Section 4.1.

Figure B2: Telling Wave-1 participants that Wave-2 participants will see their demographics

Earlier in the survey, we mentioned that we might show your profile to future study participants to give them a sense of who else is participating in the study.

In particular, we'll show your profile to **up to 10 future study participants** who **match all of the basic characteristics in the profile below** (though their hair or accessories might be different):



Note: This figure shows how we tell Wave-1 participants that their basic demographic profile will be shown to up to 10 Wave-2 participants in a certain demographic group. Wave-1 participants see this slide after seeing their own demographic profile on the previous page. We generate 208 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 (described in Appendix C.2). Wave-1 Democrats in a particular demographic cell are randomly paired to be shown to Wave-2 participants in a certain demographic cell; within that Wave-2 group, they are randomly paired with participants who lean towards the Democratic or Republican parties. We randomly choose an avatar for participants' paired Wave-2 group from the list of avatars that participants in that age-gender cell chose during piloting. The full Wave-1 survey and all surveys in this project are available in the Online Supplement.

Figure B3: Explanation of the Tell-after and Invitation treatments

Like we said earlier, we'll be running a second survey in a few weeks, and up to 10 participants in that survey will be randomly paired to see your demographic profile.

If you decide to contact Congress, we'll also show those participants an extra profile **saying that you did so**.

Those future participants **won't be able to identify who you are**. Even so, we want to make sure you're informed about how your basic information will be used in our upcoming survey, in case you have privacy concerns around contacting Congress.

We'll lay out all the details in the next few slides.

Please **pay close attention**, so that you can make an informed choice about whether to contact Congress or not.

After the description, we'll ask you several **comprehension questions**.

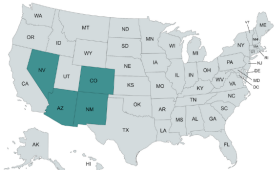
We will randomly choose **20 participants** to receive an extra **gift card worth \$5** for each comprehension question they answer **correctly**, so please answer carefully!

Important note: You won't be able to go back to earlier explanation pages, so you should try to pay attention the first time through. (Clicking back in your browser takes you back to the beginning of the survey.)

(a) Tell-after group

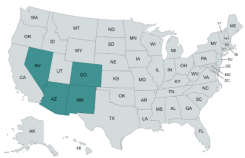
Remember that up to 10 participants in the demographic group below will see your own demographic profile when they take our survey.

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.

	Leans: Republican
	Says climate change is: Human-caused
	State: Lives in Nevada, Arizona, New Mexico, or Colorado
	
Age: 50-64	
Gender: Man	
College: 4-year degree or more	

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:

I emailed Congress to push for climate action!	
	Leans: Democrat
	Says climate change is: Human-caused
	State: Lives in Nevada, Arizona, New Mexico, or Colorado
	
Name: Logan	
Age: 35-49	
Gender: Woman	
College: 4-year degree or more	

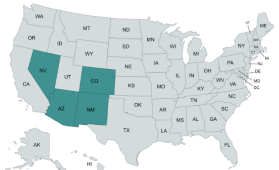
That means that they **won't know** you contacted Congress **when they decide** whether to do so or not.

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.

(b) Invitation group

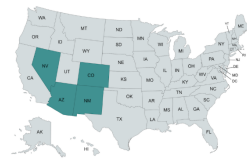
Remember that up to 10 participants in the demographic group below will see your own demographic profile when they take our survey.

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.

	Leans: Republican
	Says climate change is: Human-caused
	State: Lives in Nevada, Arizona, New Mexico, or Colorado
	
Age: 50-64	
Gender: Man	
College: 4-year degree or more	

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**:

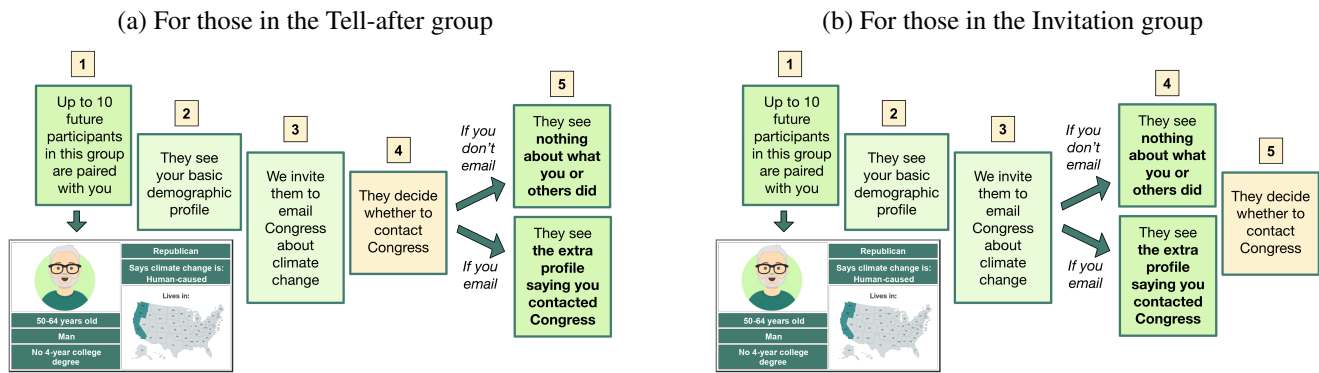
I emailed Congress to push for climate action!	
Please consider joining me!	
	Leans: Democrat
	Says climate change is: Human-caused
	State: Lives in Nevada, Arizona, New Mexico, or Colorado
	
Name: Logan	
Age: 35-49	
Gender: Woman	
College: 4-year degree or more	

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.

Note: This figure reproduces the treatment survey pages that participants see if they are assigned to the Tell-after and Invitation groups. The top panel (above the black line) shows how we initially introduce the Invitation and Tell-after treatments to both groups after showing them the upcoming opportunity to email Congress. Below that line, the images on the left and right show the Tell-after and Invitation treatments, respectively. The first demographic profile would show the Wave-2 group to which they are assigned, and the second profile would be their own. Note that spacing differences only show up in the presentation here.

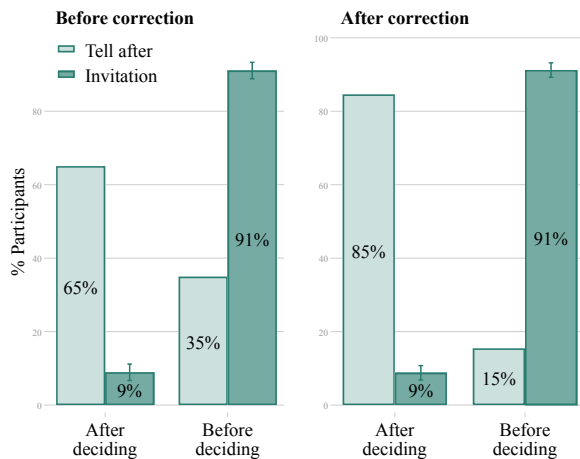
Figure B4: Flowchart to reiterate profile timing



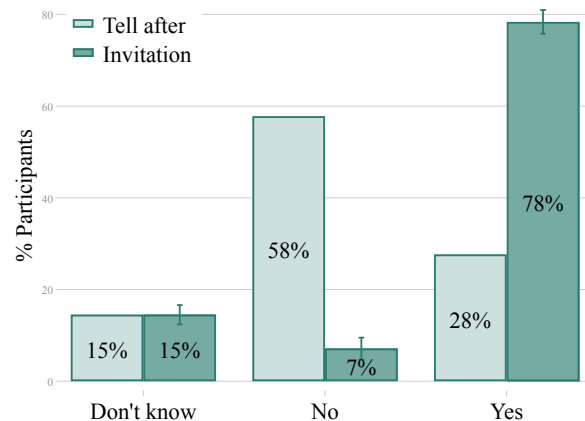
Note: These figures shows the flowcharts that we show to Invitation and Tell-after participants to reiterate the timing of when future participants will be told that they emailed Congress. Note that the flowcharts fill in the demographic profiles of each Wave-1 participant's paired Wave-2 demographic group.

Figure B5: Wave-1 comprehension of profile timing and opportunities to influence Wave 2

(a) When would future participants see the extra profile saying you emailed Congress?

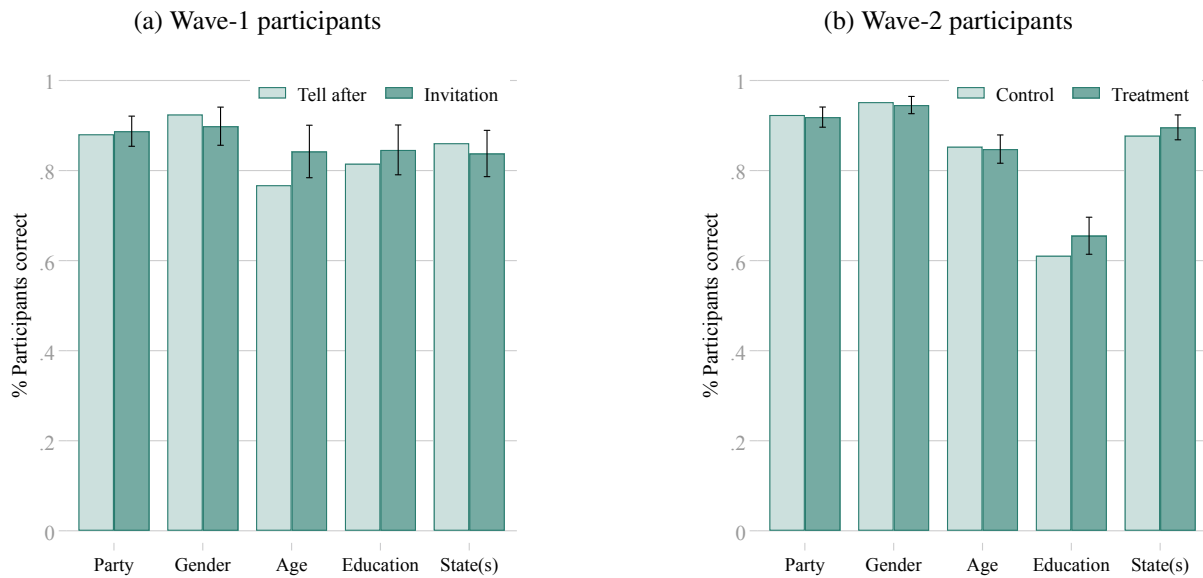


(b) In theory, could your choice to email Congress or not affect whether paired Wave-2 participants do so?



Note: This figure plots participants' responses to incentivized comprehension questions. First, the "Before correction" panel of subfigure (a) plots responses to the following, asked just after we describe the Invitation and Tell-after treatments: "If you choose to contact Congress, when would these future participants see the extra profile of you saying that you did so?" We then correct participants' answers. The "After correction" panel of subfigure (a) then plots responses to the following, asked near the end of the survey: "Remember that we told you that we would tell up to 10 future participants if you contacted Congress during this survey. [If you had contacted Congress, would participants in your paired demographic group have seen/Since you contacted Congress, will participants in your paired demographic group see] a profile saying that you did so before or after they choose whether to contact Congress or not?" Subfigure (b) then plots the distribution of responses to the following, asked near the end of the survey: "The future participants you're paired with will be choosing whether to contact Congress when they take this survey. In theory, could your decision to contact Congress or not influence whether they do so during the survey?" The error bars give the 95% confidence intervals for a regression of indicators for each response on an indicator for being in the Invitation group. N = 4,781 in the left panel of subfigure (a), N = 4,293 in the right panel of subfigure (a), and N = 4,291 in subfigure (b), corresponding to 90%, 81%, and 81% of Tell-after and Invitation participants, respectively. The Online Supplement gives more detail on all comprehension questions.

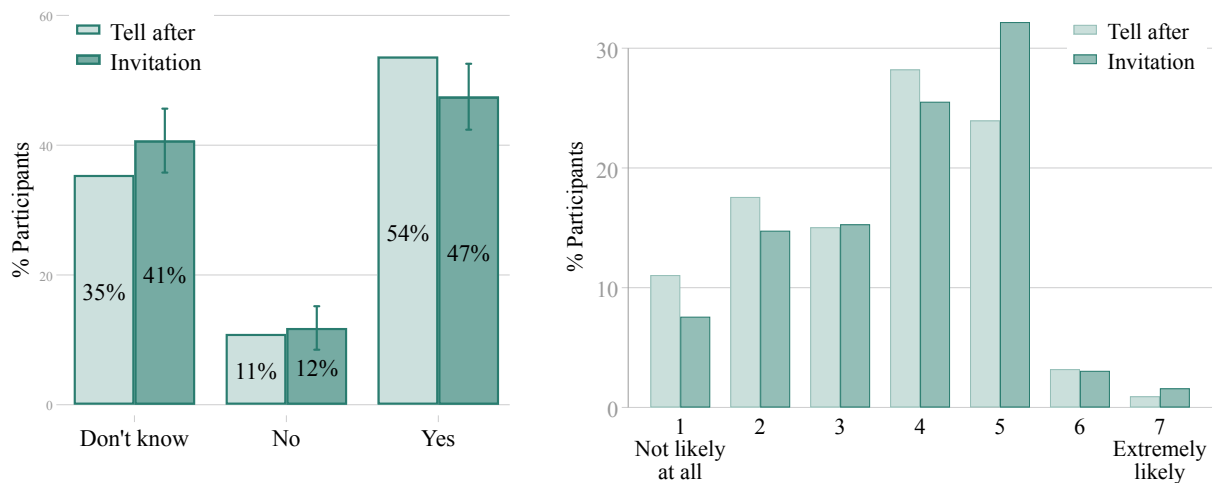
Figure B6: Whether participants correctly remember paired participants' traits



Note: This figure plots the share of Wave-1 (a) and Wave-2 participants (b) who correctly identify their matches' demographics at the end of their respective surveys. The error bars give the 95% confidence intervals for a regression of whether participants answered correctly on an indicator for being in the [Invitation/Treatment] group. We randomized subsets of Wave-1 and Wave-2 participants to answer multiple-choice questions on their matches' traits. The sample sizes in subfigures (a) and (b) are $N = \{1534, 779, 777, 779, 776\}$ and $N = \{1907, 1907, 1907, 1904, 1904\}$ for party, gender, age, education, and state group, respectively. The Online Supplement details all comprehension questions.

Figure B7: Wave-1 beliefs on action after the survey

(a) In theory, could you influence whether targets do environmentally-friendly things after the survey? (b) How likely is your decision to affect what targets do after the survey?



Note: Subfigure (a) plots the distribution of Tell-after and Invitation participants' responses to the question shown in the figure title. The error bars give the 95% confidence intervals for a regression of indicators for each response on an indicator for being in the Invitation group. Participants were then asked to rate how likely it is that their choice to email Congress or not would affect what paired participants do after finishing the survey. Subfigure (b) plots the distribution of responses. We define "somewhat likely" as 4 or above. $N = 1,541$ and $N = 1,546$ in (a) and (b), respectively. The Online Supplement gives more detail on all comprehension questions.

Table B1: Wave-1 attrition results and other secondary outcomes

	(1)	(2)	(3)	(4)	(5)
	Observe if emailed	Sample attrition Answered demand effects	Answered % others emailing	Estimated % Wave 1 emailing	Experimenter demand (std)
Tell	-0.115*** (0.007)			10.782*** (0.662)	0.313*** (0.031)
Invite	0.005 (0.009)	0.004 (0.010)	0.012 (0.011)	-0.639 (0.644)	0.059** (0.028)
Pure control mean	0.992	0.830	0.820	29.766	0.000
N	8937	5321	5321	6071	6163

Note: This table tests for treatment gaps in survey attrition and other secondary outcomes in the Wave-1 experiment. Column 1 tests for treatment gaps in whether participants stay in the survey through actively choosing whether to email Congress. Columns 2 and 3 test for differential response rates to secondary outcomes among only the Invitation and Tell-after groups. Column 4 estimates treatment effects on participants' estimates for the share of other participants who emailed Congress via the form. We incentivize these beliefs by randomly choosing 10 participants and paying them \$5 if their answer is within 10pp of the truth. Column 5 estimates treatment effects on participants' reports for how strongly they thought we (the researchers) wanted them to email Congress during the survey, elicited from 1 (Not at all) to 6 (Very much so). We elicit the outcomes in columns 4 and 5 just after participants choose whether to email Congress or not; we ask these of all Tell-after and Invitation participants, while randomizing Control participants to answer one or the other. These regressions include all controls in our main Wave-1 regressions (Appendix B.2). We present heteroskedasticity-robust standard errors in parentheses and indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

Table B2: Wave-1 sample summary and balance

	(1)	(2)	(3) (4)		(5)	(6)	(7)	(8)
	Full sample Mean	Control Mean	Tell after		Invitation			
			vs. Control		vs. Control		vs. Tell-after	
			Δ Mean	<i>p</i> -value	Δ Mean	<i>p</i> -value	Δ Mean	<i>p</i> -value
Woman	0.638	0.632	0.006	(0.617)	0.012	(0.317)	0.006	(0.644)
Hispanic	0.026	0.027	-0.001	(0.803)	0.000	(1.000)	0.001	(0.803)
Has $\geq$ 4-year college degree	0.826	0.828	-0.007	(0.484)	0.001	(0.920)	0.008	(0.424)
Age ranges:								
20-39	0.114	0.119	-0.019**	(0.018)	-0.001	(0.901)	0.019**	(0.035)
40-59	0.341	0.337	0.016	(0.182)	-0.001	(0.934)	-0.017	(0.191)
60-79	0.545	0.544	0.003	(0.817)	0.002	(0.878)	-0.001	(0.943)
Income bins (USD):								
Less than 50,000	0.167	0.170	-0.004	(0.689)	-0.007	(0.484)	-0.003	(0.764)
50,000-99,999	0.321	0.325	-0.009	(0.453)	-0.008	(0.505)	0.001	(0.939)
100,000-149,999	0.234	0.231	-0.004	(0.716)	0.015	(0.173)	0.018	(0.134)
150,000-199,999	0.130	0.126	0.007	(0.437)	0.007	(0.437)	-0.000	(1.000)
200,000 or more	0.148	0.147	0.009	(0.317)	-0.008	(0.374)	-0.017*	(0.089)
Residence by state marginality:								
Red state	0.236	0.231	0.017	(0.122)	0.002	(0.856)	-0.016	(0.182)
Blue state	0.441	0.438	-0.005	(0.701)	0.016	(0.218)	0.021	(0.134)
Purple state	0.322	0.331	-0.012	(0.317)	-0.017	(0.157)	-0.005	(0.701)
Climate beliefs:								
Climate worry (1-7)	6.421	6.430	-0.013	(0.516)	-0.016	(0.446)	-0.003	(0.892)
Desire for climate action (1-7)	6.734	6.735	-0.001	(0.947)	0.000	(1.000)	0.001	(0.947)
Perceived local impacts (1-7)	5.499	5.496	0.026	(0.298)	-0.015	(0.564)	-0.040	(0.139)
Political engagement and beliefs:								
Political-engage. index (std)	0.000	-0.003	0.005	(0.848)	0.005	(0.841)	-0.000	(1.000)
Prev. contacted reps	0.731	0.733	-0.009	(0.413)	0.004	(0.716)	0.013	(0.279)
Prev. donated	0.819	0.816	0.007	(0.484)	0.004	(0.689)	-0.003	(0.785)
Prev. canvassed	0.081	0.081	-0.001	(0.886)	-0.001	(0.886)	-0.000	(1.000)
Prev. signed petition	0.828	0.835	-0.010	(0.317)	-0.014	(0.162)	-0.004	(0.689)
Prev. phonebanked	0.111	0.100	0.024***	(0.003)	0.013	(0.104)	-0.011	(0.222)
Political-efficacy index (std)	-0.000	0.008	-0.006	(0.810)	-0.020	(0.442)	-0.015	(0.579)
Degree prefer Dem friends (1-7)	6.038	6.040	-0.001	(0.968)	-0.007	(0.779)	-0.005	(0.853)
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 in the Control group. Appendix B.2 gives more details on these traits. Columns 3 and 4 present the differences in means between the Tell-after and Control groups and heteroskedasticity-robust *p*-values testing the null of equality across these groups, respectively; columns 5 and 6 compare the Invitation and Control groups, and columns 7 and 8 compare the Invitation and Tell-after groups. In columns 3, 5, and 7, we indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

Table B3: Categorization of free-text responses for why treatment did or did not affect Wave-1 participants

	Among all participants:				Among those affected	
	Tell-after (1)	Invitation (2)	Δ Mean (3)	p -value (4)	Tell-after (5)	Invitation (6)
Treatment affected	0.192	0.301	0.108***	(0.000)		
Stated reasons affected or not:						
Could motivate others	0.172	0.257	0.084***	(0.000)	0.659	0.668
Social-image concerns	0.053	0.057	0.003	(0.785)	0.156	0.159
Made you feel accountable	0.047	0.053	0.007	(0.484)	0.139	0.137
Could make others less likely to act	0.001	0.010	0.009***	(0.003)	0.000	0.015
Could motivate anti-climate action	0.008	0.011	0.003	(0.549)	0.017	0.007
Wouldn't or shouldn't affect what others do	0.062	0.053	-0.009	(0.413)	0.000	0.000
Don't care what others think about you	0.192	0.139	-0.053***	(0.002)	0.000	0.000
Would have contacted no matter what	0.229	0.203	-0.026	(0.171)	0.000	0.000
Wouldn't have contacted no matter what	0.099	0.081	-0.018	(0.166)	0.000	0.000
Other reason	0.128	0.136	0.008	(0.617)	0.069	0.085
No reason given	0.150	0.150	-0.000	(1.000)	0.069	0.074
Sample size	900	900			173	271

Note: This table summarizes and estimates treatment differences in Tell-After and Invitation participants' responses to the following question: "Did seeing that future participants might see that you contacted Congress affect whether you did so or not? Why or why not?" We asked this question near the close of the Wave-1 survey. A treatment-blind academic hired by the authors coded free-text responses for 1800 participants, split evenly between the Tell-After and Invitation groups. The coder first assessed whether each respondent said the treatment did affect them and then classified the reasons given into the categories shown in the table. The Online Supplement details the coding scheme. Columns 1 and 2 show the share of each treatment group that reported a reason in each category, and column 3 presents the difference in these means across treatment groups, where we indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively. Column 4 presents heteroskedasticity-robust p-values for these differences in means. Columns 5 and 6 then show the share of Tell-after and Invitation participants who stated a given reason and said the treatment affected them, among those who said the treatment affected them overall (the first row of columns 1 and 2, respectively). Note that these values need not equal the corresponding value in columns 1 or 2 divided by the share of each treatment group affected overall, as some cited a given reason but said the treatment did not affect them.

Table B4: Categorization of free-text responses for participants' belief about the study purpose

	Tell-after (1)	Invitation (2)	Δ Mean (3)	p -value (4)
Measuring rates of climate action/belief and correlates	0.262	0.196	-0.067***	(0.001)
Trying to mobilize political action	0.324	0.291	-0.033	(0.134)
Testing spillovers from seeing others act	0.241	0.397	0.156***	(0.000)
Testing effects of peer accountability	0.100	0.067	-0.033**	(0.011)
Testing whether try to influence others	0.017	0.031	0.014**	(0.046)
Testing impacts of unspecified peer pressure	0.031	0.040	0.009	(0.317)
Other	0.127	0.103	-0.023	(0.125)
Participant is unsure	0.039	0.029	-0.010	(0.267)
Sample size	900	900		

Note: This table summarizes and estimates treatment differences in Tell-After and Invitation participants' guesses for the Wave-1 study purpose, elicited soon after participants choose whether to email Congress. A treatment-blind academic hired by the authors coded free-text responses for 1800 randomly-chosen participants, split evenly between the Tell-After and Invitation groups, into the categories shown in the table. The Online Supplement describes the coding scheme for these purpose guesses. Columns 1 and 2 show the share of each treatment group that reported a reason in each category, and column 3 presents the difference in these means across treatment groups, where we indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively. Column 4 presents heteroskedasticity-robust p-values for these differences in means.

B.2 Outcome and control variables: Waves 1 and 2

This section describes our primary outcome variables and control variables in Waves 1 and 2. Throughout, we highlight any differences between our approaches in the two waves.

B.2.1 Defining outcomes: Merging participants with email records

Our main outcomes in the Wave-1 and Wave-2 experiments are binary variables for whether participants initially opt into the process of emailing Congress and ultimately match to a record of an email to Congress sent via our form. In the text and exhibits, we may refer to the latter as “emailing Congress,” though some additional participants may actually send emails to Congress that we cannot match to them.

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 participants⁴⁹ and 1,834 were sent by unique Wave-2 participants.

1. In both Waves 1 and 2, we first merge 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, 145 emails merge at this stage, for a total of 93% of emails merged. In Wave 2, 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 those where the participant did not provide a last name in the survey consent, the participant said in the survey that they emailed Congress, and the email was sent within 1 hour of the participant starting the survey. This step merges 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%), with no gaps by treatment.

B.2.2 Defining control variables

We elicit all control variables at the start of the Wave-1 and Wave-2 surveys. We include the following controls in our main Wave-1 and Wave-2 specifications, but our results are robust to excluding them:

Demographic controls:

- *Age:* We control for indicators for 5-year age bins from {20-24} through {75-79}.

⁴⁹This total includes emails sent by participants who are not included in the Wave-1 experiment itself, but rather who were recruited directly for the Target-choice experiment (Section 5; Appendix C.3) or randomized to the belief sample (Section 6.1.5).

- *State*: We control for indicators for the state in which participants currently live.
- *Gender*: We control for an indicator for identifying as a woman. Recall that we restrict to those who identify as men or women.
- *Education*: We include indicators for educational attainment, selected 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)}.
- *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}. We control for indicators for each income category.
- *Ethnicity*: We control for whether participants select that they identify as Hispanic or Latino.

Climate-belief controls:

- *Climate worry*: We ask participants, "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 SD 1 in the Wave-1 and Wave-2 experimental samples.
- *Desire for climate action*: We ask participants, "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 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 SD 1 in both experimental samples.
- *Local climate impacts*: We ask participants, "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 from 1 (Not at all) to 7 (To an extremely high degree). We standardize this variable to have mean zero and SD 1 in both experimental samples.

Political controls and screeners:

- *Political affiliation*: In Wave 1, all participants are members of the Democratic party and our regressions do not control for political affiliation. Our Wave-2 regressions split the sample between Democratic- and Republican-leaners and then control for indicators that participants consider themselves to be members of each party. See Appendix [A.1](#) for additional details on these variables.
- *Political efficacy*: We elicit participants' 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 their demands.

We standardize these variables to have mean zero and SD 1 in both 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 SD 1 in each sample and control for this index.

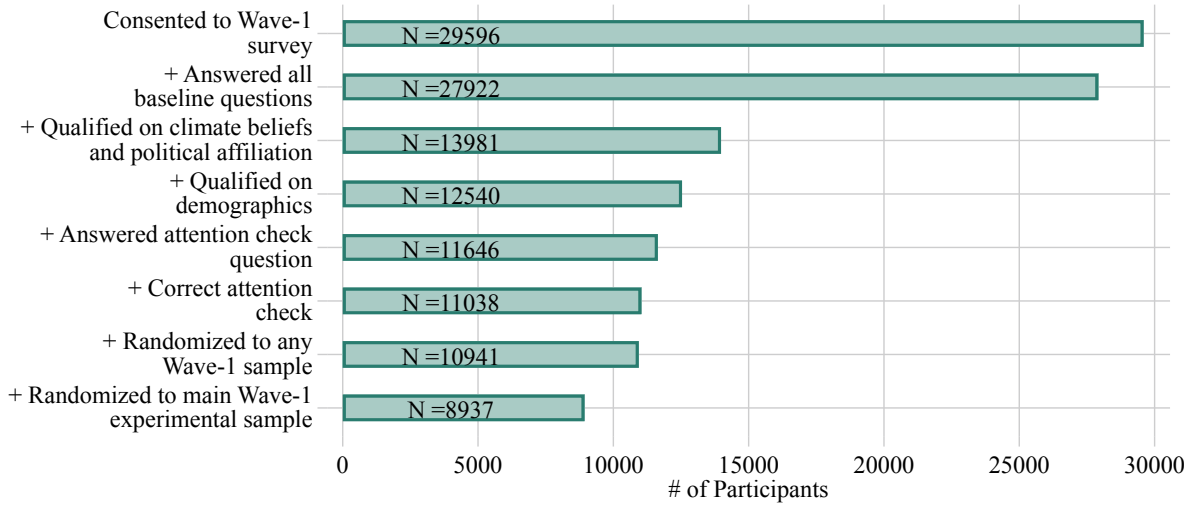
- *Baseline political engagement:* We elicit participants’ baseline political engagement with the following framing: “Some people get directly involved in social and political issues, while others don’t have the time or interest. In the last two years, have you done any of the following things? (In other 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 a political-engagement index by standardizing indicators for each of the above to have mean zero and SD one in each experimental sample, adding these together, and then standardizing the sum to have mean zero and SD one in each sample.

B.3 Wave-1 recruitment

Appendix Figure B8 summarizes Wave-1 recruitment. We recruited Wave-1 participants using ads on Facebook, Instagram, and Twitter in April-June 2023 (Appendix Figure A1). 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 impose the demographic screening criteria (footnote 6), leaving us with a sample of 12,540 participants. See Appendix A.1 for details on the screening questions.

Figure B8: 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 this project. 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 5 and 6.

B.4 Wave-1 robustness

Appendix Figure B9 shows the robustness of our main Wave-1 results to a range of perturbations.

Sample definitions: The Wave-1 results are robust to changes in the experimental sample. While we pre-registered a total sample of 8,200 participants, we did not specify whether this would include participants who left the survey before explicitly choosing to email Congress. We stopped Wave-1 recruitment when 8,269 participants had explicitly chosen whether to email Congress, but our main regressions include all of the 8,937 participants who were randomized into a Wave-1 treatment arm and assume that those who attrit after randomization (thus, after they saw the upcoming email choice) would not have emailed Congress had they continued (Section 4.3). The Wave-1 estimates are robust to restricting to the first 8,200 participants randomized to a Wave-1 treatment, 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 email choice.⁵⁰

Study comprehension: Our results for the Invitation versus Tell-after gap are robust to restricting to participants who correctly reported when Wave-2 participants would see that they emailed Congress and to those

⁵⁰As expected, restricting to those who make explicit email choices substantially increases our estimates for the impacts of the Tell-after arm relative to Control: Tell-after and Invitation participants are less likely to finish the Wave-1 survey (Section 4.3) and thus were more likely to have outcome variables set to zero.

who correctly reported whether their email choice could affect Wave-2 action (Appendix Figure B5).⁵¹

Control variables: Our Wave-1 results are robust to the set of controls we include: no controls, only demographics, the full set of controls for demographics, baseline beliefs, and baseline political engagement that we use in our main regressions, and a set of controls selected by double-Lasso regressions from participants' demographics, baseline beliefs, and dummy variables for particular forms of past political engagement (Chernozhukov et al., 2018). See Appendix B.2 for details on these controls.

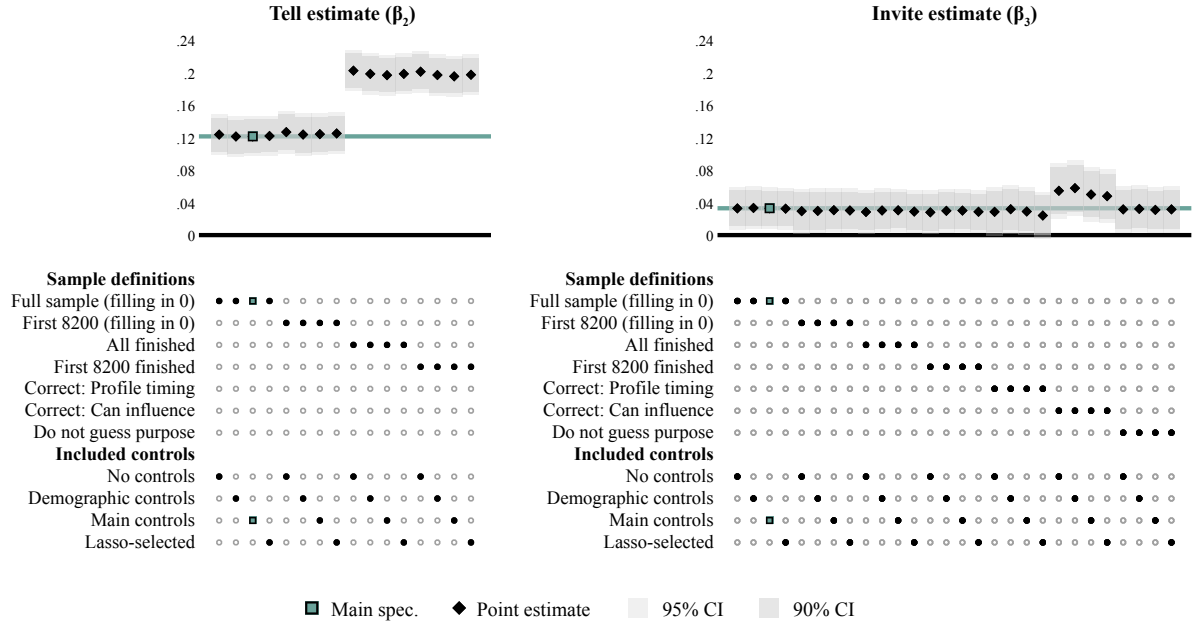
Experimenter demand effects: Our Wave-1 results are also robust to adjusting for experimenter demand. While NGOs trying to mobilize advocacy would intentionally create strong demand effects, we minimize demand effects by repeatedly telling participants that whether they email Congress does not help our research and by not directly telling Invitation participants that their action could affect Wave-2 matches' action. We thus test whether participants try to influence others without encouragement to do so; our estimates then likely under-estimate Americans' reaction to NGO-hosted opportunities to mobilize others.

For completeness, we show that our estimates of influence motives are robust to eliminating any differential demand effects that remain. We elicit perceived experimenter demand by asking participants to rate how strongly they think we wanted them to email Congress; perceived demand is 0.06sd higher among Invitation participants than Tell-after participants (column 5, Appendix Table B1). In 500 simulations, we randomly drop Invitation participants who report maximum experimenter demand until there is zero differential demand between the Invitation and Tell-after groups; we then re-estimate our main Wave-1 regressions in each adjusted sample. Our point estimates fall on average by only a small amount when we correct for differential demand (Appendix Figure B10). Moreover, our results are fully robust to excluding the 2-3% of Tell-after and Invitation participants who correctly guess that the study purpose was to test if participants try to influence others' behavior (Appendix Table B4; Appendix Figure B9).

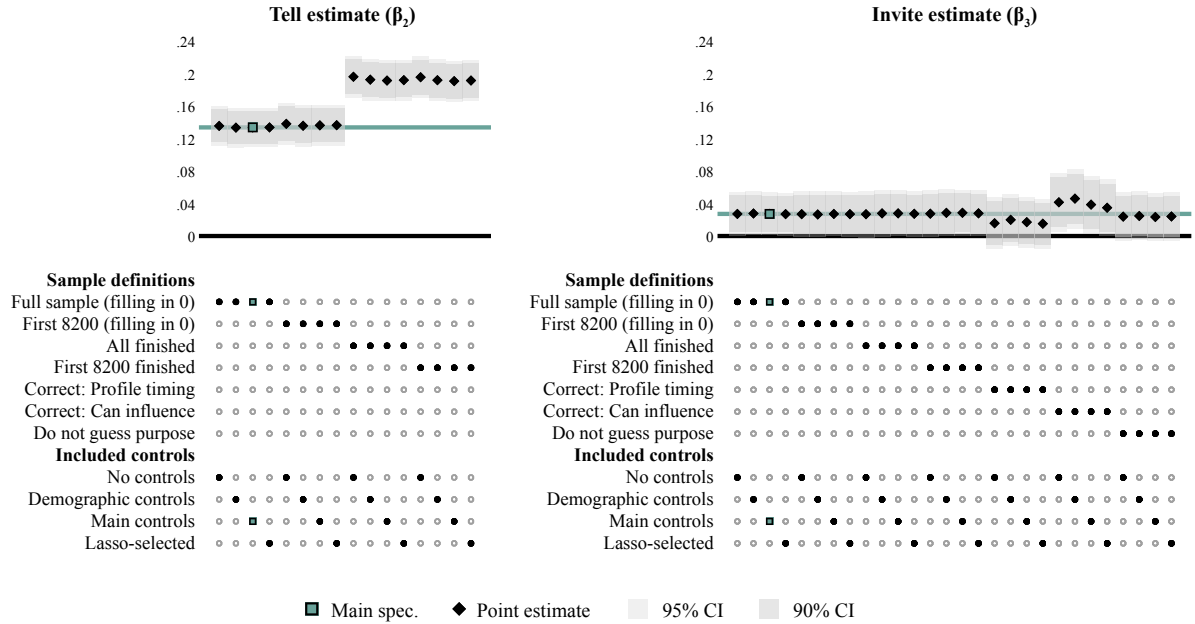
⁵¹We only asked these comprehension questions in the Tell-after and Invitation groups, so we cannot test that our estimates of the Tell-after effect are robust to this restriction. Restricting to those who correctly answered the comprehension question on profile timing keeps 67% and 75% of the Tell-after and Invitation groups, respectively, while 46% and 64% of the Tell-after and Invitation groups correctly answered whether they could influence others. Note that about 19% of participants in each treatment group had attrited before answering these comprehension questions and are excluded from these high-comprehension samples.

Figure B9: Wave-1 specification charts

Panel A. Whether participants start the email process

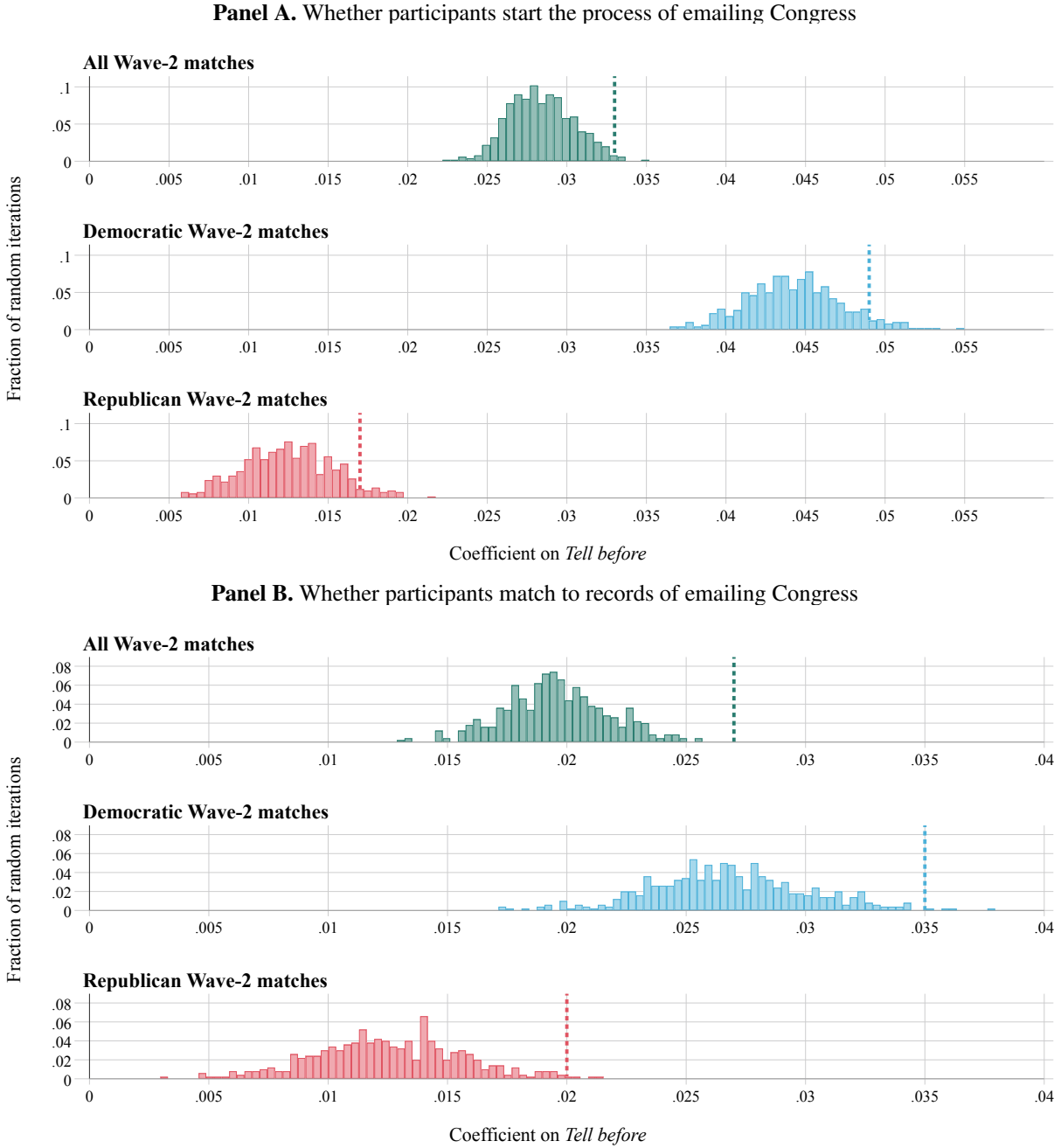


Panel B. Whether participants match to an email record



Note: This figure plots estimates for β_2 and β_3 in Equation 4 under a series of specification tests. The outcomes in Panels A and B are whether participants start the email process and match to an email record, respectively. We estimate treatment effects under various sample restrictions and control specifications, as detailed in Appendix B.4. The teal squares indicate our main specification. Here, “filling in 0” refers to assuming that any participant who left the survey after randomization, and thus after seeing the upcoming opportunity to email Congress, would not have emailed Congress had they continued. This lets us keep all randomized participants in our regression sample, while the “finished” samples refer to restricting to participants for whom we observe active choices to email Congress or not.

Figure B10: Estimated Invitation effect (β_3) with adjustment for differential demand effects



Note: This figure plots estimates for β_3 in Equation 4 when we adjust for differential experimenter demand between the Tell-after and Invitation groups. We run 500 iterations in which we drop Invitation participants who report experimenter demand at 6 on a 6-point scale until we equalize average experimenter demands between the groups. We re-estimate Equation 4 in each of these samples, across all participants and separately among those paired with Wave-2 Democrats and Republicans. The dashed line in each figure gives our main treatment estimates (Table 2).

C Appendix to the Target-choice experiment

C.1 Additional exhibits

Figure C1: Explanation of the Target-choice set-up

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:

	Leans: Democrat
	Says climate change is: Human-caused
	State: Lives in Michigan, Wisconsin, Iowa, or Minnesota
Name: Daniel	
Age: 50-64	
Gender: Man	
College: 4-year degree or more	

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."**

1 Already decided	2 Hasn't been asked	3 Hasn't been asked	4 Already decided	5 Hasn't been asked	6 Already decided	7 Hasn't been asked	8 Already decided	9 Hasn't been asked	10 Hasn't been asked
11 Already decided	12 Hasn't been asked	13 Already decided	14 Already decided	15 Already decided	16 Hasn't been asked	17 Already decided	18 Hasn't been asked	19 Hasn't been asked	20 Already decided

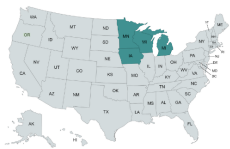
Here's what those mean:

- **"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 C2: Explanation of the Target-choice set-up, 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:

I emailed Congress to push for climate action!	
	Leans: Democrat
	Says climate change is: Human-caused
	State: Lives in Michigan, Wisconsin, Iowa, or Minnesota
	
Name: Daniel	
Age: 50-64	
Gender: Man	
College: 4-year degree or more	

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:

I emailed Congress to push for climate action!	
Please consider joining me!	
	Leans: Democrat
	Says climate change is: Human-caused
	State: Lives in Michigan, Wisconsin, Iowa, or Minnesota
	
Name: Daniel	
Age: 50-64	
Gender: Man	
College: 4-year degree or more	

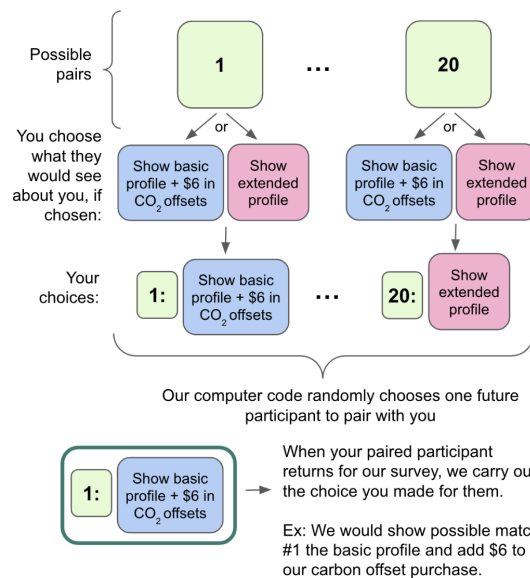
(Note that it includes an invitation to join you in acting, since they haven't yet decided whether to contact Congress or not.)

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.

Note: This figure presents screenshots of the experiment flow in which we explain the Target-choice experimental set-up to participants. We randomize whether we present the basic or extended profiles as option 1 or 2 in slides 2 through 8, whether we present the profiles shown to "Already decided" or "Haven't been asked" participants first in slides 7 and 8, and whether the example flowchart in slide 9 shows a basic or extended profile being chosen to be implemented. The survey flow then goes through a series of comprehension questions, shown in the Online Supplement, before participants make their profile choices.

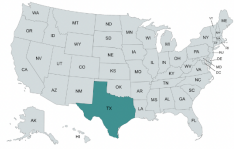
Figure C3: Examples of binary choices for possible Target-choice matches

(a) Without climate-belief info

Here is possible match #1. Which option should we carry out if you are randomly paired with them?



Leans: Democrat
College: 4-year degree or more
State: Lives in Texas
Name: Melissa
Action status: Already decided
Age: 50-64
Gender: Woman



Show basic demographic profile and buy \$5 in carbon offsets

Show extended profile saying you contacted Congress

(b) With climate-belief info

Here is possible match #1. Which option should we carry out if you are randomly paired with them?



Leans: Democrat
College: 4-year degree or more
Says climate change is: Human-caused
State: Lives in Texas
Name: Melissa
Action status: Already decided
Age: 50-64
Gender: Woman

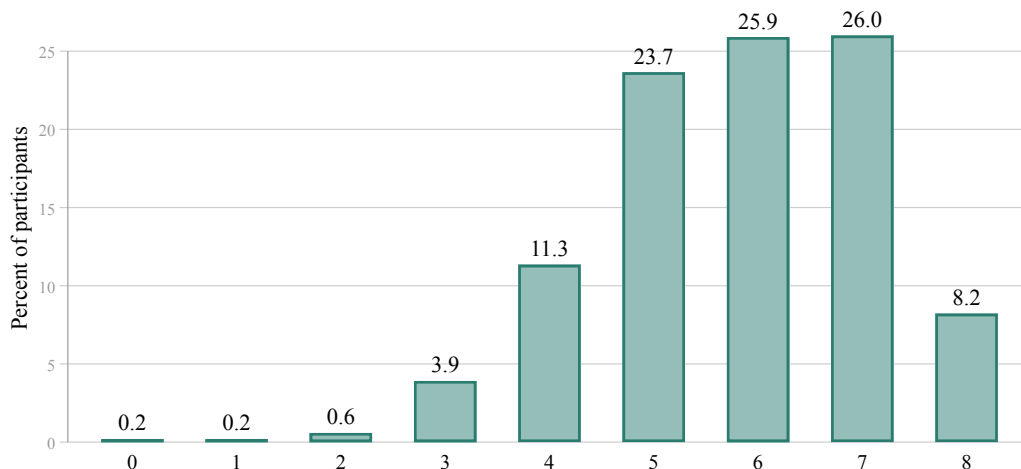


Show basic demographic profile and buy \$5 in carbon offsets

Show extended profile saying you contacted Congress

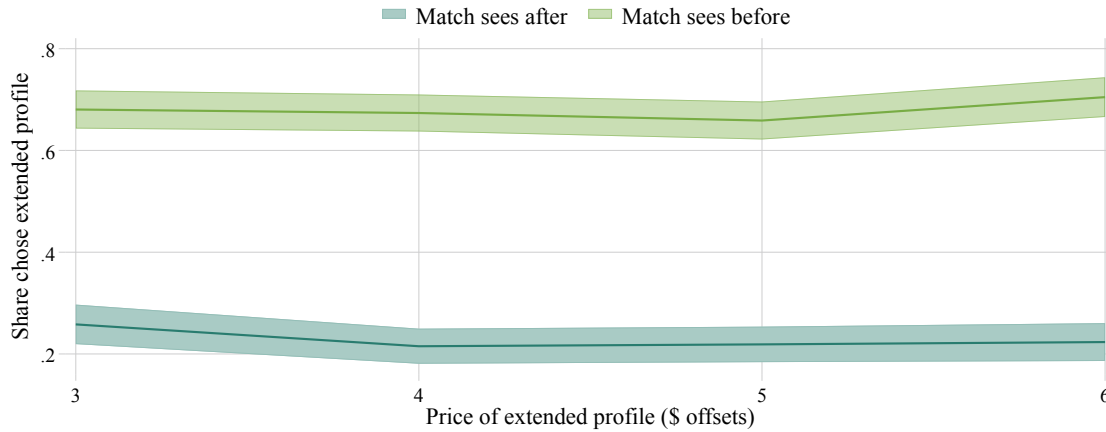
Note: This figure shows screenshots of pages on which Target-choice (TC) participants choose between a basic and extended profile for each possible match. Across TC participants, we randomize some to see profiles of possible matches that include that they believe climate change is human-caused (subfigure (a)), while randomizing others to see profiles without this information (subfigure (b)). Participants make these choices for 20 possible matches. To encourage attention to each possible match, participants must stay on each choice page for 8 seconds. Matches are real people who vary in their demographics and political leanings. Within TC participant, we randomize whether possible matches would see their profile before or after deciding whether to email Congress; this timing is shown in “Action status,” where those who would see the TC participants’ profile before or after making their email choice are labeled as “Haven’t been asked” and “Already decided,” respectively.

Figure C4: Distribution of accuracy on Target-choice comprehension questions



Note: This figure plots the percent of participants who correctly answer 0 through 8 comprehension questions correctly in the main TC experiment. The figure restricts to those who finished all 20 binary choices, all of whom were asked 8 comprehension questions and make up our main sample. See the Online Supplement for details on comprehension questions.

Figure C5: Inelastic demand for extended profiles with respect to offset price



Note: This figure plots the share of choices in which participants chose extended profiles over carbon offsets against the “price” in offset dollars, split by whether matches would see the profile before or after choosing whether to email Congress. The figure restricts to the main analysis sample. Profile choices are inelastic with respect to offset amounts.

Table C1: Attrition in the Target-choice experiments

	(1)	(2)	(3)
	Finish 20 target choices		
	<i>Round 1</i>		
	Main TC variant	Both TC variants	<i>Round 2</i>
Sees matches' climate beliefs	-0.006 (0.016)		
Has money option		-0.083** (0.038)	
Can choose politics			-0.023 (0.023)
Control means	0.926	0.758	0.790
N	1109	1516	1278

Note: This table analyzes attrition in the main and round-2 Target-choice (TC) samples. The outcome variable is whether participants complete profile choices for all 20 possible matches. Column 1 restricts to participants randomized to the main TC variant (Section 5) and tests for differential attrition between those randomized to see profiles with or without information on matches' climate beliefs. Column 2 tests for differential attrition between those randomized to the main TC variant and those randomized to the gift-card TC variant (Appendix C.5). These regressions control only for the time period in which we recruited participants, as we changed randomization probabilities at several points to meet recruitment quotas. Column 3 tests for differential attrition between those randomized to have and not have the option to hide their politics in the second-round TC experiment (Section 6.2). We present heteroskedasticity-robust standard errors in parentheses and indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

Table C2: Summary statistics and balance in the Target-choice (TC) sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Comparing samples			Balance within TC sample		
	Wave-1	TC		See beliefs	Do not see beliefs	
	Mean	Δ Mean	<i>p-value</i>	Mean	Δ Mean	<i>p-value</i>
Woman	0.632	0.128***	(0.000)	0.770	-0.021	(0.437)
Hispanic	0.027	0.013**	(0.031)	0.044	-0.010	(0.405)
Has $\geq$ 4-year college degree	0.828	0.034***	(0.001)	0.874	-0.026	(0.238)
Age ranges:						
20-39	0.119	0.026**	(0.012)	0.162	-0.036	(0.102)
40-59	0.337	0.017	(0.206)	0.358	-0.008	(0.790)
60-79	0.544	-0.043***	(0.003)	0.480	0.044	(0.156)
Income bins (USD):						
Less than 50,000	0.170	-0.004	(0.698)	0.177	-0.023	(0.318)
50,000-99,999	0.325	0.013	(0.358)	0.341	-0.007	(0.816)
100,000-149,999	0.231	0.007	(0.573)	0.226	0.024	(0.374)
150,000-199,999	0.126	-0.008	(0.395)	0.113	0.011	(0.582)
200,000 or more	0.147	-0.008	(0.447)	0.142	-0.005	(0.820)
State marginality:						
Red state	0.231	-0.011	(0.379)	0.217	0.006	(0.818)
Blue state	0.438	0.018	(0.201)	0.484	-0.058*	(0.062)
Purple state	0.331	-0.008*	(0.566)	0.299	0.052*	(0.073)
Climate beliefs:						
Climate worry (1-7)	6.430	0.118***	(0.000)	6.560	-0.028	(0.525)
Desire for climate action (1-7)	6.735	0.089***	(0.000)	6.819	0.008	(0.775)
Perceived local impacts (1-7)	5.496	0.080***	(0.005)	5.600	-0.053	(0.393)
Political engage. and beliefs:						
Political-engage. index (std)	-0.003	0.204***	(0.000)	0.225	-0.052	(0.378)
Prev. contacted reps	0.733	0.070***	(0.000)	0.801	0.003	(0.905)
Prev. donated	0.816	0.048***	(0.000)	0.863	0.000	(1.000)
Prev. canvassed	0.081	0.009	(0.283)	0.089	0.001	(0.956)
Prev. signed petition	0.835	0.045***	(0.000)	0.894	-0.031	(0.140)
Prev. phonebanked	0.100	0.034***	(0.001)	0.153	-0.042**	(0.046)
Political-efficacy index (std)	0.008	-0.150***	(0.000)	-0.181	0.084	(0.155)
Degree prefer Dem friends (1-7)	6.040	0.032	(0.236)	6.035	0.081	(0.156)
Sample size	3616	1023		548	475	

Note: This table summarizes the Target-choice (TC) sample. Columns 1-3 compare the TC analysis sample with the Wave-1 Control group. The TC analysis sample is all those who email Congress in the Wave-1 Control group (or do so after being directly recruited for the TC experiment), elect to take the TC survey, and complete all 20 binary profile choices. Column 1 presents means in the Wave-1 Control group, column 2 presents the difference in means between this group and the TC sample, and column 3 presents heteroskedasticity-robust p-values testing the null of equality. Columns 4-6 then test for balance on baseline traits between TC participants assigned to see or not see matches' climate beliefs. Column 4 presents means among those with climate-belief information, column 5 presents gaps in baseline traits between these groups, and column 6 gives heteroskedasticity-robust p-values testing the null of equality. Columns 3 and 6 indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively. The note to Table 1 gives more details on these traits, which are also described in Appendix B.2.

Table C3: Heterogeneity in TC partisan recruitment gaps by matches' state politics

	(1)	(2)	(3)	(4)	(5)	(6)
	Showed extended profile					
	<i>No climate beliefs</i>			<i>Sees climate beliefs</i>		
	Red	Blue	Purple	Red	Blue	Purple
Before	0.225*** (0.027)	0.330*** (0.030)	0.331*** (0.025)	0.420*** (0.030)	0.466*** (0.030)	0.421*** (0.024)
Democrat	0.159*** (0.026)	0.097*** (0.024)	0.153*** (0.023)	0.050** (0.024)	0.059** (0.024)	0.032 (0.021)
Before * Democrat	0.290*** (0.034)	0.225*** (0.033)	0.201*** (0.029)	0.102*** (0.031)	0.055* (0.031)	0.133*** (0.026)
Mean: After, Rep	0.146	0.179	0.174	0.212	0.192	0.209
# Participants	475	475	475	548	548	548
# Choices	2823	2771	3906	3230	3234	4479
<i>p-values: Before*Dem</i>	(1)=(2)=(3): 0.057			(4)=(5)=(6): 0.136		

Note: This table estimates the main TC specifications, as in Table 3, split by whether possible matches live in red, blue, or purple states (Appendix C.2). Standard errors are clustered by TC participant and are given in parentheses. We indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively. The last row gives p-values for tests of the null that the *Before * Democrat* coefficients are equal across the three state groups.

Table C4: Target-choice participants' guesses for the study purpose

	(1)	(2)	(3)	(4)	(5)
	Share guessed	Mention non-pol sim In row	In full samp	Mention politics In row	In full samp
Testing spillovers from seeing others act	0.330	0.482	0.159	0.179	0.059
Testing if think offsets or political action more effective	0.233				
Testing whether try to influence others	0.230	0.509	0.117	0.252	0.058
Measuring rates of climate action/belief and correlates	0.161				
Testing comfort sharing info with others	0.079				
Trying to mobilize political action	0.040				
Other	0.022				
Participant is unsure	0.064				

Note: This table summarizes TC participants' responses to the following question: "What do you think is our research hypothesis in this survey? It's totally fine if your guess is uncertain, but please do provide some guess. You can note in your response that you're unsure." A treatment-blind academic hired by the authors coded free-text responses for the 1,019 TC participants (of 1,023 in our main sample) who answered this question. Column 1 reports the share of participants who guessed a purpose in each category. For two purposes—testing the spillovers from invitations and testing whether people try to influence others—the coder also indicated whether the participant specifically mentioned the role of non-political similarity or politics. Columns 2 and 4 present the share who mentioned these factors among those who guessed that purpose category, while columns 3 and 5 present the share of all participants who guessed that purpose category and mentioned that factor. The Online Supplement details the coding scheme.

C.2 Forming groups of politically similar and nearby states

A key piece of information included in possible matches' demographic profiles (as well as those of participants throughout the study) is the state or group of states in which they live. We create groups of states that are geographically close and all fall in the following categories of state climate politics:

- “Red states,” where legislators would be very unlikely to vote in favor of a climate bill, even if many state residents called those politicians to say that they supported it;
- “Blue states,” where legislators would be very likely to vote in favor of a climate bill, even if not many residents called those politicians to say they supported it;
- “Purple states,” where legislators could be convinced to vote for a climate bill if they knew that enough of their constituents supported it. In those states, contacting legislators about the climate bill could be more impactful than in others.

Grouping states that fall in each of these categories lets us test whether Democrats strategically try to mobilize climate action where they expect it to have more impact on legislators' choices.

To create state clusters, we asked 101 Democrats recruited from social media to classify each of the 48 states in the contiguous US into the three climate-politics groups described above, referring to them as Groups 1, 2, and 3. We then group geographically close states among those that most participants class in the same climate-politics tier. Using 2021 estimates for the number of Americans who believe in climate change by state and political party (Howe et al., 2015), we vary the number of states in each group so that the projected number of eligible study participants is roughly equal across groups.⁵² Table C5 presents our final state groups by the three climate marginality tiers. Social media participants were likely to classify each state in the tier to which their group is ultimately assigned (Appendix Figure C6)

Table C5: State groups by tiers of climate-policy marginality

Red-state groups:	Blue-state groups:	Purple-state groups:
SC, LA, MS, AL	ME, VT, NH, MA, CT, RI	OH, PA
MT, WY, ID, UT, ND, SD, NE, KS, OK	NY, NJ, MD, DE	VA, NC, GA
MO, AR, TN, KY, IN, WV	WA, OR, CA	WI, MI, MN, IA
FL	IL	NV, AZ, CO, NM
TX		

Note: This table lists the thirteen clusters of geographically close and politically similar states that we construct. Red states are those where legislators would be unlikely to vote in favor of a climate bill, even if a fair number of state residents advocated for it. Blue states are those where legislators would be likely to vote in favor of a climate bill, even if not many state residents advocated for it. Purple states are those where legislators are on the fence about climate policy, but could be convinced to vote for it with enough citizen advocacy.

⁵²Rather than showing the single state where a participant lives in demographic profiles throughout the project, we cluster smaller-population states into groups for several reasons. First, doing so allows us to protect the anonymity of possible TC matches, who appear with a basic avatar, first name, and other demographics. Second, our experimental design in the Wave-1 and Wave-2 experiments (Sections 4 and 6.1) relies on matching demographic cells defined by gender, 15-year age ranges, educational attainment, politics, and states. We pool states to ensure that these demographic cells are large enough to truthfully pass on Wave-1 invitations to Wave-2 participants in a given cell.

C.3 Target-choice (TC) recruitment and completion

In total, 3,616 participants were assigned to the Wave-1 Control group and 1,205 were recruited in the top-up sample. Of these total 4,821 participants, 1,657 (34%) said that they emailed Congress during the survey. We were unable to confirm in real-time that they'd done so, but 85% match to email records. We invited these 1,657 participants to take the additional TC survey, and 1,519 (92%) began it. Of these, we randomize 1,350 to the main TC experiment and randomize 166 to a variant that we detail in Appendix C.5. Our main analysis restricts to participants assigned to the main TC variation.

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C.4 Target-choice (TC) robustness

Appendix Figures C7 through C9 show the robustness of our main TC results—coefficients β_1 , β_2 , and β_3 in Equation 5—to a range of perturbations.

Including design variants: First, the TC results are robust to including participants who finished all 20 target choices in the Gift-card group (Appendix C.5), not just in the main TC variant.

Study comprehension: Our results are also robust to excluding the 166 participants (16% of our main sample) who answer fewer than 5 comprehension questions correctly. Appendix Figure C4 gives the full comprehension distribution. We correct all answers, and the Online Supplement details the questions.

Experimenter demand effects: Next, experimenter demand effects could confound our results if participants guess that we are studying differential outreach by political party. Our results are robust to excluding 59 participants (6% of our main sample) who guess this study purpose in a free-text response at the end of the TC survey, coded by a treatment-blinded academic hired by the authors.⁵³ Appendix Table C4 summarizes participants' guesses for the study purpose. (See the Online Supplement for the coding scheme)

Excluding antagonistic Republican beliefs: Next, our results are robust to excluding the 29 participants (3% of our main sample) who report being concerned that Republicans would email Congress *opposing* climate policy. We aim to shut down this possibility throughout the experiment—for example, fixing the email subject line to support climate policy—and to instead test whether Democrats try to mobilize allied political action across the aisle. We observe concerns about Republican opposition by asking participants to report how they made their binary choices between basic and extended profiles—and whether there were particular features they paid attention to—after guessing the study purpose. A treatment-blind academic hired by the authors hand-coded participants responses. (See the Online Supplement for the coding scheme).

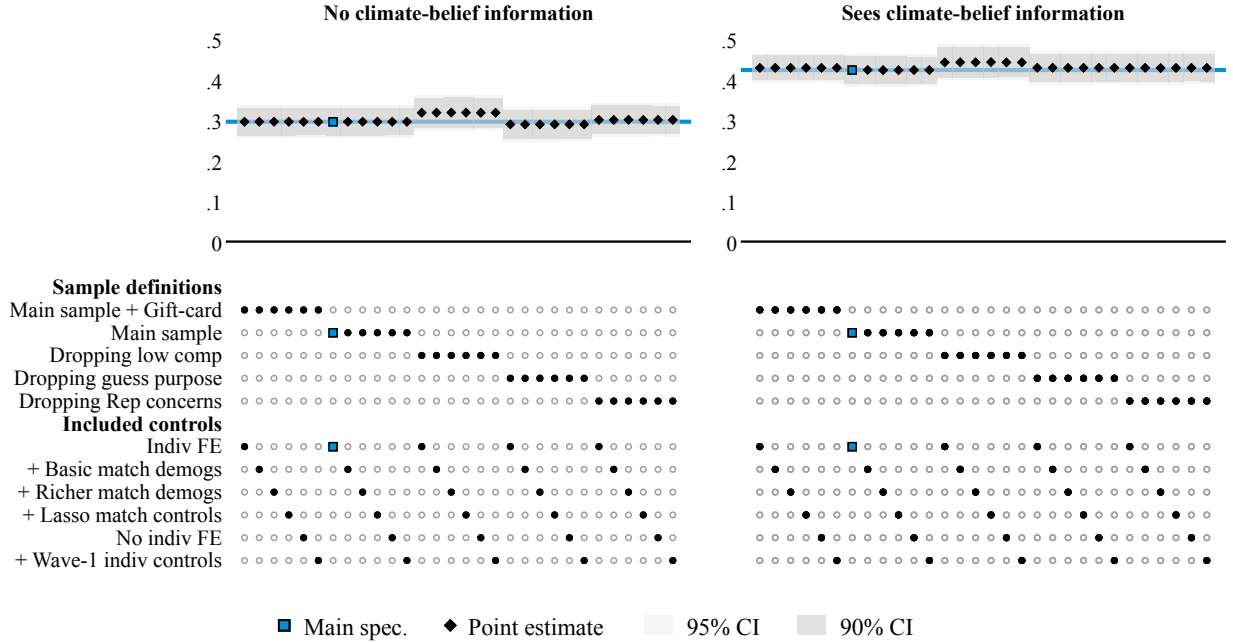
Control variables: Finally, our TC results are robust to the set of control variables we include. While our main specifications include only TC-participant fixed effects and choice-number fixed effects, our estimates are stable to adding controls for basic match demographics (gender, 20-year age categories, college attainment, and state climate-policy tier), a more detailed set of match demographics including separate fixed effects for each of the 13 match state groups and age decades, and Lasso-selected controls drawn from the full set of possible controls for match demographics and avatar traits like hairstyle and color. Our estimates are also fully stable to excluding individual fixed effects and including only the individual-level controls included in our main Wave-1 specifications (Appendix B.2).

Moreover, the coefficients on Dem_{ij} and $Before_{ij} * Dem_{ij}$ in Equation 5 are robust to controlling for the interactions of all match demographics with $Before_{ij}$ and for the main effects and interactions with $Before_{ij}$ of all traits selected by Lasso to predict Dem_{ij} . Thus, our estimates for partisan gaps in influence motives are not capturing correlations between matches' politics and other traits. (Note that we do not test

⁵³This robustness check also excludes the 4 main-sample respondents who left the survey between making their 20 binary choices and answering this prompt. The same is true for the next robustness results, in which we drop those who did not report how they made binary choices.

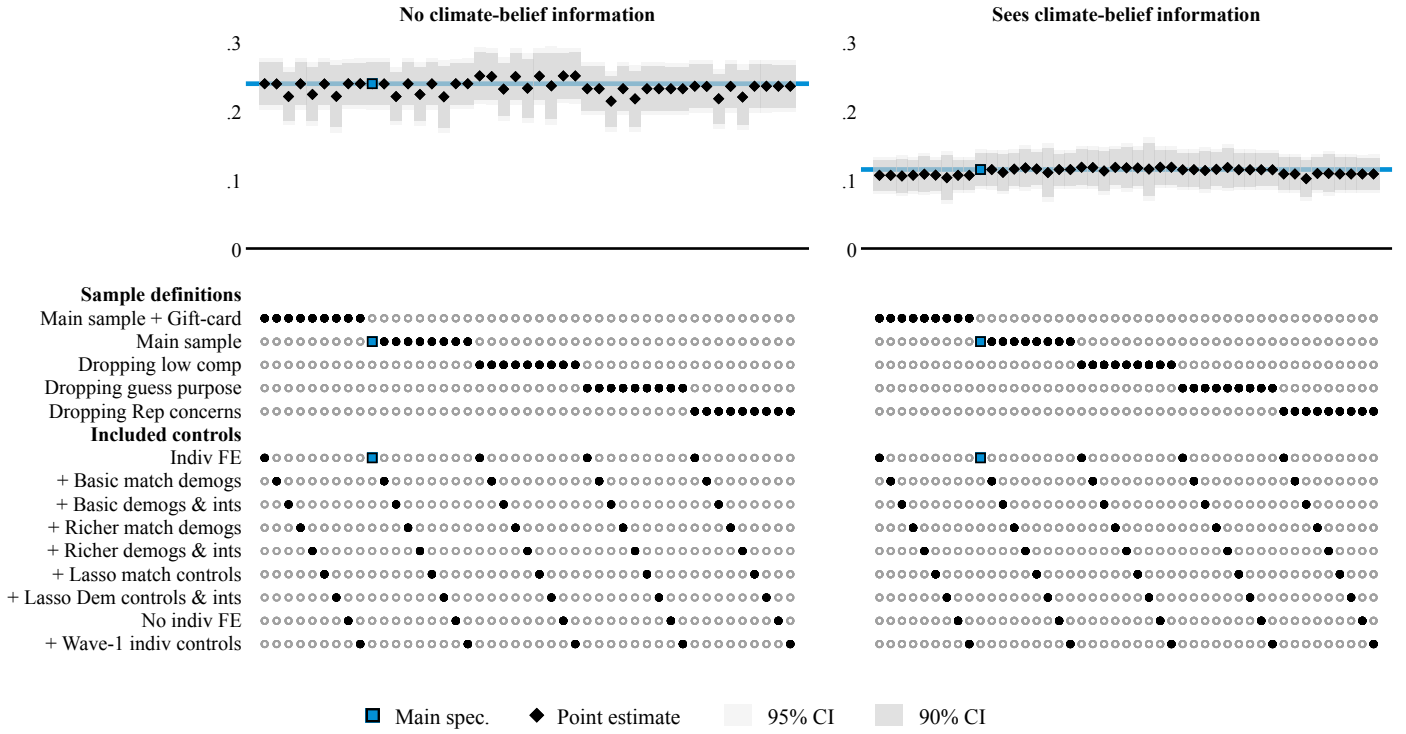
that the coefficient on $Before_{ij}$ (β_1) is robust to controlling for the interactions of $Before_{ij}$ with match demographics, as doing so changes the object that β_1 measures.)

Figure C7: Specification chart: Target-choice experiment, β_1 coefficient on $Before_{ij}$



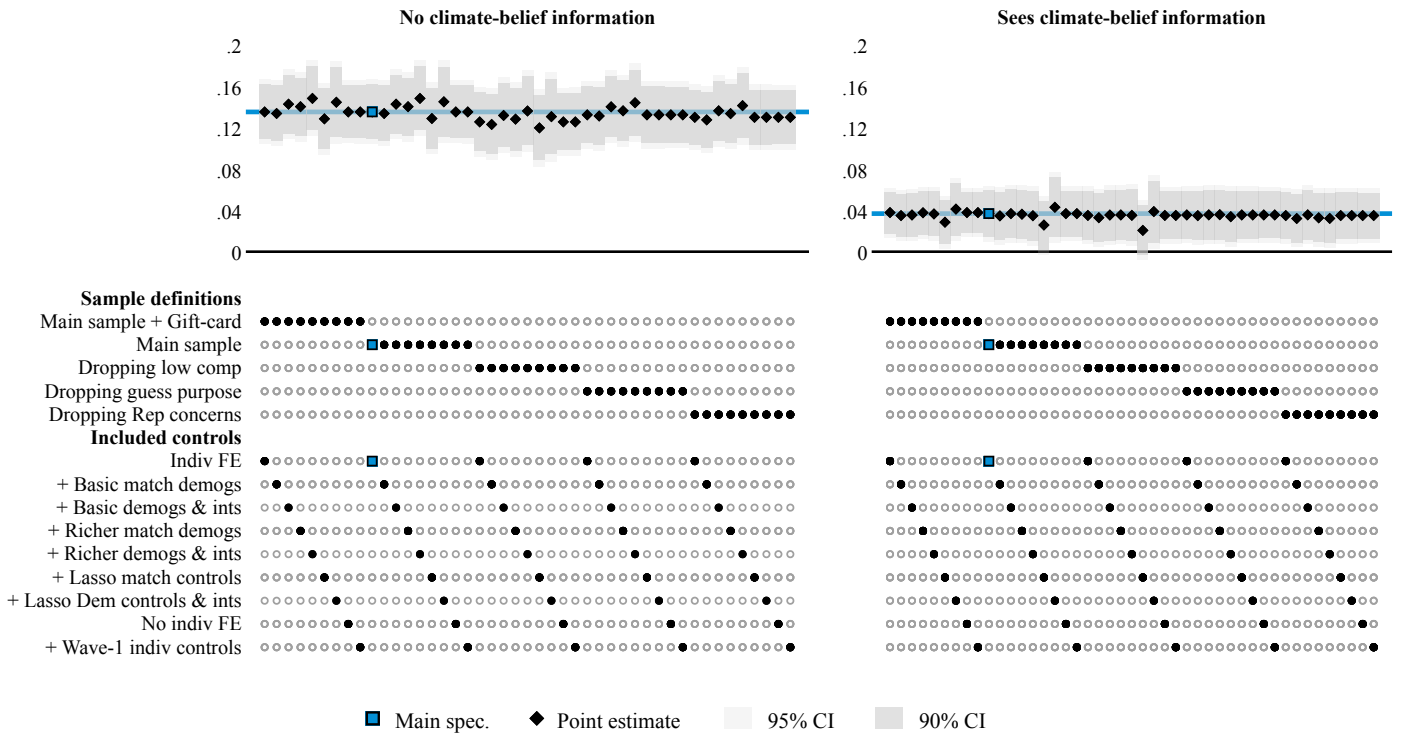
Note: This figure plots our estimates for β_1 in Equation 5 under a range of sample and control specifications (described in Appendix C.4). The panels at left and right plot coefficients in the TC subsamples who saw profiles without and with information on matches' climate beliefs, respectively. The outcome variable is whether participants pass on an extended profile showing that they emailed Congress. The blue squares indicate our main specification.

Figure C8: Specification chart: Target-choice experiment, β_2 coefficient on $Before_{ij} * Dem_{ij}$



Note: This figure is analogous to Appendix Figure C7, but with estimates for β_2 in Equation 5. Labels that include “& ints” refer to controlling for interactions of $Before_{ij}$ with the match traits listed.

Figure C9: Specification chart: Target-choice experiment, β_3 coefficient on Dem_{ij}



Note: This figure is analogous to Appendix Figure C8, but with estimates for β_3 in Equation 5.

C.5 Target-choice design with gift-card option

One threat to our interpretation of the TC experiment is that participants may not value carbon offsets; then, large differences in participants' choices between offsets and extended profiles for certain demographic groups could reflect only small differences in how much participants value trying to recruit them to email Congress. We alleviate this concern using a variant of the TC experiment in which participants also have the option to choose take-home gift cards.

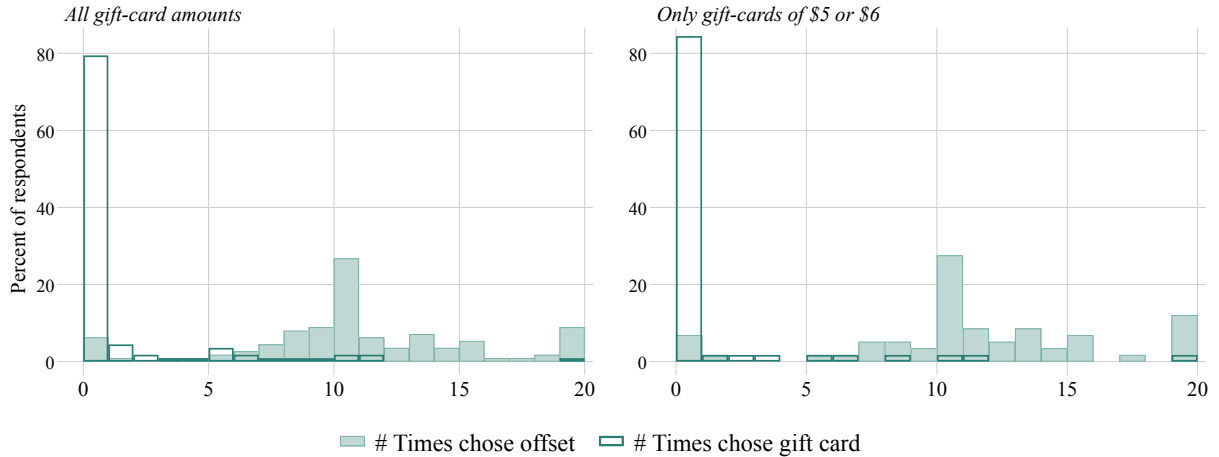
Set-up: While participants in the main TC version choose between two options for each possible match, we randomize some participants to an alternate set-up (the “Gift-card group”) in which they choose between these two baseline options and a third option: we will show the returning match the TC participant’s basic demographic profile and send the TC participant a gift card valued at the same amount as the carbon-offset donation. Participants can claim this gift card through Tango Rewards to hundreds of online retailers, including Amazon. All of these participants see profiles showing possible matches’ climate beliefs.

Differential attrition: In total, we randomize 166 participants to the Gift-card group. Participants assigned to the Gift-card group are about 8 pp (11%) less likely to finish all 20 binary choices than those in the main TC variant (column 2, Appendix Table C1). Due to this differential attrition, we restrict to participants in the main TC variant throughout our main analysis of the TC experiment. However, Appendix C.4 shows that all of our results are fully robust to including these participants in our main regressions.

TC participants value carbon offsets: While participants choose the basic profile and offset donation in an average of 10.5 choices, they choose the basic profile and gift card in an average of only 1.2 choices; about 80% of participants never choose the take-home gift card (Appendix Figure C10). Thus, most participants value one dollar of carbon offsets more highly than one dollar in a gift card.

There are two remaining concerns. First, participants might 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, participants may not value gift cards 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 C10).

Figure C10: Share of respondents choosing offset donation vs. gift card



Note: These figures plot the share of TC participants who are assigned to the Gift-card variant and complete all 20 choices and who choose the carbon-offset or gift-card option in 0 through 20 choices. Subfigure (a) pools across all of these participants, while (b) restricts to those who were randomized to choose between offsets and gift cards of \$5 or \$6 each. (Others were randomized to amounts of \$3 or \$4.)

D Appendix to the mechanism experiments

D.1 Additional exhibits

Figure D1: Wave-2 treatment: Action invitation from Wave-1 match

(a) Showing Wave-1 match's demographics

Many other Americans have participated in this survey already.

To give you a sense of who else is involved, here's a profile for a **randomly chosen** recent participant:

	Leans: Democrat
	Says climate change is: Human-caused
	State: Lives in Ohio or Pennsylvania
Name: Jordan	
Age: 35-49	
Gender: Woman	
College: No 4-year degree	

(b) Treatment: Action invitation from Wave-1 match

Remember that earlier in this survey we **randomly paired** you with Jordan and showed you their demographic profile.

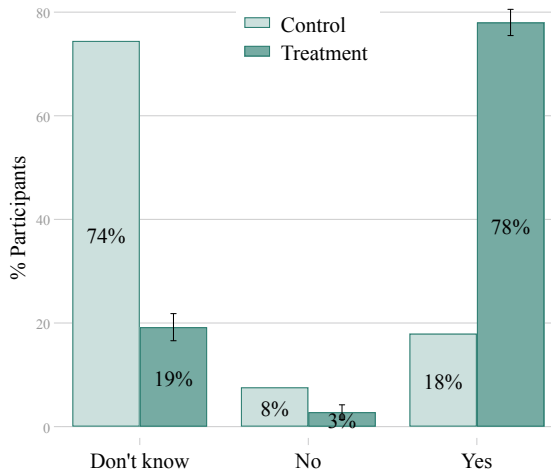
When Jordan took this survey, they **chose to contact Congress** via our form and to **show you this profile of themselves**:

I emailed Congress to push for climate action!	
Please consider joining me!	
	Leans: Democrat
	Says climate change is: Human-caused
	State: Lives in Ohio or Pennsylvania
Name: Jordan	
Age: 35-49	
Gender: Woman	
College: No 4-year degree	

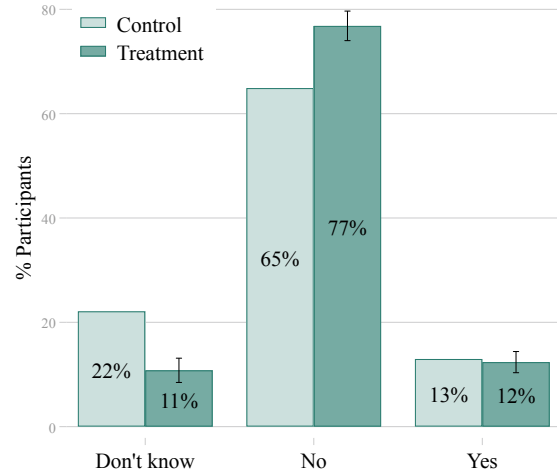
Note: This figure shows an example of how we first show Wave-2 participants a demographic profile for a randomly matched Wave-1 participant (subfigure (a)) and then show Treatment participants an invitation from this match to join in emailing Congress (subfigure (b)). Note that we ensure that these statements are always truthful, meaning that we only pass on invitations from Wave-1 participants who emailed Congress knowing that doing so meant an invitation would be passed on to a Wave-2 participant in the relevant demographic group.

Figure D2: Comprehension among Wave-2 participants

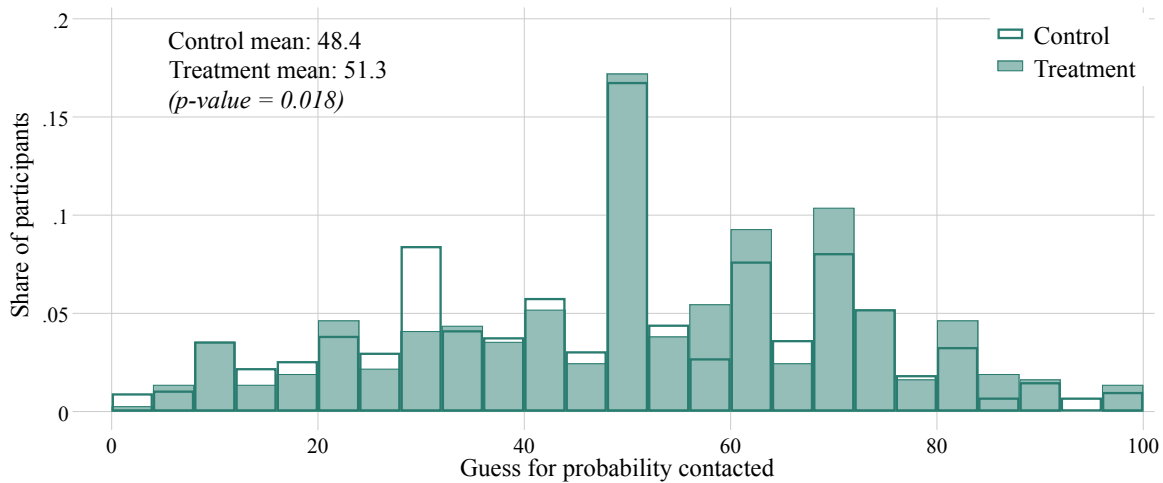
(a) Do you know whether your paired earlier participant emailed Congress while taking our survey?



(b) Will any future participants be told whether or not you contacted Congress?

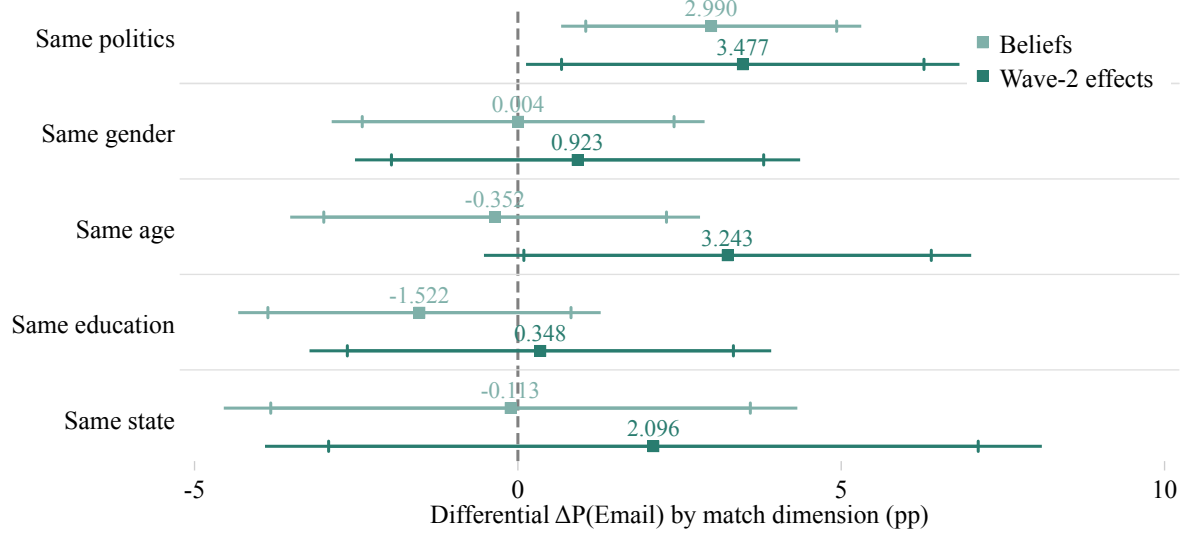


(c) If answered they don't know in subfigure (a): What would you guess is the probability that the past participant we showed you emailed Congress via our form?



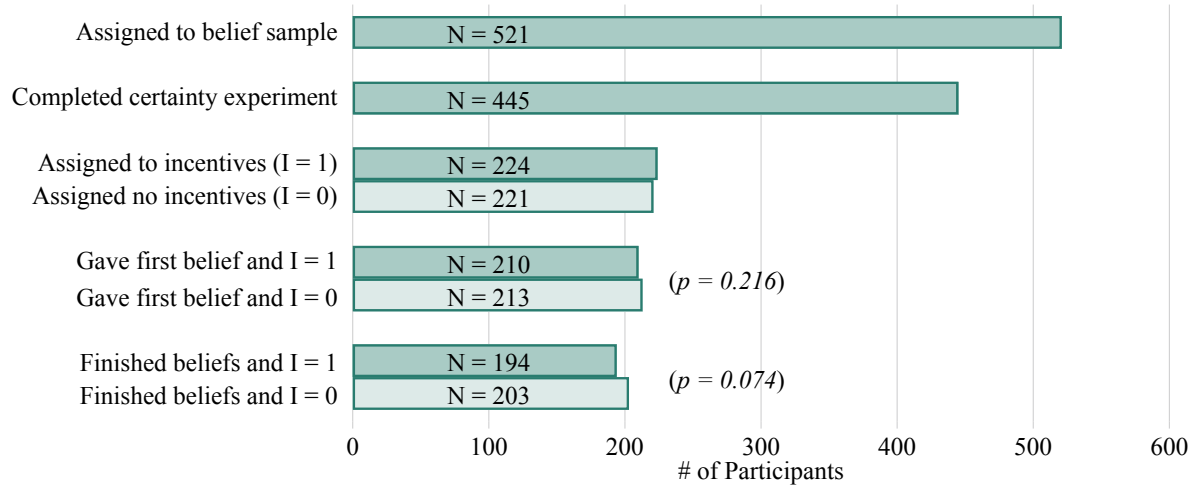
Note: This figure plots Wave-2 participants' responses to incentivized comprehension questions, which we randomize half of the participants to answer near the end of the Wave-2 survey. Subfigures (a), (b), and (c) plot responses to the questions shown in the figure headings, with sample sizes of $N = 3,827$, $N = 3,824$, and $N = 1,766$, respectively. The error bars in subfigures (a) and (b) give the 95% confidence intervals for a regression of indicators for each response on an indicator for being in the Treatment group. The p-value shown in subfigure (c) tests for equal average beliefs between the Treatment and Control groups. The Online Supplement gives more detail on all comprehension questions.

Figure D3: Differential perceptions of and actual $\Delta P(Email)$ by shared match traits



Note: The panels of this figure compare the belief sample's estimates for differential $\Delta P(Email)$ with actual heterogeneous Wave-2 invitation effects by sender-recipient similarity on particular traits. We estimate differential influence beliefs in versions of Equation 7 in which we simultaneously interact $Invited_i$ with each of the similarity traits. Likewise, we estimate actual differential $\Delta P(Email)$ by match traits in versions of Equation 6 in which we simultaneously interact $Treatment_i$ with each of these traits. The capped and uncapped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively.

Figure D4: Belief-sample completion



Note: This figure shows recruitment and attrition in the belief sample. 521 of the total 10,941 participants randomized into samples associated with the Wave-1 experiment (Appendix B.3) are randomized to the belief sample. They first complete a first round of the email-valuation experiment (Section 6.3.2) and then are randomized either to incentives (N = 224) or not (N = 221). We then plot the number of participants in each of these groups who complete one belief elicitation and who complete all four belief elicitation. Those who complete all four elicitation reports compose the belief sample we analyze. The p-values in the figure test whether the share of those who complete one or all belief reports differs between the incentives and no-incentive groups; these tests use heteroskedasticity-robust standard errors.

Figure D5: Explanation of the belief elicitation

Slide 1.

Next, we'd like to ask you to ask you to make several **predictions** about future participants who will take this survey. The survey they take will be identical to what you've done up to this point, **except that it won't include making the choices you just made.**

We're going to show you the basic demographics of two participant groups.

For each group, we'll ask you to predict **how many** out of 100 people in that group will email Congress during our survey, under a couple of circumstances.



Throughout these questions, please **do not click back** in your browser. Doing so will send you back to the beginning of the survey.

Slide 2.

Here's the **first** demographic group for which we'll ask you to make a prediction. You might recognize that they're the **same** group of future participants we said would see your basic demographic profile.

Everyone in this group would match the demographics below, except that they might have different hair or accessories.

	Leans: Republican
	Says climate change is: Human-caused
	State: Lives in Virginia, North Carolina, or Georgia
Age: 50-64	
Gender: Woman	
College: No 4-year degree	

Slide 3.

	Leans: Republican
	Says climate change is: Human-caused
	State: Lives in Virginia, North Carolina, or Georgia
Age: 50-64	
Gender: Woman	
College: No 4-year degree	

Imagine that **100 future participants** in the group above took the same basic survey you just took, except for making the 10 choices you just made.

How many of those 100 participants do you think would choose to **email Congress about climate policy** during our survey?



Slide 4.

When these future participants take our survey, we'll **randomly pair** each of them with an earlier participant in **your demographic group**.

If their paired earlier participant **emailed Congress** when they took our survey, we'll **show the future participant** an extra profile of their earlier match that looks like this:

I emailed Congress to push for climate action!	
Please consider joining me!	
	Leans: Democrat
	Says climate change is: Human-caused
	State: Lives in Florida
Name: (Filled in with pair's name)	
Age: 64-79	
Gender: Man	
College: 4-year degree or more	

(We filled in your avatar here, but the profiles would be of other participants who share all of your demographics. They would only differ from you in name and avatar.)

Figure D6: Explanation of the belief elicitation, cont.

Slide 5.

I emailed Congress to push for climate action!	
Please consider joining me!	
	Leans: Democrat
	Says climate change is: Human-caused
	State: Lives in Florida
	
Name: (Filled in with pair's name)	
Age: 64-79	
Gender: Man	
College: 4-year degree or more	

If 100 people from your paired group of future participants (see profile below) **saw one of these profiles before** they decided to email Congress or not, **how many** do you think would choose to **email Congress** during our survey?

(Remember that you guessed that 57 out of 100 future participants in this group who **didn't see** this extra profile would email Congress.)

of Participants



	Leans: Republican
	Says climate change is: Human-caused
	State: Lives in Virginia, North Carolina, or Georgia
	
Age: 50-64	
Gender: Woman	
College: No 4-year degree	

Slide 6.

Thank you for doing that! We'll now ask you to make the same kinds of predictions for **a different demographic group** of future participants who are **quite similar** to your paired group.

Their only difference is in **political leanings**.

Everyone in this group of future participants would match all of the demographics in the profile below, except that their hair or accessories could be different:

	Leans: Democrat
	Says climate change is: Human-caused
	State: Lives in Virginia, North Carolina, or Georgia
	
Age: 50-64	
Gender: Woman	
College: No 4-year degree	

Slide 7.

	Leans: Democrat
	Says climate change is: Human-caused
	State: Lives in Virginia, North Carolina, or Georgia
	
Age: 50-64	
Gender: Woman	
College: No 4-year degree	

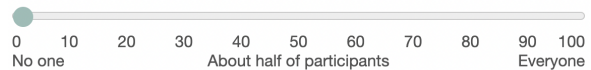
Again, imagine that **100 future participants** in the group above took the same basic survey you just took, except for the 10 choices you made.

(Assume that they **didn't see** any profiles about earlier participants contacting Congress.)

How many of those 100 participants do you think would choose to **email Congress about climate policy** during our survey?

(As a benchmark, you guessed that 58 out of 100 would email Congress if they belonged to or leaned towards the party and didn't see that an earlier participant emailed Congress.)

of Participants



Slide 8.

Future participants in this demographic group will also be **randomly paired** with earlier study participants from **your demographic group**.

If their paired earlier participant **emailed Congress** when they took our survey, we'll **show the future participant** the following profile of this earlier match:

I emailed Congress to push for climate action!	
Please consider joining me!	
	Leans: Democrat
	Says climate change is: Human-caused
	State: Lives in Florida
	
Name: (Filled in with pair's name)	
Age: 64-79	
Gender: Man	
College: 4-year degree or more	

(Again, we filled in your avatar here, but the profiles would be of other participants who share all of your demographics. They would only differ from you in name and avatar.)

Figure D7: Explanation of the belief elicitation, cont.

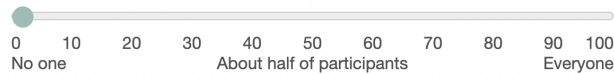
Slide 9.

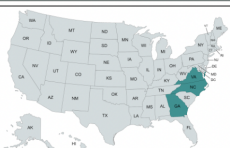
I emailed Congress to push for climate action!	
Please consider joining me!	
	Leans: Democrat
	Says climate change is: Human-caused
	State: Lives in Florida
	
Name: (Filled in with pair's name)	
Age: 64-79	
Gender: Man	
College: 4-year degree or more	

If 100 people from this group of future participants (see profile below) **saw one of these profiles before** they decided to email Congress or not, **how many** do you think would choose to **email Congress** during our survey?

(Remember that you guessed that 68 out of 100 future participants in this group who **didn't see** this extra profile would email Congress.)

of Participants



	Leans: Democrat
	Says climate change is: Human-caused
	State: Lives in Virginia, North Carolina, or Georgia
	
Age: 50-64	
Gender: Woman	
College: No 4-year degree	

Incentives description:

You can **win money** by making **accurate guesses** for how many participants will contact Congress.

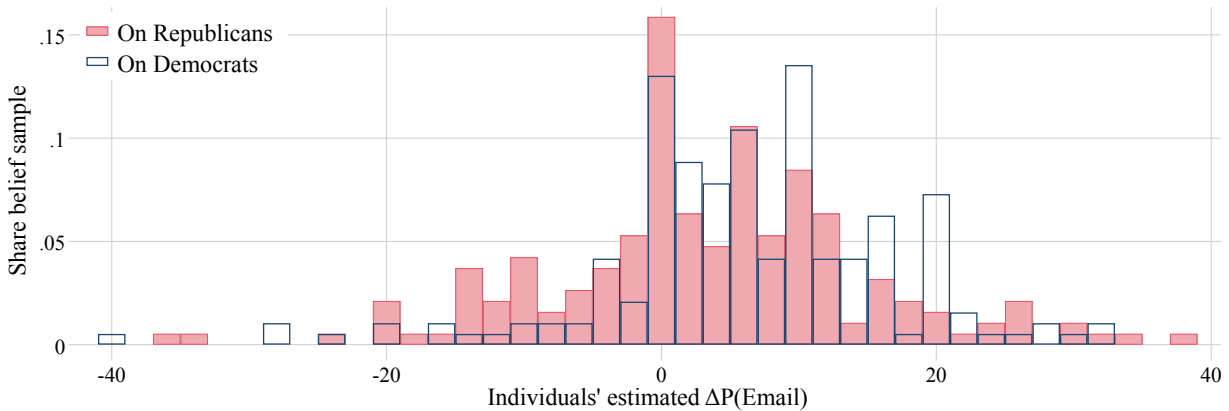
Once we finish collecting our survey data, we will calculate the true answer for each of the predictions we ask you to make.

Then, we will randomly choose 20 participants and randomly select one of the predictions they made. If their guess on that prediction is **within 10** of the correct answer, we'll email them a **gift card for \$15** to a store of their choice.

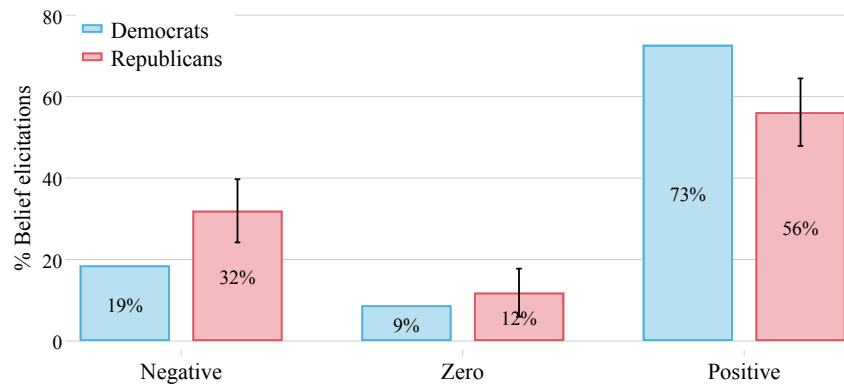
Note: Figures D5 through D7 show screenshots of the survey flow in which we elicit participants' beliefs about the impacts of invitations on whether Wave-2 participants email Congress. Participants are randomized into this survey sequence after seeing the upcoming opportunity to email Congress; the survey to this point is identical to the main Wave-1 survey. They then complete a first round of the email-valuation experiment described in Section 6.3.2 and Appendix D.4; this experiment includes the 10 choices referenced in slide 1 here. After completing the belief elicitation, participants choose whether to email Congress or not and then conclude the survey. Half of participants are randomly assigned to make incentivized choices, with the description of incentives shown after slide 1.

Figure D8: Other views on $\Delta P(\text{Email})$ beliefs by target politics

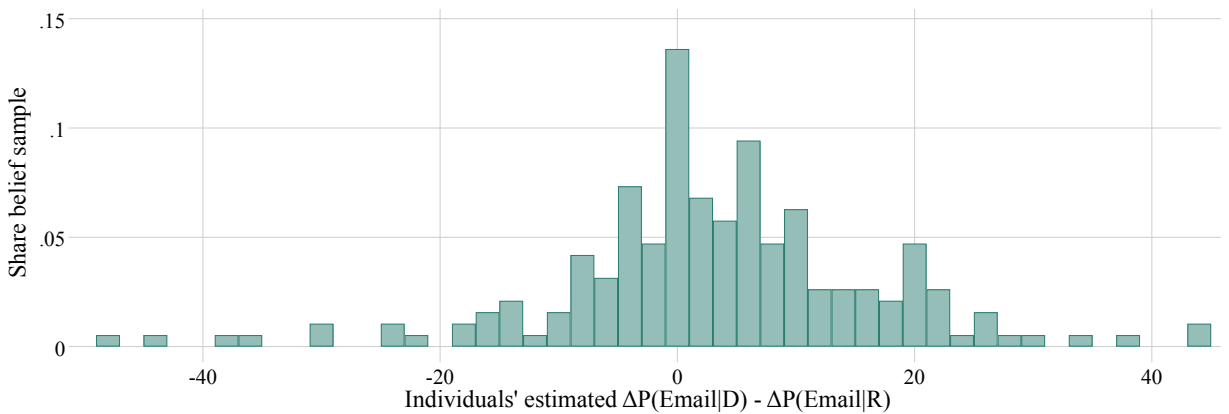
Panel A. Distribution of $\Delta P(\text{Email})$ influence beliefs by political party



Panel B. Partisan gaps in whether individuals estimate that $\Delta P(\text{Email})$ is positive, negative, or zero

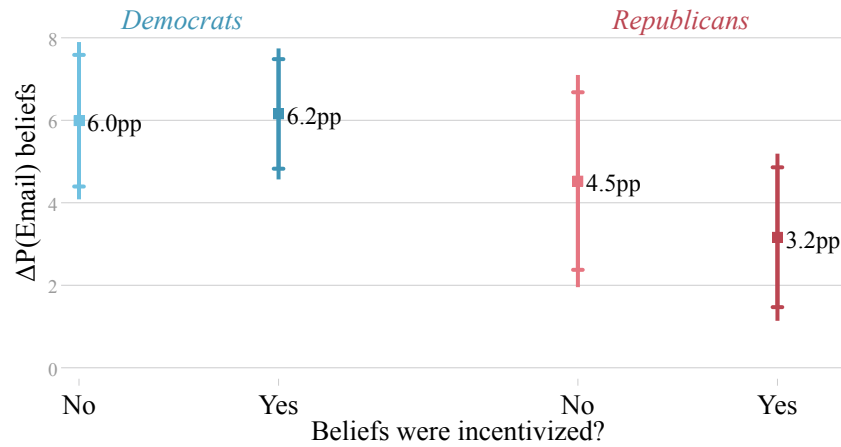


Panel C. Distribution of individuals' beliefs for $\Delta P(\text{Email}|D) - \Delta P(\text{Email}|R)$



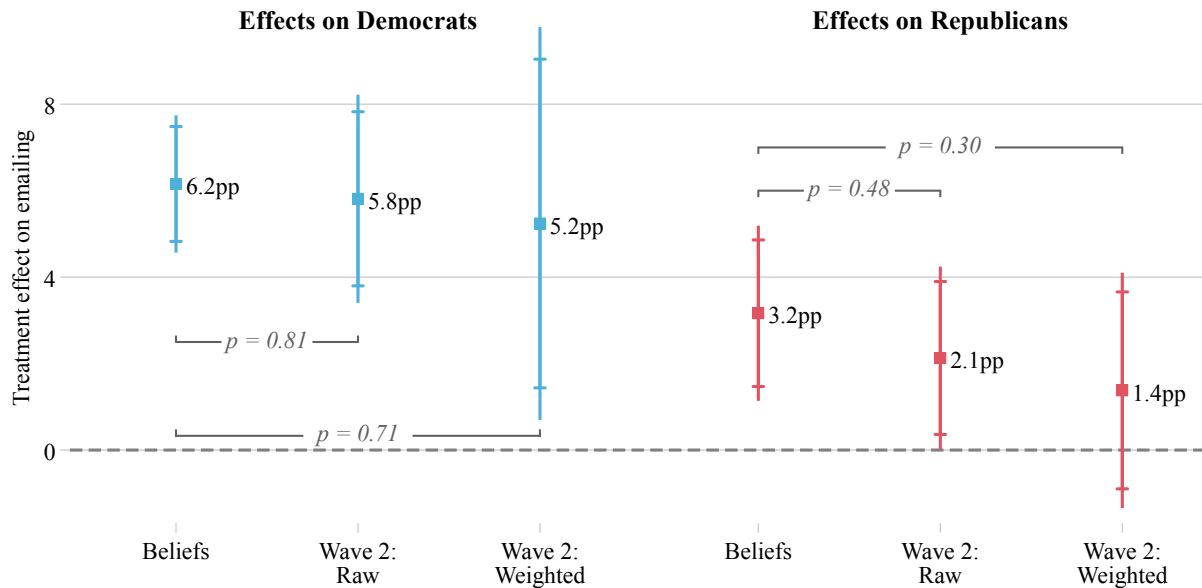
Note: Panel A plots the distribution of participants' individual estimates of $\Delta P(\text{Email})$, calculated as how many more of 100 members of a particular Wave-2 demographic group they think would email Congress if they saw an invitation from a member of the belief participant's own demographic group than if they did not. The sample is restricted to incentivized belief participants, each of whom estimates $\Delta P(\text{Email})$ for Republican and Democratic members of the same Wave-2 demographic group. We exclude 7 reports above 40pp or below -40pp, leaving $N = 381$ reports across 193 participants. Panel B plots the share of elicitation in which Democrats estimate that $\Delta P(\text{Email})$ is negative, zero, or positive for Wave-2 Democrats and Republicans. We regress dummies for each category on an indicator that the Wave-2 targets are Republicans and present standard errors clustered by belief participant. Panel C plots the distribution of $\Delta P(\text{Email}|D) - \Delta P(\text{Email}|R)$, calculated by individual as in Panel A. We exclude 3 participants for whom we estimate a value above 50pp or below -50pp, leaving $N = 191$ participants.

Figure D9: Comparing estimated $\Delta P(\text{Email})$ with and without accuracy incentives



Note: This figure compares estimates for participants' $\Delta P(\text{Email})$ beliefs with and without accuracy incentives. We estimate Equation 7 separately for beliefs about Wave-2 Democrats and Republicans and by whether belief participants had accuracy incentives or not (see footnote 36). 194 participants have accuracy incentives, while 203 do not. Regressions are estimated on four elicitations per participant: for Wave-2 Democrats and Republicans in their paired demographic group who did or did not see an invitation. The capped and uncapped lines denote 90% and 95% confidence intervals, respectively, with standard errors clustered by belief-sample participant.

Figure D10: Comparing estimated $\Delta P(\text{Email})$ to raw and weighted Wave-2 estimates



Note: This figure compares estimates for participants' $\Delta P(\text{Email})$ beliefs to estimated invitation effects in Wave 2 when the Wave-2 sample is unweighted versus when we weight it to match the demographics of invitation recipients about whom the belief sample makes predictions. The belief-sample estimates and raw Wave-2 estimates match those shown in Figure 5. In the weighted Wave-2 estimates here, we weight the Wave-2 samples of Democrats and Republicans so that within each party group they match the belief-sample targets on gender, 15-year age groups, state-politics categories (red, blue, purple), and educational attainment. The capped and uncapped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively, and the p-values test whether we can reject equality between each Wave-2 estimate and the belief sample's corresponding estimate for the impact of invitations on whether recipients email Congress.

Figure D11: Explanation of the option to hide Target-choice (TC) participants' political leanings

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

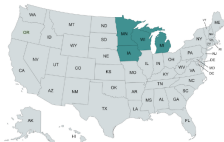
First, all of the sample profiles we've shown you so far have included your own political leanings, like this one:

I emailed Congress to push for climate action!

Please consider joining me!



College: 4-year degree or more
Says climate change is: Human-caused
State: Lives in Michigan, Wisconsin, Iowa, or Minnesota



Name: Daniel
Age: 50-64
Gender: Man
Leans: Democrat

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:

I emailed Congress to push for climate action!

Please consider joining me!



College: 4-year degree or more
Says climate change is: Human-caused
State: Lives in Michigan, Wisconsin, Iowa, or Minnesota



Name: Daniel
Age: 50-64
Gender: Man

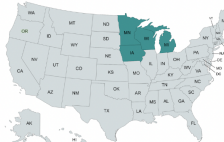
Note: This figure shows the explanation to participants in the round-two TC experiment who were randomly given the option to hide their politics from profiles they pass on to possible matches. Participants see this explanation soon before answering a set of comprehension questions, with no other changes in the explanation of the experimental set-up. Participants would see their own demographic profile here.

Figure D12: Making binary choices with the option to hide politics

Here is possible match #1. Which option should we carry out if you are randomly paired with them?



Leans: Republican
College: No 4-year degree
Says climate change is: Human-caused
State: Lives in Michigan, Wisconsin, Iowa, or Minnesota



Name: Jennifer
Action status: Already decided
Age: 50-64
Gender: Woman

Show basic demographic profile and buy \$4 in carbon offsets

Show extended profile saying you contacted Congress

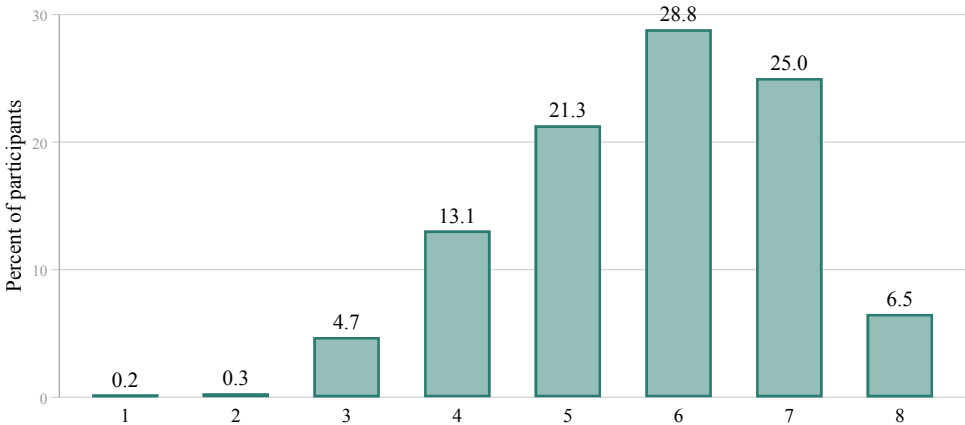
Should the profile we pass on include or remove your political leanings?

Remove political leanings

Include political leanings

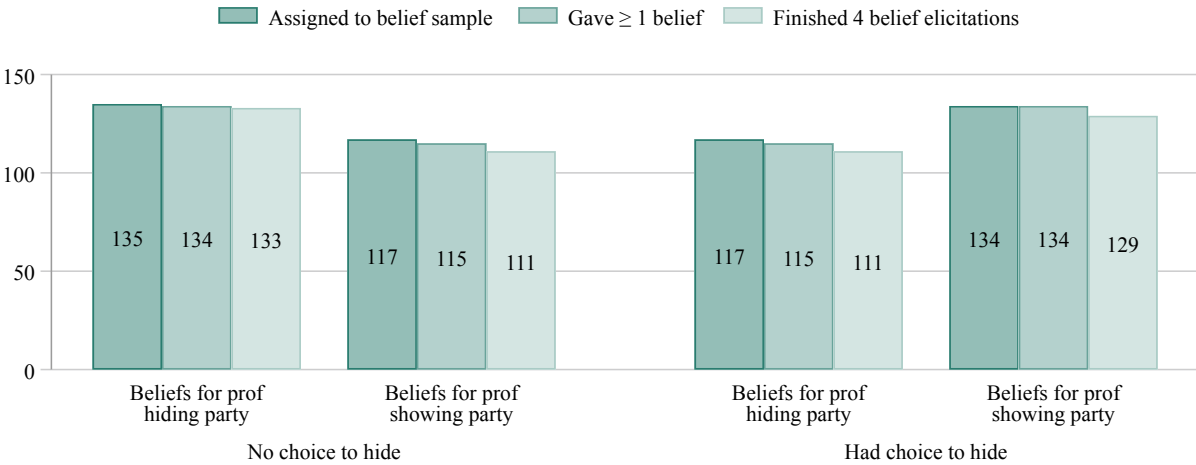
Note: This figure shows a sample of one of the 20 binary choices for possible matches made by TC participants with the option to hide their politics. They choose whether to pass on an extended or basic demographic profile on the same page on which they choose whether or not to show that they lean towards the Democratic party.

Figure D13: Distribution of accuracy on round-two Target-choice (TC) comprehension questions



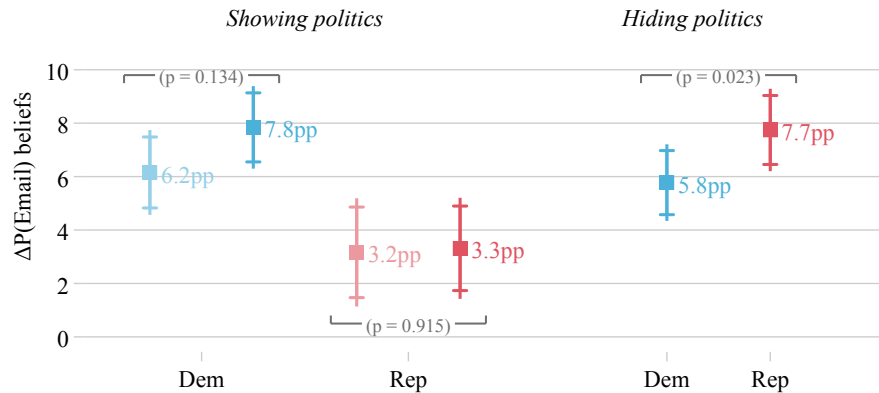
Note: This figure plots the percent of participants who correctly answered a total of 0 through 8 main comprehension questions correctly in the round-two TC experiment. The figure restricts to those who finished all 20 binary choices, who compose our sample for analysis. These 8 questions match those in the main TC experiment, where Appendix Figure C4 shows the distribution of correct answers. Note that those randomized to have the choice to hide their political leanings were asked an additional comprehension question asking which profile trait they had the option to hide, and 98% answer this correctly. See the Online Supplement for details on the comprehension questions asked.

Figure D14: Attrition in belief elicitation among round-two TC participants



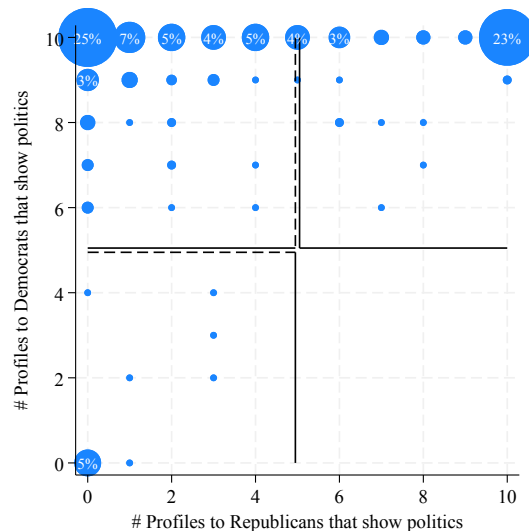
Note: This figure shows sample retention during belief elicitations for the impacts of invitations that either show or hide the sender's politics. Of the 995 participants who complete all 20 binary choices in the round-two TC experiment, 993 remain before the profile-impact belief elicitations. We randomize half of these to complete the belief elicitations for the impacts of invitations that hide or show that the sender is a Democrat. This figure shows retention of these participants during the belief elicitation, separately by two dimensions: (1) whether participants did or did not themselves have the option to hide their politics from profiles they passed on to TC matches; and (2) whether participants were randomized to guess the impacts of invitations that hide or show the sender's politics. We cannot reject equal retention across these 4 random groups.

Figure D15: Replicating $\Delta P(\textit{Email})$ beliefs for invitations that hide or show politics



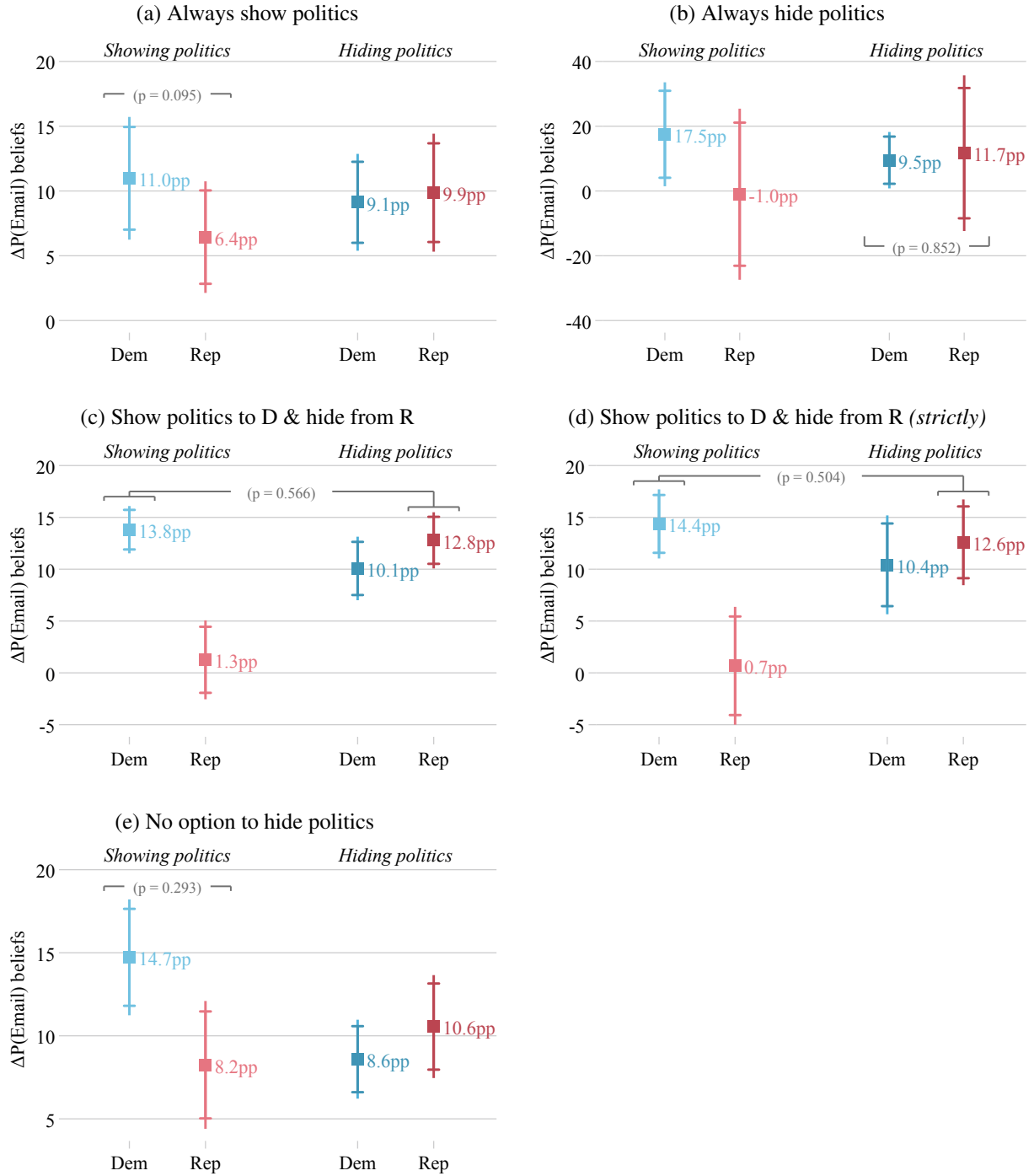
Note: This figure plots $\Delta P(Email)$ influence beliefs elicited in a replication sample recruited in June 2025. We recruited this sample to elicit incentivized beliefs for the impacts of invitations that hide politics, since the beliefs elicited in the round-two TC experiment were not incentivized (Section 6.2.2). We recruited this sample according to the same demographic and belief-based screeners as for Wave-1 Democrats. We asked all participants to guess the share of Democrats and Republicans in a particular Wave-2 demographic cell who would email Congress if they did or did not see an invitation from a Wave-1 participant of their own demographic group. This replicates the design of Section 6.1.5, except that we also elicited beliefs for the impacts of invitations that hid the sender's politics. Of 386 participants who started the belief elicitation, $N = 298$ completed it and are included in this analysis. The capped and uncapped lines denote 90% and 95% heteroskedasticity-robust confidence intervals. The darker coefficients and confidence intervals here show this replication sample's $\Delta P(Email)$ estimates. The lighter coefficients show the average $\Delta P(Email)$ beliefs estimated from the main belief sample in Section 6.1.5, and the two p-values at the left test the null hypothesis of equality between these coefficients and the parallel coefficients estimated in this replication sample; we successfully replicate the results in Section 6.1.5. The p-value at right tests the null of equal beliefs for the impacts of invitations that hide the sender's politics on Democratic and Republican recipients.

Figure D16: Categorizing participants as hiding or showing politics



Note: This figure categorizes the strategies followed by round-two TC participants with the option to hide their political leanings from profiles passed on to possible matches. The area of each circle is proportional to the percentage of participants who chose to show their politics to a given number of Republican matches and Democratic matches. We write the share inside each circle for points with shares of at least 3% of participants. The horizontal and vertical lines show the ranges of points that we grouped into three overall strategies: never showing one's politics (bottom left), always showing one's politics (top right), and showing one's politics to Democrats while hiding them from Republicans (top left). The solid and dashed lines show where a set included or excluded the boundary points, respectively.

Figure D17: $\Delta P(\text{Email})$ influence beliefs by strategy for hiding or showing politics



Note: This figure shows influence beliefs elicited among subsets of the round-two TC sample. Note that only half of the round-two TC sample is randomized to report these beliefs. These figures are parallel to that in Figure 6b. Subfigures (a) through (c) split the sample of those with the option to show or hide their politics into the three strategy categories shown in Appendix Figure D16, with sample sizes of $N = 85$, $N = 8$, and $N = 147$, respectively. Subfigure (d) then restricts to those who strictly show their politics to all 10 Democratic matches and hide them from all 10 Republicans (i.e. the circle showing 25% in Appendix Figure D16; $N = 63$). Finally, subfigure (e) plots influence beliefs among those without the option to show or hide their political leanings ($N = 244$). The p-values test for equality in the “relevant” influence beliefs for Democrats versus Republicans based on each group’s strategy of showing or hiding politics during their binary choices.

Figure D18: Explanation of the email-valuation 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.

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 preview of email form.)

Slide 4.

Next, will your choices **matter**?

Yes, they will.

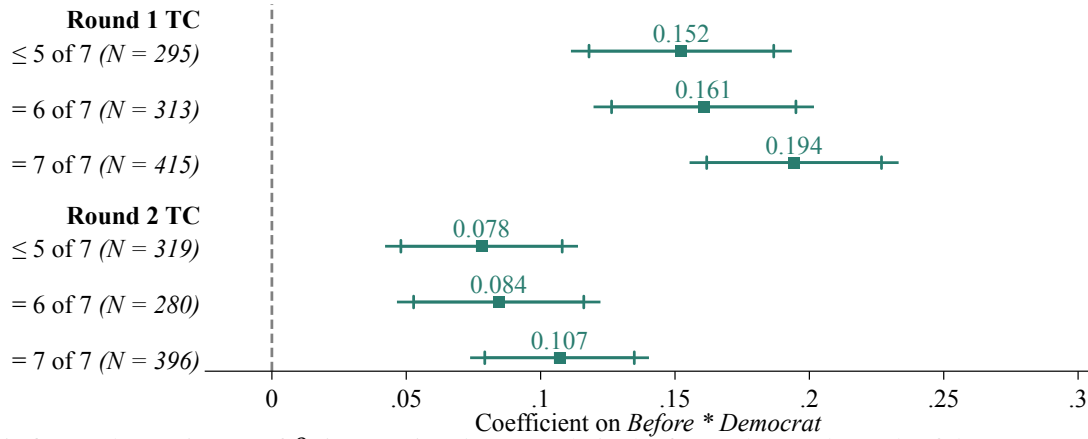
We will randomly choose 20 people and implement one of their choices. If one of your choices is selected, we'll do **whatever you picked** in that choice: We'll either donate \$10 to carbon offsets, or we'll arrange for a past participant like that to email Congress.

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.

Note: This figure shows how we explain the email-valuation experiment set-up to participants.

Figure D19: Heterogeneity in partisan outreach gaps by affective polarization



Note: This figure plots estimates of β_2 in Equation 5, separately in the first and second rounds of the TC experiment, for participants who place themselves at a 5 or below, 6, or 7 on 7-point scale of how much they would prefer being friends with another Democrat versus with a Republican, our best measure of participants' affective polarization. (We did not elicit thermometer-based polarization measures in the TC sample.) We show the distribution of this friendship variable in the full Wave-1 sample in Panel B of Appendix Figure A5. For power, we pool participants across treatment variations within each TC round: we pool those with and without information on matches' climate beliefs in round 1 and pool those with and without the option to hide their politics in round 2. We show the number of participants in our analysis sample who place themselves at each point on the friendship-preference scale in parentheses by each estimate. The capped and uncapped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively, with standard errors clustered by participant.

Table D1: Wave-2 attrition and experimenter demand

	(1)	(2)	(3)
	Observe if email Congress	Observe demand effect	Experimenter demand (std)
Treatment	0.005 (0.005)	0.003 (0.006)	0.206*** (0.022)
Control means	0.915	0.896	-0.000
Sample size	8685	8685	7806

Note: This table tests for differential attrition by treatment status in Wave 2. Column 1 tests for differential attrition in whether we observe if participants choose to email Congress, our primary outcome variable. Note that we define our main experimental sample here as those for whom we observe choices to email Congress or not. Column 2 tests for differential attrition in participants' reports of how strongly we, the researchers, wanted them to email Congress. We present heteroskedasticity-robust standard errors in parentheses and indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

Table D2: Wave-2 sample summary and balance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Full means:			Democrats:			Republicans:		
	Dem	Rep	<i>p-value</i>	Control	Δ Treat	<i>p-value</i>	Control	Δ Treat	<i>p-value</i>
From Qualtrics sample	0.171	0.945	(0.000)	0.177	-0.011	(0.317)	0.944	0.001	(0.901)
Woman	0.565	0.521	(0.000)	0.560	0.011	(0.432)	0.522	-0.002	(0.912)
Hispanic	0.055	0.101	(0.000)	0.056	-0.003	(0.617)	0.102	-0.002	(0.856)
Has $\geq$ 4-year college degree	0.758	0.361	(0.000)	0.761	-0.006	(0.617)	0.356	0.010	(0.579)
Age ranges:									
20-39	0.217	0.427	(0.000)	0.215	0.003	(0.803)	0.429	-0.004	(0.824)
40-59	0.326	0.318	(0.467)	0.340	-0.027**	(0.038)	0.319	-0.003	(0.860)
60-79	0.457	0.255	(0.000)	0.445	0.024*	(0.087)	0.251	0.006	(0.708)
Income bins (USD):									
Less than 50,000	0.232	0.397	(0.000)	0.228	0.009	(0.453)	0.399	-0.004	(0.824)
50,000-99,999	0.324	0.378	(0.000)	0.329	-0.010	(0.442)	0.388	-0.019	(0.291)
100,000-149,999	0.222	0.136	(0.000)	0.219	0.006	(0.617)	0.131	0.009	(0.489)
150,000-199,999	0.108	0.051	(0.000)	0.108	0.001	(0.912)	0.040	0.022***	(0.006)
200,000 or more	0.114	0.038	(0.000)	0.117	-0.006	(0.505)	0.042	-0.007	(0.317)
Residence by state marginality:									
Red state	0.267	0.413	(0.000)	0.271	-0.008	(0.505)	0.409	0.009	(0.617)
Blue state	0.419	0.257	(0.000)	0.415	0.008	(0.568)	0.260	-0.006	(0.708)
Purple state	0.314	0.329	(0.173)	0.314	-0.000	(1.000)	0.331	-0.003	(0.860)
Climate beliefs:									
Climate worry (1-7)	6.300	4.953	(0.000)	6.318	-0.036	(0.166)	4.928	0.049	(0.373)
Desire for climate action (1-7)	6.630	5.216	(0.000)	6.635	-0.010	(0.617)	5.155	0.123**	(0.031)
Perceived local impacts (1-7)	5.483	4.836	(0.000)	5.466	0.035	(0.243)	4.873	-0.075	(0.157)
Political engagement and beliefs:									
Member of resp. party	0.319	0.742	(0.000)	0.321	-0.005	(0.701)	0.734	0.015	(0.349)
Political-engage. index (std)	1.195	-2.033	(0.000)	1.199	-0.008	(0.943)	-2.073	0.080	(0.519)
Prev. contacted reps	0.268	0.219	(0.000)	0.265	0.006	(0.617)	0.212	0.014	(0.351)
Prev. donated	0.686	0.241	(0.000)	0.683	0.006	(0.644)	0.241	0.000	(1.000)
Prev. canvassed	0.064	0.040	(0.000)	0.063	0.003	(0.668)	0.038	0.004	(0.568)
Prev. signed petition	0.746	0.390	(0.000)	0.748	-0.003	(0.803)	0.384	0.011	(0.541)
Prev. phonebanked	0.078	0.045	(0.000)	0.079	-0.001	(0.901)	0.049	-0.009	(0.261)
Political-efficacy index (std)	-0.109	0.186	(0.000)	-0.120	0.021	(0.162)	0.193	-0.014	(0.505)
Prefer friend of own party (1-7)	5.663	4.895	(0.000)	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 Democrats and Republicans, respectively, on a range of baseline traits, and column 3 presents *p*-values for tests of equality between these means. The note to Table 1 gives more details on these traits. Columns 4 and 7 present control means for each trait, columns 5 and 8 present the difference in means between the treatment and control groups on each baseline trait, and columns 6 and 9 present heteroskedasticity-robust *p*-values testing the null of equality across treatment and control on each trait, separately among Democrats and Republicans. In columns 5 and 8, we indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

Table D3: Comparing Wave-2 participants recruited via social media vs. Qualtrics Panels

	(1)	(2)	(3)	(4)	(5)	(6)
	Democrats			Republicans		
	Social	Qualtrics	<i>p-value</i>	Social	Qualtrics	<i>p-value</i>
Woman	0.581	0.492	(0.000)	0.393	0.528	(0.001)
Hispanic	0.043	0.111	(0.000)	0.055	0.104	(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	

Note: This table compares baseline trait means among Wave-2 Democrats and Republicans recruited via social media (either directly or after being redirected from Wave-1 social-media recruitment) or from Qualtrics, restricting to the full experimental sample for whom we observe explicit choices to email Congress. All traits are defined as in Table 1. Columns 3 and 6 give heteroskedasticity-robust *p*-values for tests of equality in means between columns 1 and 2 and columns 4 and 5, respectively.

Table D4: Main Wave-2 results, split by source and party

	(1)	(2)	(3)	(4)	(5)	(6)
	Social-media sample			Qualtrics sample		
	<i>All</i>	<i>Dem</i>	<i>Rep</i>	<i>All</i>	<i>Dem</i>	<i>Rep</i>
<i>Panel A: Start the process of emailing Congress</i>						
Treatment	0.068*** (0.015)	0.069*** (0.015)	0.208* (0.107)	0.046*** (0.014)	0.061* (0.032)	0.041** (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)? <i>p-value</i> = 0.818						
Rep coefficient equal by source (3) = (6)? <i>p-value</i> = 0.035						
<i>Panel B: Have a record of emailing Congress</i>						
Treatment	0.061*** (0.014)	0.063*** (0.014)	0.085 (0.074)	0.025** (0.010)	0.044* (0.026)	0.020* (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)? <i>p-value</i> = 0.490						
Rep coefficient equal by source (3) = (6)? <i>p-value</i> = 0.234						

Note: This table tests for differential treatment effects of Wave-1 invitations among participants recruited from social media (columns 1-3) or from Qualtrics (columns 4-6). The regressions use the same control variables as in Figure 5. 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 in Panels A and B give p-values for tests of equality in treatment effects among those recruited from each source, separately for Wave-2 Democrats and Republicans.

Table D5: Wave-2 participants' belief about the study purpose

	Control (1)	Treatment (2)	Δ Mean (3)	<i>p-value</i> (4)
Measuring rates of climate action/belief and correlates	0.532	0.440	-0.104***	(0.000)
Trying to mobilize political action	0.245	0.300	0.053**	(0.042)
Something vague about climate	0.102	0.080	-0.013	(0.417)
Something vague about social interactions	0.013	0.047	0.037***	(0.001)
Testing spillovers from seeing others act	0.023	0.072	0.048***	(0.000)
Other	0.072	0.062	-0.005	(0.721)
Participant is unsure	0.077	0.058	-0.019	(0.206)
Sample size	600	600		

Note: This table summarizes and estimates treatment differences in Wave-2 participants' guesses for the study purpose, elicited soon after participants choose whether to email Congress. A treatment-blind academic hired by the authors coded free-text responses for 1200 participants, split evenly between the Control and Invitation groups, into the categories shown in the table. The Online Supplement details the coding scheme. Columns 1 and 2 show the share of each treatment group that reported a reason in each category, and column 3 presents the difference in these means across treatment groups, where we indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively. Column 4 presents heteroskedasticity-robust p-values for these differences in means.

Table D6: Round-two Target-choice sample balance

	(1)	(2)	(3)
	Standard	Choose politics	
	Mean	Δ Mean	<i>p-value</i>
Woman	0.675	0.004	(0.894)
Hispanic	0.022	0.007	(0.484)
Has $\geq$ 4-year college degree	0.818	0.056**	(0.015)
Age ranges:			
20-39	0.053	0.004	(0.790)
40-59	0.267	-0.006	(0.830)
60-79	0.679	0.002	(0.947)
Income bins (USD):			
Less than 50,000	0.196	-0.049**	(0.041)
50,000-99,999	0.350	-0.002	(0.947)
100,000-149,999	0.220	0.029	(0.283)
150,000-199,999	0.119	-0.005	(0.803)
200,000 or more	0.115	0.026	(0.216)
State marginality:			
Red state	0.263	-0.041	(0.129)
Blue state	0.446	0.018	(0.574)
Purple state	0.291	0.023	(0.428)
Climate beliefs:			
Climate worry (1-7)	6.600	0.010	(0.807)
Desire for climate action (1-7)	6.812	-0.006	(0.830)
Perceived local impacts (1-7)	5.586	0.049	(0.444)
Political engage. and beliefs:			
Political-engage. index (std)	0.195	-0.036	(0.549)
Prev. contacted reps	0.822	-0.032	(0.201)
Prev. donated	0.830	0.023	(0.318)
Prev. canvassed	0.095	0.023	(0.250)
Prev. signed petition	0.871	-0.033	(0.134)
Prev. phonebanked	0.141	0.004	(0.856)
Political-efficacy index (std)	-0.080	0.048	(0.439)
Degree prefer Dem friends (1-7)	6.016	-0.022	(0.723)
Sample size	505	490	

Note: This table summarizes and tests for balance in the round-two TC sample. We define the sample here as those who email Congress, elect to take the TC survey, and complete all 20 binary profile choices. Column 1 presents means among participants randomly assigned to not have the option to hide their politics from possible matches, column 2 presents gaps in baseline traits between this group and those randomly assigned to have this option, and column 3 gives heteroskedasticity-robust *p*-values testing the null of equality between these groups. Column 2 indicates statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively. The note to Table 1 gives more details on these traits, and Appendix Table A1 compares this sample to the round-1 TC sample.

Table D7: Choices of emails with certainty and email-impact beliefs by state politics

	(1)	(2)	(3)	(4)	(5)	(6)
	Chose email over offsets			Impact on policy support		
	<i>State groups</i>			<i>State groups</i>		
	Red	Blue	Purple	Red	Blue	Purple
Republican	0.000 (0.020)	0.022 (0.023)	0.003 (0.024)	0.074 (0.191)	-0.143 (0.130)	0.036 (0.139)
Woman	-0.004 (0.017)	0.014 (0.019)	-0.003 (0.036)	-0.346 (0.228)	0.078 (0.193)	-0.282 (0.283)
Has 4-year college	-0.037 (0.029)	0.034 (0.026)	0.010 (0.042)	0.000 (0.000)	0.128 (0.211)	-0.354 (0.270)
Age ranges:						
35-49	0.047 (0.029)	0.039 (0.035)	-0.063* (0.034)	0.000 (0.000)	-0.156 (0.282)	0.080 (0.238)
50-64	0.003 (0.023)	-0.007 (0.031)	-0.016 (0.036)	0.251 (0.228)	-0.399 (0.251)	0.000 (0.000)
65-79	-0.016 (0.025)	0.027 (0.034)	-0.006 (0.034)	0.000 (0.000)	-0.317 (0.222)	-0.028 (0.199)
Sample mean	0.644	0.538	0.689	-0.156	0.073	-0.006
N	3320	2693	2023	110	251	196

Note: Columns 1 to 3 of this table estimate Equation 8 separately for possible demographic profiles that would send letters in red, blue, or purple states, as defined in Appendix C.2. Columns 4 through 6 then test for differential beliefs in the impacts of emails on lawmakers' support for a hypothetical climate bill by email-writer traits, separately by state politics. We present heteroskedasticity-robust standard errors in parentheses and indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

D.2 Appendix to the Wave-2 experiment

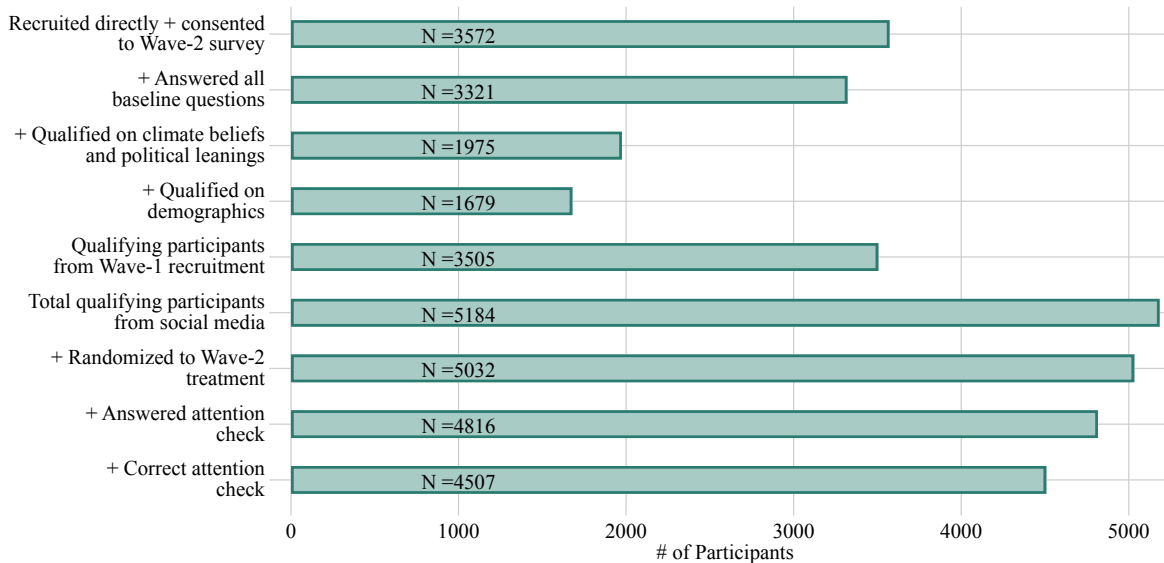
D.2.1 Wave-2 recruitment

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

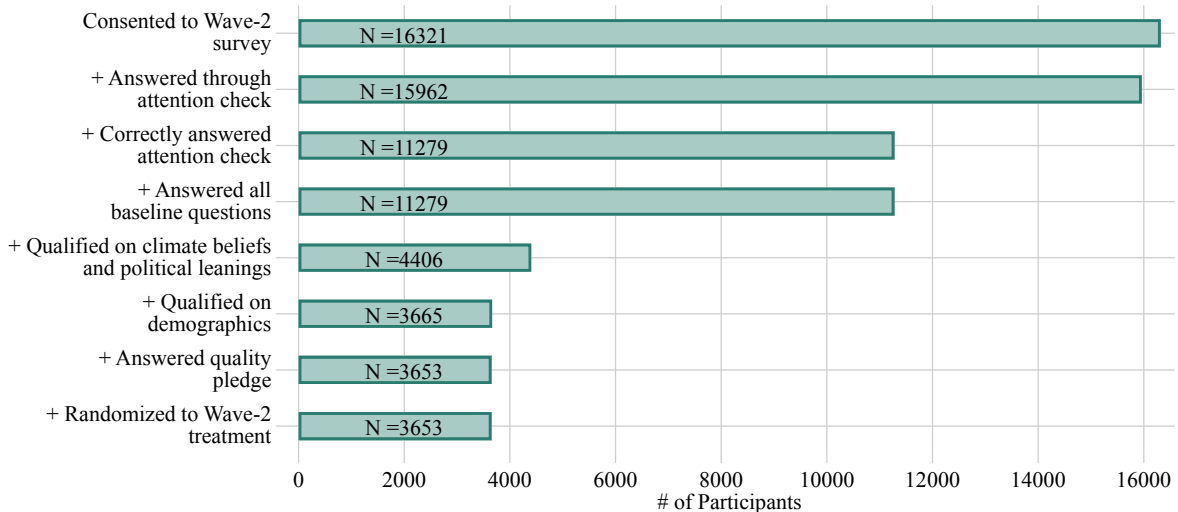
Social-media recruitment: We recruited Wave-2 participants via social media in two ways (Panel A of Appendix Figure D20). First, we recruited some participants from social media directly to the Wave-2 Qualtrics survey. In total, 3,572 unique participants from social media consented directly to the Wave-2 survey and provided 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 stated that they believe that climate change is mostly human-caused and lean towards either the Republican or Democratic party. We then imposed the feasibility-based screening criteria (Section 2.1), 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 left 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.

Figure D20: Wave-2 recruitment

Panel A. Recruitment from social media



Panel B. Recruitment from Qualtrics



Other participants from social media were redirected 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.

The full sample of N = 5,184 qualifying participants recruited from social media (either via the Wave-1 experiment or directly to Wave 2) then build an avatar, and the remaining 5,032 participants are randomized to a Wave-2 treatment group. This sample is highly skewed towards Democrats: only 211 participants recruited via social media and randomized to a Wave-2 treatment lean Republican. 4,816 of randomized participants answer an attention-check question and 4,507 do so correctly. These participants then see the

upcoming opportunity to email Congress, and we show the Treatment group their paired invitation to email Congress, the first divergence between the treatment groups.⁵⁴ Note that we eliminate participants who fail the attention check, effectively counting them as participants who attrit before we observe email choices. There is no differential attrition by treatment group (Appendix Table D1).

Qualtrics recruitment: While we recruited most Wave-2 Democrats from social media, we recruited nearly all Wave-2 Republicans 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 D20).

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 3,653 answer it. This remaining sample then answers additional baseline survey questions, build their basic avatar, and are randomized into Wave-2 treatment arms ($N = 3,653$).

To make up for the lack of Republicans in our Wave-2 sample recruited from social media, we intentionally focused Qualtrics recruitment on Republicans: 2,791 Qualtrics participants randomized to a treatment arm lean towards the Republican party, while 862 lean towards the Democratic party.

D.2.2 Wave-2 robustness

Appendix Figure D21 shows the robustness of our Wave-2 results to a range of perturbations.

Sample definitions: First, our main Wave-2 results are highly robust to changes in the sample definition. They are robust to restricting precisely to our pre-registered sample sizes,⁵⁵ as well as to expanding the Wave-2 experimental sample by assuming that all participants who leave the survey after seeing a preview of the upcoming email opportunity would not have emailed Congress had they continued. We make this assumption in our main Wave-1 analysis (Section 4.3).

⁵⁴Note that we randomize Wave-2 participants earlier because we must determine treatment status before first showing participants their Wave-1 match's demographic profile. If Wave-2 participants are in the Treatment group, in particular, we must initially pair them with a Wave-1 participant who emailed Congress and knew that an invitation would be passed on from them.

⁵⁵While we pre-registered a total Wave-2 sample size of about 4,250 Democrats, our recruitment yielded 5,200 Democrats who saw the email preview and 5,027 for whom we observe explicit choices to email Congress or not. Our results are robust to restricting to the first 4,250 Democrats either for whom we observe direct email choices or who saw the email preview. While we pre-registered that we would recruit 3,250 Republicans, our budget ultimately allowed us to recruit only 2,960.

Study comprehension: Our results are robust to restricting to participants who correctly answer whether they know if their paired Wave-1 match emailed Congress when they took our survey. Appendix Figure D2 summarizes Wave-2 comprehension.

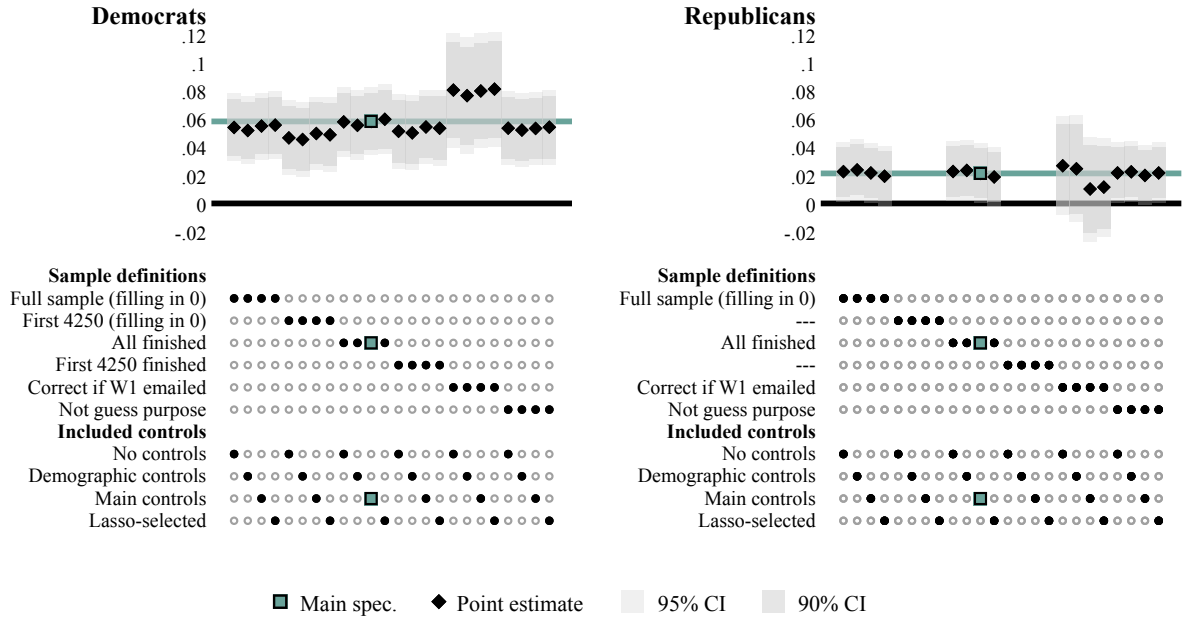
Control variables: Our Wave-2 estimates are also robust to the following control specifications: no controls, only demographic controls, the full set of controls for demographics, baseline beliefs, and baseline political engagement in our main regressions, and a set of controls selected by double-post Lasso from participants' demographics, baseline beliefs, and dummy variables for particular forms of past political engagement, separately by political party (Chernozhukov et al., 2018). See Appendix B.2 for details on these controls.

Experimenter demand effects: Our Wave-2 estimates are robust to accounting for experimenter demand effects. Strong demand effects are inherent to 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 (column 3, Appendix Table D1). Any demand effects at play only 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, research-induced demand effects can only be at play among participants who recognize our research question or hypothesis. About 2% and 7% of the control and treatment groups guess that our experiment aims to test the spillovers from invitations (Appendix Table D5), and our results are fully robust to excluding these participants. Next, our results are robust to assuming that recognizing the study purpose either increases or decreases our main outcomes by 0.2sd (Appendix Figure D22), an estimate of how much explicitly telling participants a research hypothesis shifts their real-stakes behavior (de Quidt et al., 2018; Mummolo and Peterson, 2019).

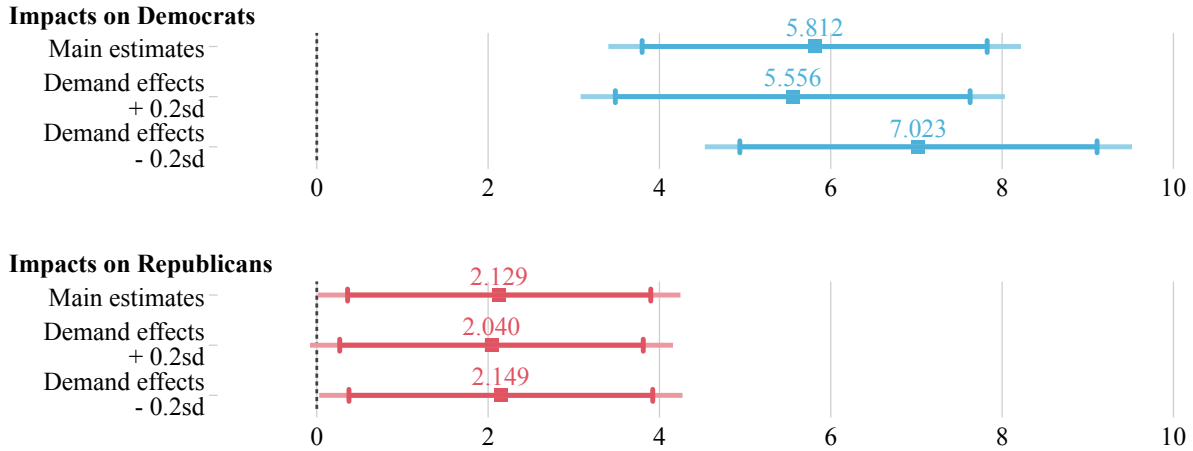
Robustness of the partisan gap: Finally, our results for the partisan gap in invitations' effects are robust to key potential confounders that are correlated with party affiliation in our sample (Section 6.1.2). Our goal here is not to identify the differential impact of invitations on Republicans and Democrats who are otherwise identical; indeed, gaps in concern about climate change and education would be at play in real-world attempts to mobilize partisans for climate action. Rather, Appendix Figure D23 shows that the partisan gap we estimate is robust (though in some cases less precise) to controlling for the interaction of treatment status with key traits associated with our differential recruitment strategies for Democrats and Republicans: whether a participant is recruited from Qualtrics, political engagement, income, and educational attainment.

Figure D21: Wave-2 specification charts



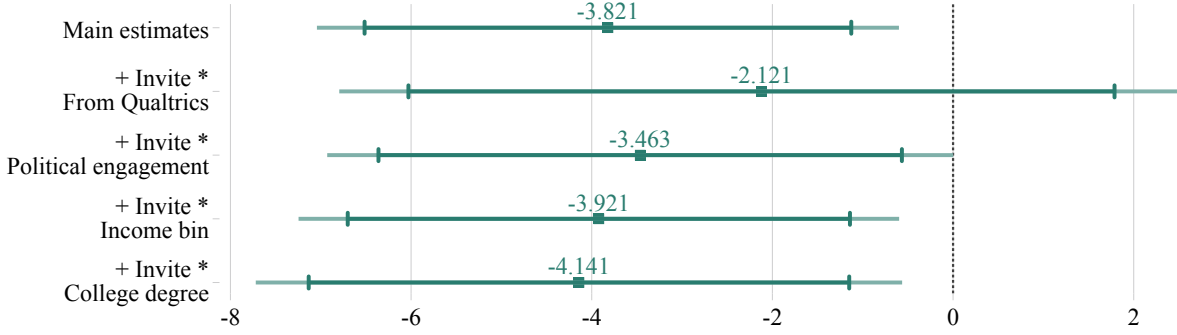
Note: This figure plots estimates for β in Equation 6 under a series of specification tests, separately for Wave-2 Democrats and Republicans. The outcomes is whether participants match to an email record, respectively. We estimate treatment effects under various sample restrictions and control specifications, as detailed in Appendix D.2.2. The teal squares indicate our main specification. Here, “filling in 0” refers to assuming that any participant who left the survey after seeing the upcoming opportunity to email Congress would not have emailed Congress had they continued. The “finished” samples refer to restricting to participants for whom we observe active choices to email Congress or not.

Figure D22: Robustness of main Wave-2 estimates to demand-effect adjustments



Note: This figure plots estimates for β in Equation 6 with bounds for the role of experimenter demand effects. Guided by de Quidt et al. (2018), we re-estimate our main results when we either add or subtract 0.2sd from our email outcome variable for any participant that guessed the study purpose. We calculate standard deviations separately in the full samples of Wave-2 Democrats and Republicans. The regressions are otherwise unchanged from our main Wave-2 specifications (Figure 5), which appear as the top coefficient in each panel here. Note that the coefficient labeled “Demand effects + 0.2sd” estimates results assuming that recognizing the study purpose increases participants’ likelihood of emailing Congress by 0.2sd, meaning that we *subtract* 0.2sd from these participants’ outcomes. The capped and uncapped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively.

Figure D23: Robustness of partisan gaps in the Wave-2 invitation effect



Note: This figure tests robustness of our estimates for the differential impacts of Wave-1 invitations on Democrats' versus Republicans' likelihood of emailing Congress (i.e. the partisan gap in estimates for β in Equation 6.) We estimate variants of the following regression: $Email_i = \alpha + \beta_1 Rep_i + \beta_2 Treatment_i + \beta_3 Treatment_i * Rep_i + \beta_4 Treatment_i * Trait_i + \Phi X_i + \varepsilon_i$, where Rep_i indicates that the Wave-2 participant is a Republican, $Treatment_i$ indicates that the Wave-2 participant received a Wave-1 invitation, and $Trait_i$ measures some other trait to which we test robustness. In particular, we iterate through including interactions of treatment with an interaction that the participant was recruited from Qualtrics, the political-engagement index, income bins, and an indicator that the participant has a 4-year college degree. The controls X_i are the same as in Equation 6. The capped and uncapped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively.

D.3 Appendix to the round-two Target-choice (TC) experiment

D.3.1 Round-two TC recruitment and completion

Sample recruitment: We recruit participants for the round-two TC experiment with advertising on Facebook and Instagram. In total, 4,492 participants qualified for the full set of demographic and belief-based restrictions for Democratic influencers and were randomized to be funneled to the round-two TC experiment. (Others were randomized into the motivating sample described in Section 2.2 or the email-valuation experiment described in Section 6.3.2). Of these, 1,612 said that they emailed Congress (36%) and were invited to complete the additional TC survey, and 1,420 (88%) started the survey.

Round-two TC attrition: Of the 1,420 participants who began the round-two TC experiment, 1,278 continued through being randomized to have the choice to hide their politics or not. 1,253 participants finished the description of the TC set-up and began answering comprehension questions. 1,016 participants made at least one binary choice, and 995 participants completed all 20 choices; as in the main TC experiment, we will restrict our analysis to this sample of participants. There is no differential completion between the groups with or without the option to hide their politics (column 3, Appendix Table C1).

D.3.2 Approximating effective partisan gaps in $\Delta P(Email)$ for those who can hide politics

In this section, we approximate the effective partisan gaps in TC participants' $\Delta P(Email)$ influence beliefs, among those with the option to hide their politics. To do so, we first categorize participants into three strategies for showing versus hiding politics. 53% of those with the option to hide politics fall cleanly into one of three categories (Appendix Figure D16): those who never show their politics to any potential match (5%), those who show their politics to all potential matches (23%), and those who show their politics to

all Democrats and to no Republicans (25%). We then categorize all respondents as falling in the closest category on the grid shown in Appendix Figure D16. 76% and 85% deviate by no more than 2 or 3, respectively, from their associated category in the number of times they showed politics to either Democrats or Republicans. We then re-estimate the $\Delta P(\text{Email})$ influence beliefs shown in Figure 6b separately in each group (Appendix Figure D17) and calculate the partisan gap in each group’s “effective” influence beliefs. For example, the effective partisan belief gap for those who typically show politics to Democrats but hide them from Republicans is: $\Delta P(\text{Email}|\text{D}, \text{show}) - \Delta P(\text{Email}|\text{R}, \text{hide}) \approx 1.0pp$.

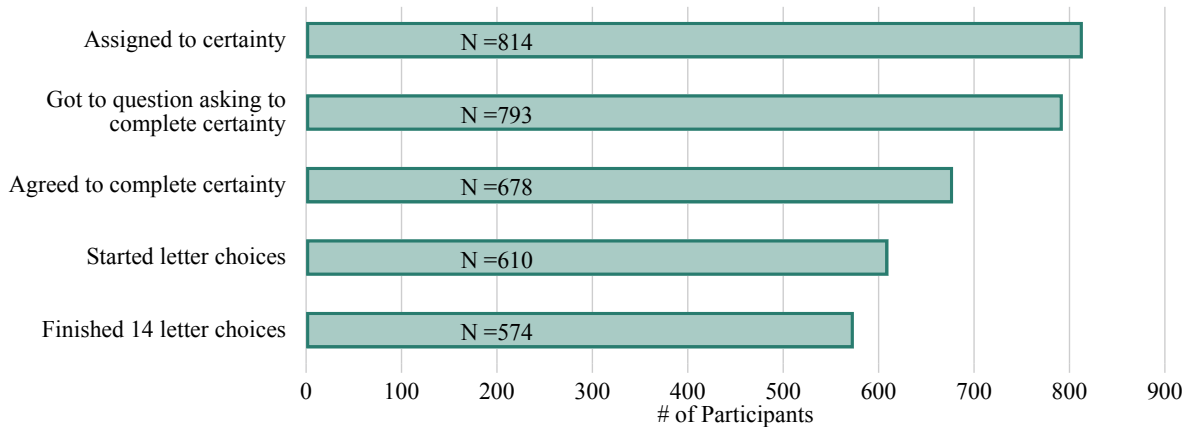
Accounting for the share of TC participants in each category and the standard errors of each estimate, we estimate that the average participant with the option to hide politics expected to have: $\Delta P(\text{Email}|\text{D}, \text{show}^*(\text{D})) - \Delta P(\text{Email}|\text{R}, \text{show}^*(\text{R})) \approx 2.1pp$, with a 95% confidence interval of (-0.9pp, 5.2pp). Here, $\text{show}^*(\text{R})$ and $\text{show}^*(\text{D})$ are participants’ chosen strategies for showing or hiding politics from Republicans and Democrats, respectively.

D.4 Appendix to the email-valuation experiment

D.4.1 Email-valuation recruitment

We recruit participants for the email-valuation experiment via ads on social media (e.g. Appendix Figure A1) after finishing the main Wave-1 recruitment. The sample is subject to the same demographic, political, and climate-belief screening as the main Wave-1 sample (footnote 6).

Figure D24: Recruitment to the email-valuation experiment



The survey matches the Wave-1 survey through showing participants a preview of the upcoming email opportunity. Then, we ask participants to choose whether to complete an additional 10-minute survey section, saying that it will ask them to choose between different ways that we as a research team could spend our money or time to have impact on climate change. Of 814 participants randomly assigned to be offered the email-valuation survey, 793 complete it through the question in which we invite them to take it and 678 agree to do so. Of these participants, 610 make at least one binary choice, and 574 complete all 14 binary choices and are included in our analysis.

D.4.2 Rounds 1 and 2 of the email-valuation experiment

Before our main email-valuation experiment, we ran an initial version of the experiment with a sample of 459 participants. We randomized these participants to the email-valuation sample from the main Wave-1 recruitment. After completing the email-valuation experiment, they went on to complete the belief elicitation described in Section 6.1.5. This first round followed almost entirely the same design as the main email-valuation experiment (Section 6.3.2), with several exceptions. First, first-round participants made choices over only 10 demographic profiles (instead of 14), and we randomized offset amounts from \$8 to \$16, instead of fixing them at \$10.

More importantly, we improved 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. Among these, we matched a set of 40 Republicans on all visible demographics to a sample of Democrats, then randomized the 80 profiles into 100 different sets of 10. While Democrats and Republicans in the possible demographic profiles are thus balanced on the other demographic traits shown in the profiles—age, gender, educational attainment, and state—they could have differed in names and avatars chosen. In the second round of the email-valuation experiment, in contrast, we fully randomize political party with respect to other demographic-group traits. We construct a series of profiles for demographic cells that are well-represented among our past study participants (and thus from which we could feasibly recruit someone to email Congress), and then randomize the profiles’ politics across participants.

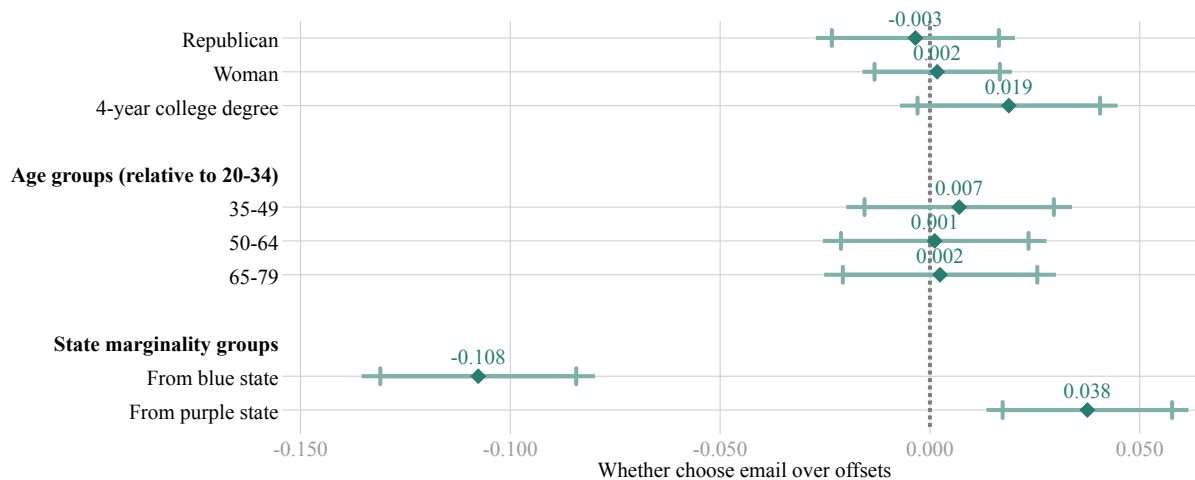
In another more substantive improvement, the second-round email-valuation experiment asked more precise questions to elicit participants’ beliefs about the effectiveness of emails sent to Congress by different groups. In the first round, we simply asked participants to answer the following: *“We’d like you to consider how impactful you think a personalized email via our form from someone in that group to their national representatives would be. How impactful would a personalized email from someone in that group be in helping to enact climate policy?”* Participants answered on a scale from 1 (Not impactful at all) to 7 (Extremely impactful). In the second round of the email-valuation experiment, in contrast, we asked participants two questions to decompose their beliefs about email impact:

1. “Imagine that a climate bill were introduced to Congress in November 2023, and imagine that someone in this group sent a personalized email to their national representatives in Congress about the bill via our form. How likely do you think it is that their national representatives would read the personalized email?” Participants answered from 1 (Definitely won’t read) to 7 (Definitely will read).
2. “Imagine that 20 people in this group sent personalized emails to their national representatives in Congress about the bill via our form. Assuming that their national representatives read the personalized emails, how would these emails affect whether those representatives support the climate bill?” Participants answered from 1 (Make much less likely to support) to 9 or 11 (Make much more likely).

to support). We lengthened the scale from 9 to 11 after finding that 70% of participants clustered on only 2 values of the 9-point scale.

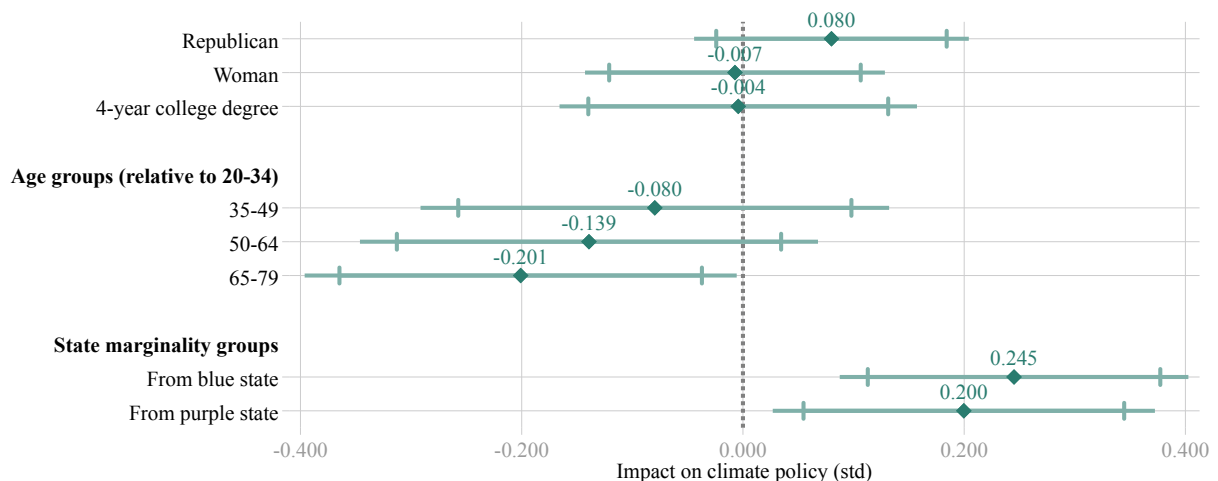
These altered questions offer two improvements: they decompose the concept of letter impact and allow for the possibility that emails to Congress about a climate bill could oppose it, letting us test our assumption throughout the project that participants expect all emails to support climate policy (Section 2.1.2). Given the methodological improvements of the second round, we present these results in the main text. However, Appendix Figures D25 and D26 show that our findings are robust to pooling data across rounds.

Figure D25: Determinants of choosing emails with certainty over carbon offsets, pooling across rounds



Note: This figure parallels Panel A of Figure 7, but we pool data from both rounds of the email-valuation experiment. This combination yields a total sample of 12,626 choices across 1,033 participants.

Figure D26: Perceived impact of emails on climate-policy support, pooling across rounds



Note: This figure parallels Panel B of Figure 7, except that we pool data across both rounds of the email-valuation experiment. We pool responses to the only impact question asked in round 1 and the second impact question asked in round 2 (see text above.) This combination yields a total sample of 1,002 responses.