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

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

**Abstract**

Policy change typically requires bipartisan support in Congress, and bipartisan constituent lobbying might be key to building these coalitions. Political movements spread through social networks, so citizens may themselves shape the composition of grassroots movements. Do citizen activists try to build bipartisan coalitions or to recruit others like them? We focus on the climate movement, where most advocates are Democrats. Even if citizens recognize the value of bipartisan action, record-high affective polarization—animosity towards counter-partisans—may hold back outreach across party lines. In online experiments with 21,000 participants, we give Democrats opportunities to recruit Americans across the political spectrum (all of whom believe climate change is human-caused) for climate advocacy. Democrats do try to recruit others, but they are 27% more likely to invite other Democrats than Republicans to email Congress—even while they say that a bipartisan climate movement would be more effective. This outreach gap does not arise from Democrats' own distaste for cooperating with counter-partisans, but rather can be explained by accurate beliefs that their invitation will have half as much impact on Republicans' action. Anticipated affective polarization drives these beliefs: Democrats estimate that Republicans would respond three times more to invitations that did not identify them as Democrats.

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## 1 Introduction

Addressing climate change in the long run in the US may require building a bipartisan coalition for climate policy: 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).

Mobilizing bipartisan grassroots citizen lobbying may be an effective tool in building this coalition in Congress. Constituent lobbying is common—over 20% of Americans contact politicians each year (Oliphant, 2018)—and changes legislators' behavior (Bergan, 2009; Bergan and Cole, 2015; Broockman and Skovron, 2018). If legislators differentially respond to their voter base, climate activists' partisan split could crucially affect their impact (Kastellec et al., 2015; Lax et al., 2019). Climate advocacy today skews heavily to the political left, but many on the right support climate policy and could in theory be brought into a broader coalition: 33% of Republicans agree that Congress should do more to address climate change, and 40-50% support a range of climate policies (Tyson, 2021; Leiserowitz et al., 2023).

Political and social movements spread through social networks, so members of the mainstream liberal climate movement may themselves shape the spread of this coalition (Nickerson, 2008; Bond et al., 2012; González, 2020; Bursztyn et al., 2021). But even if citizens recognize the value of bipartisan action, record-high affective polarization (Iyengar and Krupenkin, 2018; Boxell et al., 2022)—animosity towards counter-partisans—could prevent them from building coalitions across party lines. Americans favor co-partisans over counter-partisans in many domains (e.g. Gift and Gift, 2015; Huber and Malhotra, 2017), and there is widespread concern that affective polarization may reduce bipartisan cooperation (Hetherington and Rudolph, 2015; Finkel et al., 2020).

We run a series of online experiments with 21,000 participants homing in on three questions. First, do Democrats in the mainstream climate movement try to recruit others for climate advocacy? If so, do they differentially try to recruit others along versus across party lines? Finally, is any differential outreach strategic, or is it driven by preferences like affective polarization?

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 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 1b) 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.[1] 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.

In the *Wave-1 experiment* (N = 8,937), we first show that Democrats try to recruit others to email Congress. Before Wave-1 participants choose whether to email Congress, we randomize them across three arms. In an "Invitation" group, participants are told that if they email Congress, we will pass on invitations (Figure 1b) from them to future Wave-2 participants to join in action. In a "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—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 isolate influence motives, we randomize other Wave-1 participants to a "Tell-after" group that holds fixed these self- or social-image concerns: we will tell future Wave-2 participants if they email Congress, but only *after* those targets make their own email choices.

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, while people value telling others that they emailed Congress when it cannot affect their action during the survey itself, influence motives are also strong: Democrats are more likely to email Congress when doing so can inspire others to do the same.[2]

While Democrats work to build the climate movement overall, they are 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 *Willingness-to-Pay (WTP) 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; one of these choices will be randomly implemented. To identify influence motives, we randomize whether each of the 20 possible matches would see that the WTP participant emailed Congress before or after themselves deciding whether to do so. WTP participants are 27% more likely to try to recruit other Democrats than Republicans, even when they know that all matches believe that climate change is human-caused. This partisan gap dwarfs recruitment gaps by any other trait.

[1] Wave-1 participants see targets' political affiliation alongside their climate beliefs, shutting down the less interesting possibility that Democrats avoid mobilizing Republicans because they expect them not to believe in anthropogenic climate change. Similarly, we fix the email subject line to explicitly support climate action, minimizing concerns that Democrats avoid recruiting Republicans because they expect them to email Congress opposing climate policy.

[2] The Tell-after affect likely also captures influence motives: Tell-after participants may email Congress in part to motivate Wave-2 targets' post-survey action, and 30% of Tell-after participants mistakenly think they can affect whether Wave-2 participants email Congress during the survey itself.

Guided by a conceptual framework, we then use a series of mechanism experiments to decompose the preferences and beliefs that hold back Democrats' efforts to recruit counter-partisans. The partisan recruitment gap may be strategic, arising either because Democrats expect their invitations to have differential impacts on Democrats' and Republicans' likelihood of emailing Congress or because they expect emails from Democrats or Republicans to differentially impact climate policy. On the other hand, 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.

We find that Democrats strategically recruit co-partisans because they 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 $p = 0.029$. These beliefs closely match the true impacts of invitations in Wave 2. When we later 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.

Additional experiments suggest that this gap in influence beliefs fully explains Democrats' differential outreach along party lines. We find no evidence that Democrats prefer emails from co-partisans when they can be *obtained with certainty*. We ask a separate sample of 1,083 Democrats to choose, for a range of demographic groups, whether our research team should donate to carbon offsets or enlist someone in that group to email Congress for certain. Democrats are equally likely to choose emails from Democrats and Republicans. In the same set-up, we find no partisan gap in Democrats' beliefs about the impacts of emails from each group on climate policy in the US. Putting these together, we find no evidence that Democrats' reluctance to reach across the political aisle arises from a distaste for cooperating with Republicans.[3] Rather, Democrats differentially try to engage co-partisans because they don't expect cross-party outreach to succeed.

However, cross-party outreach may still be held back by a broad context of partisan antipathy: second-order affective polarization shapes Democrats' beliefs about the impacts of their invitations on Democrats versus Republicans. In a second round of the WTP 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 hide their political leanings from profiles shown to fellow Democrats only 8% of the time, they hide their political leanings from 56% and 66% of profiles that

[3]Our conceptual framework allows both for differential affective returns from knowing that a co- versus counter-partisan is engaged in the climate movement, as captured in the certainty experiment, and for differential affective returns from personally inviting a Democrat versus Republican to join in climate action, which the certainty experiment may not capture. In a variant of the WTP experiment, we find no evidence for partisan gaps in these other affective returns.

Republican matches would see after and before 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.

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, anticipated affective polarization holds back cross-party cooperation. Partisan outreach gaps are likely even starker in other settings—when working via segregated social networks (Cox, 2021; Jones et al., 2022) or with more scope for direct interaction, where affective polarization may play a 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 second-order polarization. For example, a growing literature in political science suggests that cross-party conversations can reduce both own and second-order affective polarization (Santoro and Broockman, 2022; Fang et al., 2023; Rossiter, 2023).

Our paper contributes to several key literatures. First, we add to the long and growing literature on the political-economy constraints to efficient environmental policy (e.g. Oates and Portney, 2003; Besley and Persson, 2023; Shapiro, 2021; Dechezleprêtre et al., 2022). While recent work studies international cooperation on climate change (Battaglini and Harstad, 2016; Harstad, 2016; Battaglini and Harstad, 2020), we focus on domestic coalition-building, finding frictions to grassroots cooperation that could hold back climate policy in polarized countries.

Next, we contribute to a growing literature on the spread of political movements (e.g. Qin et al., 2019; Aidt et al., 2022; García-Jimeno et al., 2022), where recent work shows that individuals have externalities on others' political action (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 political spillovers and, in fact, intentionally try to spread political movements.[4] 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; Han et al., 2017; Bursztyn et al., 2021; Turnbull-Dugarte et al., 2022), while most prior work focuses on voting (e.g. Gerber and Green, 2000; 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 find that influence motives combine with social-image concerns to make publicizing political action a powerful mobilization tool.

In doing so, we also add to a small literature finding evidence for influence motives in some pro-

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

social domains but not others (Reinstein and Riener, 2012; Karlan and McConnell, 2014; Esguerra et al., 2023). Esguerra et al. (2023) use a similar experimental design in contemporaneous work to show that Germans are more likely to register for COVID-19 vaccination when doing so may encourage others to do the same. While they find that participants have no impact on others' vaccination, Americans in our setting can in fact influence others' behavior, a feedback cycle that may be necessary to sustain influence motives in the long run. Together, our paper and Esguerra et al. (2023) suggest that opportunities to motivate others can effectively promote pro-social behavior.

Finally, we add to the literature on rising affective polarization in the US. While much work highlights that American favor co-partisans in non-political domains (e.g. Gift and Gift, 2015; Huber and Malhotra, 2017; McConnell et al., 2018), we add to the limited and mixed literature on how affective polarization affects democratic processes (Broockman and Ryan, 2016; Broockman et al., 2023). In our context, second-order affective polarization, but not Democrats' own partisan distaste, holds back grassroots cooperation. We also provide some of the first evidence on how beliefs versus preferences contribute to partisan engagement gaps. With a few exceptions (Dimant, 2023; Zhang and Rand, 2023), most prior work implicitly attributes these gaps to preference-based affective polarization. In contrast, we find that Democrats' relative unwillingness to reach across the political aisle is strategic.

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 to whom Democrats try to build the climate movement, and Section 6 examines the mechanisms underlying Democrats' differential efforts to reach out 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

We explore how Democrats try to expand the climate coalition by constructing a 21,000-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 connects two waves of participants: a Wave 1 of Democratic "influencers" and a Wave 2 of "targets," who belong either to the Democratic or Republican parties. Network participants are semi-anonymous, connected only by demographic profiles (Figure 1a).

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 costly opportunities to send invitations (Figure 1b) to Wave-2 participants to join in emailing Congress. The experiments in Sections 4, 5, and 6 use variants on this basic structure to provide a detailed picture of how Democrats try to expand the mainstream 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 "typical" climate activists: Democrats who believe that climate change is mostly human-caused (Atske, 2020; Leiserowitz et al., 2023). 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.[5] While our sample therefore cannot represent the entire environmental movement, it well approximates a mainstream movement that remains predominately white, affluent, and educated.[6] 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 restrict 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; Leiserowitz et al., 2023), seek to broaden it with potential allies across the aisle or to recruit others like themselves. 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.

### *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

[5]For 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 politically 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 able to recruit few non-white participants in experimental pilots, so we restrict Waves 1 and 2 to participants who live in the contiguous US, are between 20 and 79, identify as a man or woman, and identify as white. Appendix A.1 details participant screening.

[6]The mainstream US environmental movement has historically been dominated by white and educated members of the middle class (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).

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, its subject line is fixed and supports climate action. We impose these fixed subject lines to mitigate 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 analyze whether Democrats choose to pursue *allied* advocacy across party lines.

### 2.2 Motivating descriptive evidence

Before we experimentally observe whether and how Democrats seek to recruit others for climate action, we recruit a non-experimental sample of Democrats (N = 197) whom among whom we document a series of qualitative patterns.[7]

**Democrats want bipartisanship on climate and see their own role in building it:** 89% of our sample agrees that building support for climate policy among both Democratic and Republican politicians is crucial for reducing emissions, and 87% agree that the US can only pass future climate legislation if lawmakers in both parties support it (Panels A and B, Appendix Figure A3). 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 A3). Democrats see a role for themselves in building this bipartisan movement: 82% agree that Democrats should try to get more Republicans involved in climate advocacy (Panel B, Appendix Figure A4).

**But few have themselves tried to recruit Republicans**: 58% of Democrats in our sample say that they've invited someone to join in climate advocacy in the last 5 years, but they've largely invited co-partisans (Panel A, Appendix Figure A5). While this gap could arise simply because Democrats have tighter social ties to other Democrats (Panel B, Appendix Figure A5), it could also stem from more subtle beliefs or preferences, such as affective polarization. Our experimental design allows us to 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 both co- and counter-partisans.

[7] We recruit the motivating sample via the same advertising and sample restrictions as the Wave-1 experimental sample. These samples are statistically indistinguishable on most baseline traits (columns 3 and 4, Appendix Table A1).

## 3 Conceptual framework

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

### 3.1 Identifying efforts to build the climate movement

Consider a Wave-1 citizen who has just emailed Congress and can pass on a profile (Figure 1b) inviting Wave-2 participants to do the same. Do invitation choices capture whether Democrats value recruiting others for climate action? We outline the payoffs to inviting others as follows:

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

Participants may indeed value mobilizing others, either to email Congress during the survey itself, $V(Try\ Influence\ Email)$, or to engage in pro-climate action after the survey, $V(Try\ Influence\ After)$. However, they may in part pass on invitations because they derive self- or social-image benefits from telling others that they emailed Congress, $V(Image)$. Inviting someone to email Congress may also incur costs in money, time, or emotional costs of interaction, $C$. Equation 1 highlights a key empirical challenge: to test whether Democrats pass on invitations in order to mobilize others for climate action, we must separate influence motives from the image returns of simply telling others that one emailed Congress, $V(Image)$.

In several experiments, we test whether participants value mobilizing others by varying *when* they 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:

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

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, $V(Try\ Influence\ Email)$.[8]

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*.

[8]Contemporaneous work by Esguerra et al. (2023) and earlier work by Karlan and McConnell (2014) use similar experimental variation to isolate influence motives. Note that this identification strategy does not assume equal social-image returns overall in $V(Invite)$ versus $V(Tell\ After)$; we describe below that $V(Try\ Influence\ Email)$ may include social-image returns to being seen as trying to mobilize others. We assume 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.

### 3.2 Decomposing influence motives

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

$$\begin{aligned} V(Try\ Influence\ Email) &= \Delta P(Email) * [V(Email\ Impact) + A(Target\ Involved)] \\ &\quad + A(Try\ Influence\ Email) \end{aligned} \tag{3}$$

If participants only value impacts on climate policy, the returns to trying to influence others would be the product of $\Delta P(Email)$—the sender's beliefs about her invitation's impact on the probability that the target emails Congress—and $V(Email\ Impact)$—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 value seeing themselves or being seen by others as trying to recruit action, $A(Try\ Influence\ Email)$. Second, the total value of successfully recruiting the target—the terms scaled by $\Delta P(Email)$—may also include warm-glow returns to cooperating with that person in the climate movement, $A(Target\ Involved)$.[9]

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

What could then underlie partisan gaps in attempts to recruit others?

**Strategic drivers of partisan gaps:** First, such gaps could arise from strategic beliefs. Democrats may believe that their invitations would have differential impact on whether Democrats versus Republicans email Congress ($\Delta P(Email|D) \neq \Delta P(Email|R)$), in line with evidence that Americans trust co-partisans more in a range of settings (Lee et al., 2014; Marks et al., 2019; Zhang and Rand, 2023). Participants may also believe that emails from Democrats or Republicans would be differentially impactful in achieving short- or long-term climate policy goals ($V(Email\ Impact|D) \neq V(Email\ Impact|R)$), perhaps because they would advocate for different policy solutions, serve as differentially informative signals of citizen preferences, or influence co-partisan lawmakers.

**Preference drivers of partisan gaps:** On the other hand, Democrats may differentially try to mobilize co- versus counter-partisans due to *preference* gaps arising from affective polarization (Finkel et al., 2020; Kingzette et al., 2021; Broockman et al., 2023). In our framework, Democrats may derive differential emotional returns from cooperating with co- versus counter-partisans in the climate movement ($A(Target\ Involved|D) \neq A(Target\ Involved|R)$) or from trying to reach out to

[9]This framework assumes that the 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 $A(Target\ Involved)$ or $A(Try\ Influence)$.

Democrats or Republicans ($A(Try\ Influence\ During|D) \neq A(Try\ Influence\ During|R)$).[10]

**Role of affective polarization in strategic drivers:** While affective polarization may directly shape gaps in affective returns, second-order or anticipated affective polarization may also drive gaps in Democrats' beliefs about their ability to influence others, $\Delta P(Email)$. In particular, Democrats may estimate $\Delta P(Email|D) > \Delta P(Email|R)$ because they expect counter-partisans to dislike them.

### 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 *Willingness-to-pay (WTP) experiment* (Section 5), use the timing logic of Section 3.1 to show that Democrats are motivated to engage others in climate action and differentially try to engage co-partisans.

Guided by Section 3.2, we then run a series of mechanism experiments to identify the beliefs and preferences that underlie why Democrats differentially reach out along party lines. We measure Democrats' beliefs about their influence on co- and counter-partisans' action, $\Delta P(Email|D,R)$; test for partisan gaps in the perceived impact of emails to Congress, $V(Email\ Impact|D,R)$; and test for gaps in the affective returns to cooperating with Democrats and Republicans, $A(Try\ Influence\ During|D,R)$ and $A(Target\ Involved|D,R)$. Finally, we test whether second-order affective polarization drives Democrats' influence beliefs, $\Delta P(Email|D,R)$.

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 Democrats' strategic 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.

**Learning that Wave-2 matches will see their demographic profile:** Wave-1 participants first build a basic demographic profile (Figure 1a). Next, they learn that this profile will be shown to up to 10 future Wave-2 participants in a particular demographic group to give them a sense of other

[10] Affective polarization may also drive partisan outreach gaps if Americans simply dislike interacting with counter-partisans (Chen and Rohla, 2018). This gap would enter our framework as differential costs, $C$, in Equation 1. This channel is likely small in our setting given the limited participant interactions. We don't test its existence in particular, instead decomposing the returns to mobilizing others for climate action *conditional* on interacting with them. Distaste for cross-party interaction could make partisan outreach gaps even starker in other settings.

study participants.[11] 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.

In the **"Control"** group, participants are told nothing about whether future participants will know if they email Congress. In the **"Invitation" group**, participants learn that if they email Congress, the same Wave-2 participants who see their basic demographic profile will also see a profile saying that they emailed Congress and inviting them to join in doing so (Figure 1b); Wave-2 targets would see this invitation **before** deciding whether to email Congress. Finally, in the **"Tell-after" group**, participants learn that if they email Congress, their paired Wave-2 participants will see a profile saying that they emailed Congress (Figure 1c), but only **after** deciding whether to do so 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.

Guided by the framework in Section 3, 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 so; comparing email rates between these groups identifies whether Democrats try to engage others in climate advocacy. Any gaps in email rates between the Tell-after and Control groups may reflect image benefits of knowing others will be told that you emailed Congress, as well as any efforts to affect Wave-2 participants' behavior after the experiment.[12]

**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 an address, and finally write and send their emails.

### 4.2 Wave-1 empirical specifications

We first estimate the following specification among those in the Control and Invitation groups:

$$Email_i = \alpha + \beta_1 Invite + \Phi X_i + \varepsilon_i \tag{4}$$

where $Email_i$ indicates that participant $i$ emailed Congress, $Invite_i$ indicates that participant $i$ is in

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

[12] Any Invitation or Tell-after effects cannot operate through participants' concerns about showing their demographics to Wave-2 participants: all Wave-1 participants know that future participants may see their demographics, no matter their treatment group or whether they email Congress.

the Invitation group, and $X_i$ is a vector of controls. Here, $\beta_1$ gives the policy-relevant impacts of opportunities to invite others. We then isolate influence motives in the full Wave-1 sample:

$$Email_i = \alpha + \beta_2 Tell_i + \beta_3 Invite + \Phi X_i + \varepsilon_i \tag{5}$$

where $Tell_i$ indicates that participant $i$ is in the Invitation or Tell-after groups. $\beta_2$ captures the effects of knowing that future participants will be told if one emails Congress, while $\beta_3$ isolates 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 record to Congress sent via the embedded form. We match participants to email records by first name and email address, their treatment status, and the timing of their survey completion. We successfully merge 94% of all recorded emails sent via the embedded Wave-1 forms to participants, with no differential success by treatment group (Appendix B.2). 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.[13]

**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 fully robust to excluding them.

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

**Recruitment:** We recruit Wave-1 participants with ads on Facebook, Instagram, and Twitter for a survey on climate change or environmental issues; we then screen for members of the Democratic party who believe that climate change is mostly human-caused. In total, 29,596 participants consented to the Wave-1 survey and 12,755 participants met our screening criteria. We randomized 8,937 participants into the Wave-1 sample and 2,004 participants into parallel 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 explanation and comprehension questions before deciding. Thus, we directly observe whether only 87% of the Invitation and Tell-after groups email Congress, with no differential attrition between them, while observing active email choices for 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

[13] 8% of those who start the email process opt out after learning that the form will ask for their address. 20% of those who still say they want to email Congress at the second elicitation 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.

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 fully robust to including baseline controls for the small imbalances that remain (Appendix B.4).

**Set-up comprehension and attention:** Most Wave-1 participants 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 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 that Democrats do indeed seek to mobilize others to email Congress, with suggestive evidence that they may differentially seek to mobilize co-partisans.

#### *4.4.1 Impacts of opportunities to invite others*

We first consider the total effects of the Invitation treatment, finding that opportunities to invite others to join in emailing Congress substantially increase climate advocacy. While 31% of the Control group emails Congress, participants in the Invitation group are 16pp more likely both to initially opt into the email process and to match to an email record than those in the Control group, increases of 34% and 52%, respectively (column 1, Table 2).

**Isolating influence motives:** Next, we use the Invitation versus Tell-after variation to show that these total effects arise in part from participants' efforts to mobilize Wave-2 participants 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 (column 2, Table 2). 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 have an email record (column 2, Table 2), about 7% and 9% of the relevant control means, respectively. Then, 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 knew they could motivate others (Appendix Table B3).[14] Second, we find no evidence that the $\beta_3$ effect arises because the Invitation treatment changes participants' beliefs about overall rates of action or induces differential social-image benefits from others knowing that one emailed Congress.[15]

**Influence motives in the Tell-After effect:** While the Invitation versus Tell-After effect shows that influence motives exist, this effect alone likely under-estimates influence motives here: the large Tell-after effects likely also reflect attempts to affect targets' action after the survey or (mistakenly) during the survey itself. Indeed, 17% of Tell-after participants said that the treatment affected them because they it allowed them to motivate others, about 66% of those who said it affected them overall (Appendix Table B3). 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 nearly all others answering that they don't know, and about 60% say it would be at least somewhat likely to do so (Appendix Figure B7). Moreover, 28% of Tell-after participants incorrectly state that their choice to email Congress could affect targets' action during the survey itself (Appendix Figure B5, Panel B), so the Tell-after affect also captures these mistaken influence motives.

The large Tell-After effects likely also derive in part from self- or social-image concerns, which are strong drivers 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 said that it activated social-image concerns or made them feel accountable, about 30% of those who said it affected them overall (Appendix Table B3).

**Robustness:** Our main Wave-1 results are robust to excluding participants who misunderstand the

[14] A 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 significant different 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.

[15] If Invitation participants expect other survey-takers to also have opportunities to influence others' action, they may differentially revise their beliefs about aggregate email rates via our survey; these beliefs may then affect their action either 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; Rogers et al., 2018; Cantoni et al., 2019; Hager et al., 2022). 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 had strong cover for acting on social image: free-text responses suggest that many in the Tell-After group saw telling others that they emailed Congress as an opportunity to show others the strength of the climate movement or mobilize them after the survey, 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).

experimental set-up or guessed the study purpose,[16] to changing controls, and to correcting for differential demand effects (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.

**Partisan gaps in the total Invitation effect ($\beta_1$):** When Democrats are paired with Wave-2 Democrats (column 3, Table 2), the Invitation treatment makes them 18pp (40%) more likely to opt into the email process and 18pp (58%) more likely to match to an email record. In contrast, the Invitation treatment only makes Democrats 12pp (26%) more likely to opt into the email process and 14pp (44%) more likely to match to an email record when paired with Wave-2 Republicans (column 5, Table 2). We reject ($p = 0.03$) that the Invitation effects on opting into the email process are equal across those paired with Democrats and Republicans.

**Partisan gaps in influence motives ($\beta_3$):** Our point estimates imply that these partisan gaps arise in part because Democrats differentially try to mobilize action by other Democrats. Here, we test for differential $\beta_3$ coefficients in Equation 5 when Wave-1 Democrats are 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 match an email record when doing so could influence whether Wave-2 Democrats do the same (column 4, 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 (column 6, Table 2). 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 section explores these partisan gaps in more detail.

## 5 The WTP 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. 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 a fixed carbon-offset donation from our research funds. This "Willingness-to-pay (WTP) 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.

[16] In responses hand-coded by a treatment-blind academic, only 2-3% of Tell-After and Invitation participants guessed that the study purpose was to test whether people try to influence others (Appendix Table B4). Most guessed that the purpose was to measures of climate actions and beliefs, to mobilize political action, or to estimate spillovers from invitations on others' action.

### 5.1 WTP experimental structure

**The WTP sample:** We embed the WTP 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 WTP 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 WTP 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.[17] Each WTP participant will be randomly matched with one these 20 others, who will see a profile of the WTP participant when they return for our follow-up survey. For each possible match, participants then choose between the following two options for the profile that match would see if paired:

- *Option 1. "Basic profile" and carbon offsets:* We will show the returning match the WTP participant's basic demographic profile and add some amount, randomized at $3 to $6 across participants, to a carbon-offset purchase by our research team.[18]
- *Option 2. "Extended profile:"* We will show the returning match an extended profile that both includes the WTP participants' 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 (Appendix Figure C1; described in more detail below), so they may choose differentially by match traits. We describe the appearance of the basic and extended profiles in more detail below.

**Identifying influence motives by randomizing profile timing:** As in the Wave-1 action experiment, we identify influence motives using variation in *when* possible matches would see that the WTP participant emailed Congress. Within participant, we randomize whether each possible match would see their profile *before* or *after* themselves choosing whether to email Congress; we motivate 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 denoted "Action status" in matches' profiles (Appendix Figure C1) communicates this timing: those who would see one's

[17] We recruited these past study-takers before starting the Wave-1 experiment and from participants 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.

[18] We 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., Forthcoming), participants value offsets in this context (see footnote 21).

profile *before* versus *after* 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 WTP 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 all possible matches, basic profiles are identical to the demographic profile that all WTP participants created in the Wave-1 experiment (Figure 1a). 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 1b—including an invitation to join in action—while one assigned to see an extended profile *after* deciding would see the profile shown in Figure 1c.

**Testing differential targeting by match traits:** WTP participants' possible matches are real people who vary in a range of traits appearing in their profiles (Appendix Figure C1), 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. In particular, we test whether WTP participants differentially try to recruit co- or counter-partisans.[19]

***By state politics:*** Another key test of interest is whether WTP 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 categories capturing how citizen action could affect legislator action: "blue states," where legislators would likely support a climate bill even if few residents advocated for it; "red states," where legislators would be unlikely to support a climate bill even if residents 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 WTP participants either to see profiles of possible matches that show that all matches believe climate change is human-caused or that hide these beliefs (Appendix Figure C1). This cross-participant variation lets us test how inferences about climate beliefs drive any partisan gap in Democrats' attempts to recruit others.

[19] One 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 WTP 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 WTP participants that we will only make the carbon-offset donation if their paired match returns for a follow-up survey and sees their basic demographic profile.

### 5.2 WTP empirical specifications

We estimate partisan gaps in WTP participants' mobilization attempts as follows:

$$Extended_{ij} = \alpha_i + \beta_1 Before_{ij} + \beta_2 Before_{ij} * Dem_{ij} + \beta_3 Dem_{ij} + \Theta P_{ij} + \varepsilon_{ij} \quad (6)$$

where $Extended_{ij}$ indicates that WTP participant $i$ chose to show possible match $j$ an extended profile saying that they emailed Congress, $Before_{ij}$ indicates that possible match $j$ would see participant $i$'s profile before deciding whether to email Congress or not, and $Dem_{ij}$ indicates that possible match $j$ leans towards the Democratic party. $\beta_1$ captures whether WTP Democrats 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 $\beta_2$ tests whether this "influence" gap differs when paired with Democrats. Finally, $\beta_3$ tests whether Democrats are more or less likely to tell co- or counter-partisans that they emailed Congress when it cannot affect their in-survey action.

**Control variables:** Each WTP participant makes 20 binary choices, so our main regression specifications control for participant fixed effects, $\alpha_i$. We also control for $P_{ij}$, a vector of other match features. $P_{ij}$ only includes choice number in our main specifications, but our results are fully robust to controlling for other demographics in match $j$'s profile (Appendix C.5).[20]

### 5.3 WTP sample recruitment, summary, and fidelity

**Attrition:** In total, 1,350 participants start the WTP survey after emailing Congress from the Wave-1 Control group (see Appendix C.3 for recruitment details). Just before participants begin making choices for each of their 20 possible matches, we randomize 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) complete 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 WTP sample captures "inframarginal" climate activists—those who email Congress from the Wave-1 control group. Consistent with this selection, the WTP sample is more concerned about climate change and has higher baseline political engagement than the full Wave-1 Control group (columns 1 and 2, Appendix Table C2). The WTP sample is largely balanced between those who see or do not see information on matches' climate beliefs (columns 3-5, Appendix Table C2). Participant fixed effects control for any remaining baseline differences in our main analysis.

[20] In particular, one concern is that matches' political leanings are correlated with their other traits, so $\beta_2$ could be biased by differential attempts to mobilize matches by these correlated traits. We will show that $\beta_2$ is robust to controlling for the interaction of $Before_{ij}$ will all other traits shown in possible matches' profiles.

**Set-up comprehension:** We invest heavily in ensuring that participants understand the WTP set-up. After describing the set-up once, we review it while asking a series of 8 incentivized comprehension questions and correcting participants' answers. Participants initially answer an average of 5.8 questions correctly (Appendix Figure C2), and our results are robust to restricting to participants who initially answer at least half correctly (Appendix C.5).

### 5.4 WTP results: Democrats differentially try to mobilize co-partisans

**WTP participants try to mobilize political action:** As in the Wave-1 experiment, WTP 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 versus after choosing whether to email Congress, and thus when doing so could affect matches' political action. 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).[21,22]

**A large partisan gap in outreach attempts:** WTP participants differentially try to recruit other Democrats to email Congress. When climate beliefs are hidden (column 2, Table 3), knowing that a Republican match would see their profile *before* deciding whether to email Congress makes WTP participants 30 pp more likely to choose an extended profile; this influence gap is 24 pp (81%) higher when matches are Democrats. This partisan influence gap falls but remains substantial 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, WTP 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 choice. Again, these partisan gaps could arise from differential social-image concerns or efforts to affect matches' action after the survey.

[21] Do WTP participants value carbon offsets? We randomize 132 participants to 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 (Appendix C.4). 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 giftcard.

[22] We 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 C3.)

**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 they lack information on matches' climate beliefs, participants 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 they know that all matches believe climate change is human-caused (Appendix C.2).[23]

Targeting by shared politics also dwarfs differential recruitment by participants' non-political similarity with possible matches (Panel B, Figure 4). When WTP 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.

**Robustness:** The WTP results are robust to changing controls and excluding participants who misunderstand the experimental set-up, guessed the study purpose,[24] or expressed concern that Republican emails would actively oppose climate policy (Appendix C.5).[25]

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

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

In this section, we return to Section 3's conceptual framework to decompose the beliefs and preferences that underlie Democrats' reluctance to reach across party lines. We find no evidence that these gaps arise from Democrats' distaste for cooperating with counter-partisans. Rather, they may be fully explained by Democrats' strategic beliefs that their invitations have about twice as much impact on Democrats' than Republicans' likelihood of emailing Congress. On the other hand, *anticipated* affective polarization shapes these strategic beliefs: Democrats estimate that hiding their own politics from invitations to Republicans would make them three times as effective.

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

[24] In responses hand-coded by a treatment-blind academic, 23% of WTP participants guessed that the experiment aimed to test whether people try to influence others, but only 25% of those (6% of the full sample) guessed that we were interested in the role of politics in these influence motives (Appendix Table C4).

[25] Recall that we aim to test whether Democrats pursue allied cross-party action as much as possible (Section 2.1.2).

### 6.1 Characterizing strategic 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' beliefs for 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 in emailing Congress. We then test the invitations' impacts on whether Wave-2 participants email Congress via our embedded form.

**See basic demographic profile of Wave-1 match:** Wave-2 participants first build a basic demographic profile (Figure 1a). 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. 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.[26]

**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 from the same Wave-1 participant whose demographic profile they saw earlier; 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.[27] To eliminate the risk that Wave-2 participants who see invitations infer that their own action will be shown to others, treatment 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, and fidelity*

**Recruitment:** We recruit 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 were restricted to believe that climate change is mostly human-caused. In total, 8,685 participants were randomized to a Wave-2 treatment, of whom 3,002 lean Republican and 5,683 lean Democratic.

[26] Note that we show all participants an earlier participant at this stage to ensure that any impacts of invitations do not arise just from seeing details about a Wave-1 participant.

[27] To ensure that this statement is truthful, we only pass on invitations from Wave-1 participants who emailed Congress and knew when they did so that an invitation would be passed on to Wave-2 survey-takers in the Wave-2 participant's demographic group. The Online Supplement describes this matching process in detail.

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.

**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 and 2, Appendix Table D2). Wave-2 Democrats are older, more likely to live in blue or purple states, more educated, and wealthier than Wave-2 Republicans, as well as reporting higher baseline political engagement, concern about climate change, and preference for friends of their own party.[28] 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 3 through 8, Table D2).

**Set-up comprehension and attention:** Wave-2 comprehension is high (Appendix Figure D1). Treated participants are 60pp more likely to state that their paired Wave-1 participant emailed Congress when they took our survey. Even when treatment participants state that they don't know whether their Wave-1 match emailed Congress, they guess that their match 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:

[28] While some of these gaps, such as in educational attainment, mirror demographic gaps across parties in national samples (ANES 2020), others diverge. For example, Democratic- and Republican-leaners report about equal political engagement in national samples (Oliphant, 2018). These differential gaps in our survey in part reflect the differential selection induced among liberals and conservatives when restricting to those who believe climate change is mostly human-caused—88% of Democrats and only 35% of Republicans say so in national samples (Fuong and Skelley, 2022)—a form of differential selection that is also at work in the real-world climate movement. More artificially, the differences between liberals and conservatives in the Wave-2 sample also arise in part from the fact that we largely recruited 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.

$$Email_i = \alpha + \beta Treatment_i + \Phi X_i + \varepsilon_i \quad (7)$$

where $Email_i$ indicates that participant $i$ emails Congress, $Treatment_i$ is an indicator for being assigned to see a Wave-1 invitation, and $X_i$ is a vector of control variables. Here, $\beta$ captures the impacts of invitations on the likelihood that participants email Congress.

As in the Wave-1 experiment, our primary outcome variable is whether participants match to a record for an email sent via our form. We successfully merge 91% of all recorded emails sent via the embedded Wave-2 forms to Wave-2 participants, with no differential success by treatment group (Appendix 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 that participants identify as members of the Republican party, Republican-leaning Independents, Democratic-leaning Independents, or members of the Democratic party.

#### *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 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.[29] We reject ($p$ = 0.023) that invitations have the same absolute impact on whether Democrats and Republicans match to email records.[30,31]

**Invitations' impacts vary with other recipient and match traits:** In addition to having larger impact on Democrats, Wave-1 invitations have larger impacts on action by Wave-2 women and those aged 60 to 79 ($p = 0.067$). Notably, Wave-2 participants may 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

[29] Appendix 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.

[30] Proportionally, 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-partisasns 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 9).

[31] The Online Supplement explores the mechanisms by which invitations increase action. These effects may be primarily 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 participants' perceived norms of political action in our context.

and state (Appendix Figure D2).

**Robustness:** The Wave-2 results are highly robust to excluding participants who misunderstand the experimental set-up or guessed the study purpose,[32] changes in control variables, and controlling for experimenter demand effects (Appendix D.2.2).

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

**The belief sample:** We estimate Democrats' beliefs for $\Delta P(Email)$ in a "belief sample" that we recruited alongside Wave 1. Participants complete the Wave-1 experimental survey through seeing the upcoming opportunity to email Congress; we then randomize them to complete the "certainty experiment" described in Section 6.2 and the belief elicitations here.[33]

**Estimating beliefs:** To estimate $\Delta P(Email)$ beliefs, we ask participants to estimate how many out of 100 Democratic and 100 Republican participants in a particular Wave-2 demographic cell would email Congress in each of two cases: if they did or did not see invitations from matched Wave-1 Democrats. In total, 397 participants complete all four belief elicitations, of whom we incentivized 194 for accuracy (Appendix Figure D3).[34] We analyze incentivized participants throughout this section, estimating the following regressions:

$$ProbEmail_{ijp} = \alpha_i + \beta Profile_{ijp} + \varepsilon_{ijp} \tag{8}$$

Here, $ProbEmail_{ijp}$ gives participant $i$'s guess of how many of 100 participants in group $j$ would email Congress in elicitation $p$, and $Profile_{ij}$ indicates that elicitation $p$ is a case in which group $j$ would see invitations to act. $\alpha_i$ are individual fixed effects. Here, $\beta$ 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 $\beta$ across the two parties.

[32] In responses hand-coded by a treatment-blind academic, only 7% of the Wave-2 treatment group guessed that the study purpose was to estimate spillovers from invitations on other's action (Appendix Table D5). Most guessed that the purpose was to measures of climate actions and beliefs or to mobilize political action.

[33] 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 WTP sample. Note that the belief sample differs slightly from the main Wave-1 sample, despite randomization, because of attrition during the certainty experiment and belief elicitation. Appendix Figure D3 summarizes belief-sample recruitment and attrition.

[34] We 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).

#### 6.1.6 *Results: Democrats correctly estimate $\Delta P$ 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 3.2pp more likely to do so (Panel A, Figure 5).[35,36] While Democrats overestimate overall rates of action, we cannot reject that these average $\Delta P$ beliefs equal the true impacts of invitations on Wave-2 action. In other words, they correctly anticipate that invitations will have about twice as much absolute impact on action by co- versus counter-partisans. This accuracy stands in stark contrast to a large literative 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, and match the sender on an additional non-political trait.

### 6.2 The certainty experiment: Shutting down $\Delta P(Email)$ beliefs

In the next mechanism experiment, we find that these $\Delta P(Email)$ influence beliefs fully explain Democrats' differential tendency to reach out along, rather than across, party lines. In particular, we find no evidence that Democrats differentially recruit emails from co- and counter-partisans *when they can be obtained with certainty*, shutting down the role of $\Delta P(Email)$. Thus, we find no evidence that Democrats' own distaste for engaging Republicans in the climate movement, captured in $A(Target\ Involved|D,R)$ or $A(Try\ Influence\ During|D,R)$ in Section 3's framework, holds back cross-party outreach. Rather, Democrats appear to strategically target co-partisans because they expect it to be more effective.

#### 6.2.1 *Experimental structure, sample, and specifications*

In the "certainty experiment," 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 for14 demographic

[35] Besides calculating average values for $\Delta P(Email)$ in Equation 8, we use each participant's multiple reports to derive individual's estimates of $\Delta P(Email)$, though these estimates are likely noisy (Panel A, Appendix Figure D4). The belief sample is 16pp (22%) less likely to expect $\Delta P(Email)$ to be positive and 14pp (78%) more likely to expect it to be negative for Republican than Democratic targets (Panel B, Appendix Figure D4). About 56% estimate that $\Delta P(Email|D) > \Delta P(Email|R)$ in their matched Wave-2 group (Panel C, Appendix Figure D4).

[36] We cannot reject that participants' estimates for $\Delta P(Email)$ are equal with and without accuracy incentives, but those without incentives estimate somewhat higher $\Delta P(Email)$ on Republicans (Appendix Figure D5). This gap could stem from lower effort or motivated reasoning.

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 through the experiment (e.g. Figure 1a). These profiles are evenly split between leaning towards the Republican and Democratic parties. For each demographic profile, participants then choose between two options:

- *Option 1. Carbon offset donation:* The research team will donate $10 to carbon offsets.
- *Option 2. Enlist an email with certainty:* We will enlist 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.[37]

Appendix Figure D6 shows how we explain these choices to participants. We incentivize them by saying that we will implement one choice for 30 participants.

**Mapping the certainty experiment to the framework in Section 3:** By offering Democrat the choice to recruit emails with certainty, this experiment sets $\Delta P(Email|D,R)$ to 1. Thus, this exercise elicits Democrats' valuation of emails to Congress along and across party lines, the sum of how much they value the impact they expect an email to have ($V(Email\ Impact|D,R)$) and any affective preference for engaging the email writer in the climate movement ($A(Target\ Involved|D,R)$). By combining estimates of any partisan gaps in expected email impacts, $V(Email\ Impact)$, with any gaps in overall preferences for emails by 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 key risk of affective polarization here.[38]

**Recruitment and sample summary:** In total, 574 participants complete all choices in the certainty experiment (Appendix D.3 details recruitment).[39] Like the belief sample, participants in the certainty sample are somewhat younger, more likely to be women, and more likely to have a college degree than the main Wave-1 experimental sample (columns 8-10, Appendix Table A1). While the certainty sample is slightly more worried about climate change, they largely match the Wave-1 sample in climate beliefs, affective polarization, and engagement.

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

[38] This experimental exercise likely does not capture gaps in Democrats' affective returns to seeing themselves or being seen by others as trying to recruit climate action—($A(Try\ Influence\ Email|D,R)$), where affective polarization may also play a role—because participants here are not themselves reaching out to co- and counter-partisans to recruit action. We will test separately...

[39] In addition to this sample, we ran a first round of the certainty experiment with about 400 participants, as preregistered. This first round suffered from several implementation errors, so we re-ran the experiment with an improved design. Appendix D.3.2 details the first round of the experiment and shows that our results throughout this section are robust to pooling data across both experiments.

**Empirical specifications:** We estimate the following regression in the certainty sample:

$$Email_{ij} = \alpha + \beta_1\, Dem + \beta_2\, Woman_{ij} + \beta_3\, College_{ij} + \sum_{a=2}^{4} \gamma_a\, Age_{ija} + \sum_{s=2}^{3} \delta_s\, State_{ijs} + \varepsilon_{ij} \quad (9)$$

where $Email_{ij}$ indicates that certainty participant $i$ chooses an email from demographic profile $j$ over a carbon offset donation, and $Dem_{ij}$, $Woman_{ij}$, and $College_{ij}$ indicate that members of demographic group $j$ 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 climate-marginality. The set of $\beta$, $\gamma$, and $\delta$ coefficients capture Democrats' relative preferences for emails to Congress from Americans with varying demographic traits and to legislators in states across the political spectrum.

### *6.2.2 Results: No evidence that Democrats' affective polarization holds back outreach*

**No partisan preferences for emails obtained with certainty:** When Democrats can obtain emails with certainty, they are equally likely to recruit emails over offsets for Democrats and Republicans (Figure 6). In other words, the large partisan gaps in Democrats' $\Delta P(Email)$ beliefs fully explain differential outreach along and across party lines.

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 11pp less likely to choose an email over offsets for 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).[40]

**No detectable gaps in perceived email impacts or affective returns to cooperation:** 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 emotional benefits by partisanship, or because of offsetting partisan 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

[40] While we might expect participants to differentially prioritize emails from Democrats or Republicans in red, purple, or blue states, we find no partisan gaps in email choices in any state-politics category (Appendix Table D6).

from individuals across the political spectrum. We elicit Democrats' beliefs for $V(Email\ Impact)$ by asking them to imagine that a climate bill were introduced to Congress several months later, randomly showing them one demographic profile, and asking them to imagine that someone in that group emailed Congress about the bill via our form. Then, participants answer two Likert-scale questions. First, how likely would national representatives be to read the email from someone in that group? Second, if representatives read emails from 20 people in that demographic group about the bill, how would the emails affect the representatives' support for the bill? We then regress these standardized impact beliefs on the email-writer traits in Equation 9.

We find no gap in either measure of perceived impact when pooled across state groups (Figure 7), though we find marginally-significant evidence that Democrats perceive emails from Republicans to differentially affect legislator support in red states (column 6, Appendix Table D6). While we interpret these impact measures with caution,[41] they suggest that Democrats do not perceive differential impact from emails by Democrats and Republicans when making choices over individual emails. Combining the null partisan gap in email choices with the null partisan gap in perceived email impacts ($V(Email\ Impact)$), we find no evidence that Democrats have differential affective preferences to cooperate with co- versus counter-partisans in climate action, ($A(Target\ Involved)$).

**No heterogeneity by affective polarization across outreach choices:** Consistent with the lack of evidence here for Democrats' own affective polarization playing a role in their email choices, we find no consistent evidence across the Wave-1, WTP, and certainty experiments that Democrats with stronger distaste for counter-partisans have larger partisan outreach gaps (Appendix Figure D7; Appendix D.4 describes how we measure affective polarization).[42]

### 6.3 Letting Democrats hide politics: Second-order affective polarization shapes ΔP

So far, our results suggest that Democrats startegically target co-partisans becuase they correctly expect cross-party outreach to be less effective ($\Delta P(Email|D) > \Delta P(Email|R)$); we find no evidence that Democrats' own distaste for engaging Republicans in the climate movement holds back cross-party outreach.

However, our last mechanism experiment suggests that Democrats' influence beliefs, $\Delta P(Email)$

[41] According to these measures, Democrats actually rate emails from Americans in blue and purple states as more effective than from those in red states, even while they are less likely to choose emails from Americans in blue states. This pattern suggests that there are other returns to mobilizing action in red or purple states that do not map neatly either to affective polarization or to the concept of short-term email impact that our questions elicit here. For example, Democrats may see mobilizing action in red and purple states as in line with an effective long-term climate movement, even if they expect legislators to ignore their emails in the short term.

[42] If anything, partisan outreach gaps are usually larger for Democrats with lower affective polarization. The only exception is in the certainty experiment, where those with below-median stated preference for friends who are Democrats rather than Republicans are more likely to choose emails from Republicans than Democrats. Nonetheless, even those with above-median affective polarization on this measure are equally likely to choose emails from Democrats and Republicans over offsets.

are crucially shaped by a context of strong partisan mistrust. 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 revised WTP 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. This variation lets us test the role of anticipated affective polarization by measuring WTP participants' beliefs for how their own party alignment shapes Democrats' and Republicans' reactions to their invitations.

#### *6.3.1 Experimental structure and sample*

The round-two WTP experiment follows the same procedure as the main WTP experiment described in Section 5.1, with two key alterations. Most importantly, we randomize half of round-two WTP participants to have the option to hide their own political leanings from the basic or extended profiles that they pass on to each possible match. (Appendix Figure D8 shows how we explain this choice to participants). Participants with the option to hide politics choose whether to do so or not separately for each possible match alongside the choice to pass on a basic or extended profile. Second, while the original WTP experiment randomized some participants to see profiles of possible matches without information about their climate beliefs, all participants in the round-two WTP experiment see profiles that include matches' climate beliefs.

**Recruitment and attrition:** We recruit participants for the round-two WTP experiment via ads on Facebook, inviting participants to take the additional WTP survey section if they report that they emailed Congress (Appendix D.5.1 details recruitment.) 1,420 participants start the round-two WTP survey in total. Midway through explaining the WTP experimental set-up, we randomize the 1,278 remaining participants to either have or not have the option to hide politics. A total of 995 participants (70% of those who start the experiment and 78% of those randomized) complete all 20 binary choices, with no differential attrition by treatment arm (column 3, Appendix Table C1).

**Sample description and balance:** Like the main WTP sample, round-two WTP participants are more concerned about climate change and politically engaged than the main Wave-1 sample (column 12, Appendix Table A1). The round-two WTP sample is older and somewhat less likely to identify as women than the main WTP sample, but it matches this sample in climate beliefs, political beliefs, and political engagement (column 13, Appendix Table A1). The sample is fully balanced across treatment arms (Appendix Table D7).

**Set-up comprehension:** As in the main WTP experiment, comprehension was high. We ask and correct participants' answers to the same 8 comprehension questions administered in the main WTP experiment; participants initially answer 5.7 of these correctly, compared to 5.8 in the main WTP sample. (Appendix Figure D9 shows the full distribution of comprehension performance.)

98% of those with the option to hide their politics correctly answer an additional comprehension question asking which dimension of their demographics they could choose to exclude.

#### *6.3.2 Results: The role of anticipated affective polarization*

Democrats' choices in the round-two WTP experiment suggests that *anticipated* affective polarization plays a key role in the $\Delta P(Email)$ 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 8a). 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 suggests that Democrats strategically hide their politics in order to better mobilize Republicans' action.

**Gaps in influence beliefs ($\Delta P(Email)$):** Indeed, Democrats believe that their invitations will have much higher impact on Republicans' action if they hide their own political leanings. At the end of the Round-2 WTP survey, we randomize half of participants to estimate the probability that two of their possible matches would email Congress if they did or did not see an invitation (Figure 1b); we randomize whether participants make these predictions for invitations that do or do not include their own political leanings.

Democrats with the option to hide their politics estimate that an invitation that hid their politics would make Republicans 11.4pp more likely to email Congress, while an invitation identifying them as a Democrats would increase Republicans' action by only 3.4pp (Figure 8b). On the other hand, they estimate that hiding their political leanings would somewhat decrease the impact of invitations on Democrats' action from 13.3pp to 9.8pp.[43] These patterns suggest that second-order affective polarization plays an important role in Democrats' beliefs about their ability to spread the climate movement across the political aisle: they expect Republicans to respond less to their invitations precisely because they are Democrats. Indeed, Democrats estimate that hiding their political leanings would close the gap in the impact of invitations on Democrats and Republicans.

In theory, Democrats might expect invitations that hid their politics to more strongly affect Republicans' action because they expect Republicans to rationally respond more to the signal of

[43] In contrast, we do not find this large belief gap among participants who were not randomly given the option to hide their politics, who are more optimistic about the impacts of baseline profiles on Republicans (Appendix Figure D10). This divergence could arise for several reasons. Participants with the option to hide their politics may formulate more precise and differentiated beliefs about the impacts of profiles with and without political leanings over the course of the binary WTP choices, or those without the option to hide politics may overstate the impacts of baseline invitations via motivated reasoning,

an invitation from someone with whom they expect to share more baseline 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 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 political party, while only 19% mention rational beliefs.[44]

## 7 Conclusion

In a large online experiment with over 21,000 participants, we create opportunities for Democrats to invite Americans across the country, demographic groups, and the political spectrum to join them in political climate advocacy. Democrats are motivated to engage others in climate action, but they are much more likely to reach out to other Democrats than to potential allies across the political aisle. Even as affective polarization reaches record highs, however, we find no evidence that this gap arises from a distaste for cooperating with counter-partisans: when emails can be obtained with certainty from different groups, Democrats target action to more strategically-valuable states, with no preference for letters from Democrats or Republicans per se. Rather, our results suggest that Democrats strategically target co-partisans for action because they believe that they can more strongly affect their action. Nonetheless, the broader context of strong affective polarization still plays a key role. *Anticipated* affective polarization drives Democrats' differential influence beliefs: they estimate that hiding their politics from invitations to Republicans would make them more than three times as effective.

Unified bipartisan movement at the grassroots may then be unlikely to form, either on climate change or other issues. 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 segregated 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. 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 second-order polarization beliefs. For example, a growing literature in political science suggests that cross-party conversations reduce both own partisan dislike and

[44] An 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.

second-order affective polarization, especially when they focus on non-political common ground (Santoro and Broockman, 2022; Fang et al., 2023; Rossiter, 2023). 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).

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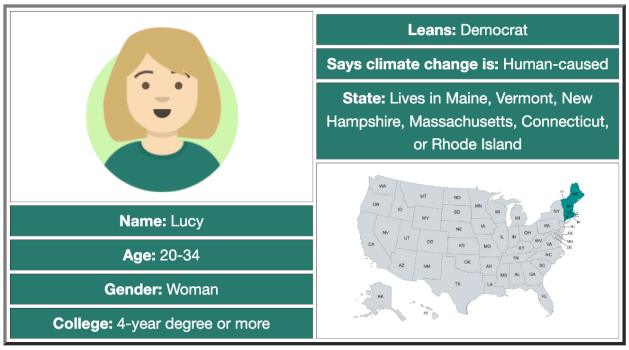
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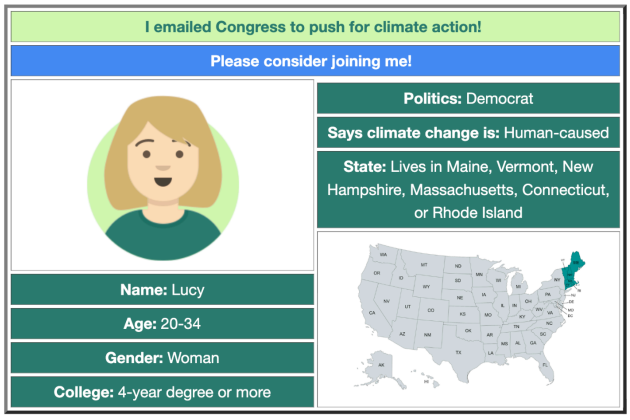
# 8 Main-text figures

Figure 1: Demographic profiles and invitations to act

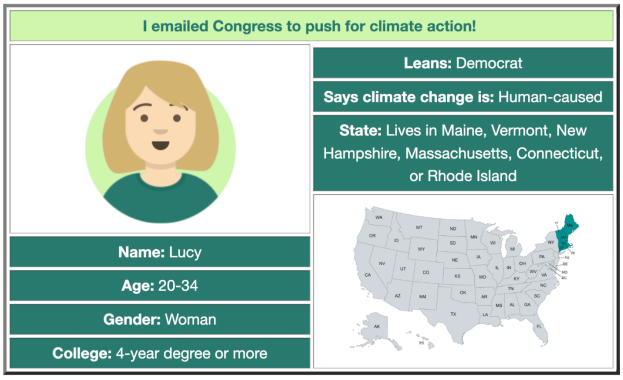
(a) Sample basic demographic profile




(b) Sample invitation for Wave-2 participants

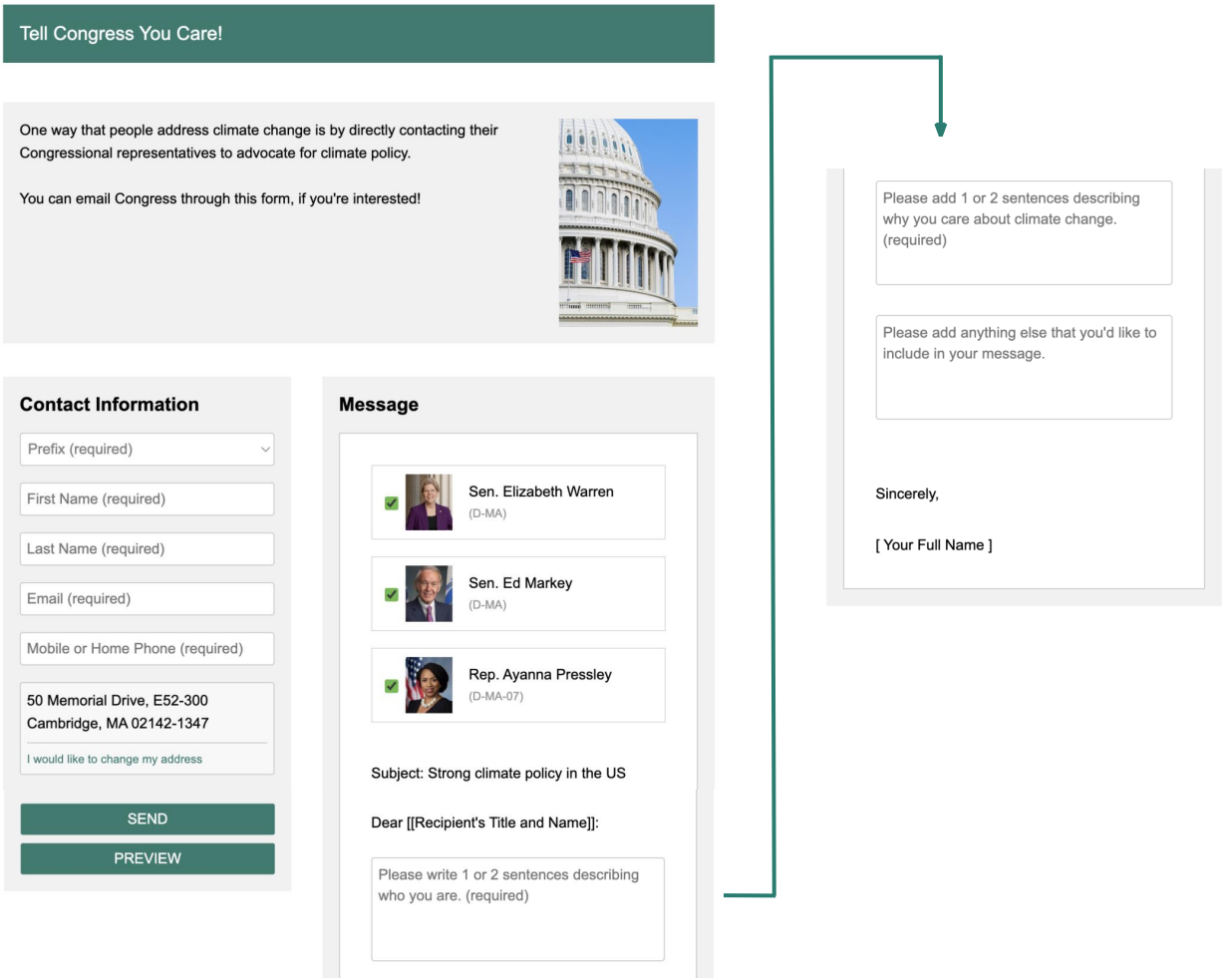



(c) Sample profile saying emailed Congress



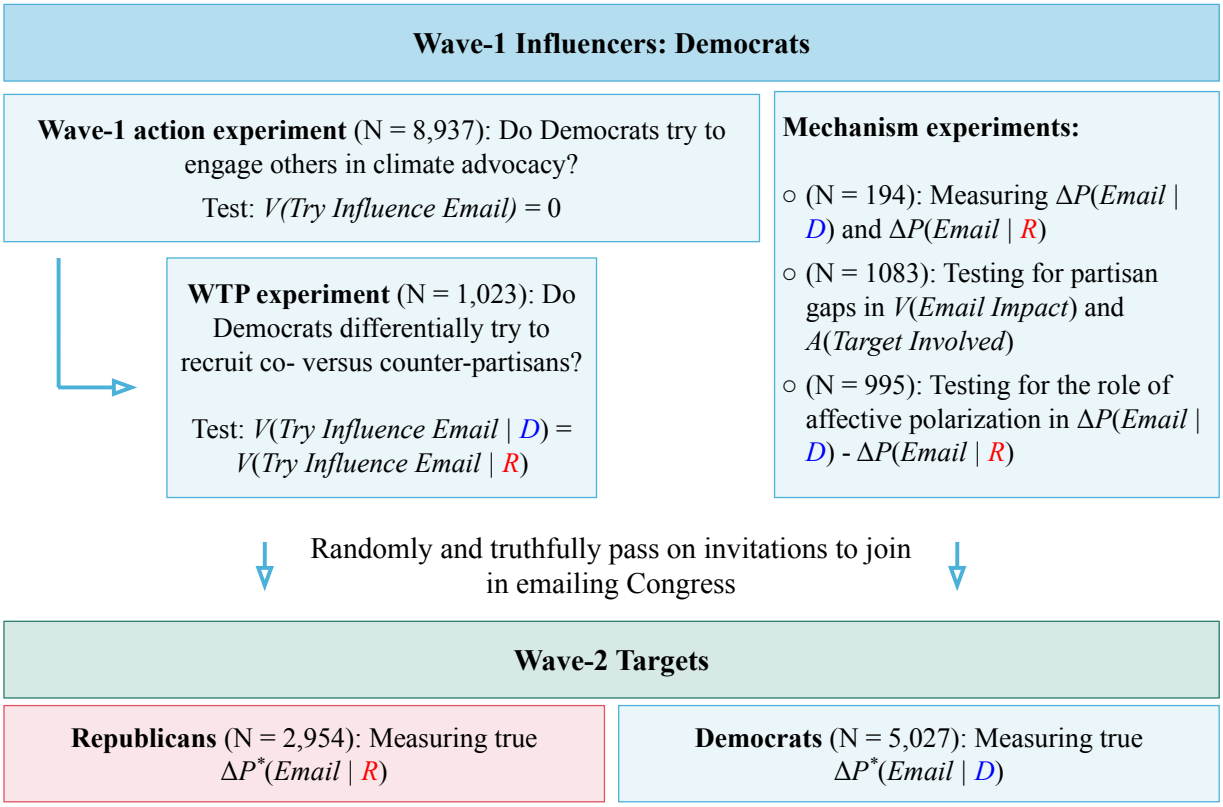

Note: Demographic profiles like those in sub-figure (a) form the basis of all interactions between Wave-1 and Wave-2 participants. Across several experimental designs, Democratic influencers can send future participants either invitations to email Congress (sub-figure (b)) or profiles that say that they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

Figure 2: The measure of climate action: Emailing Congress



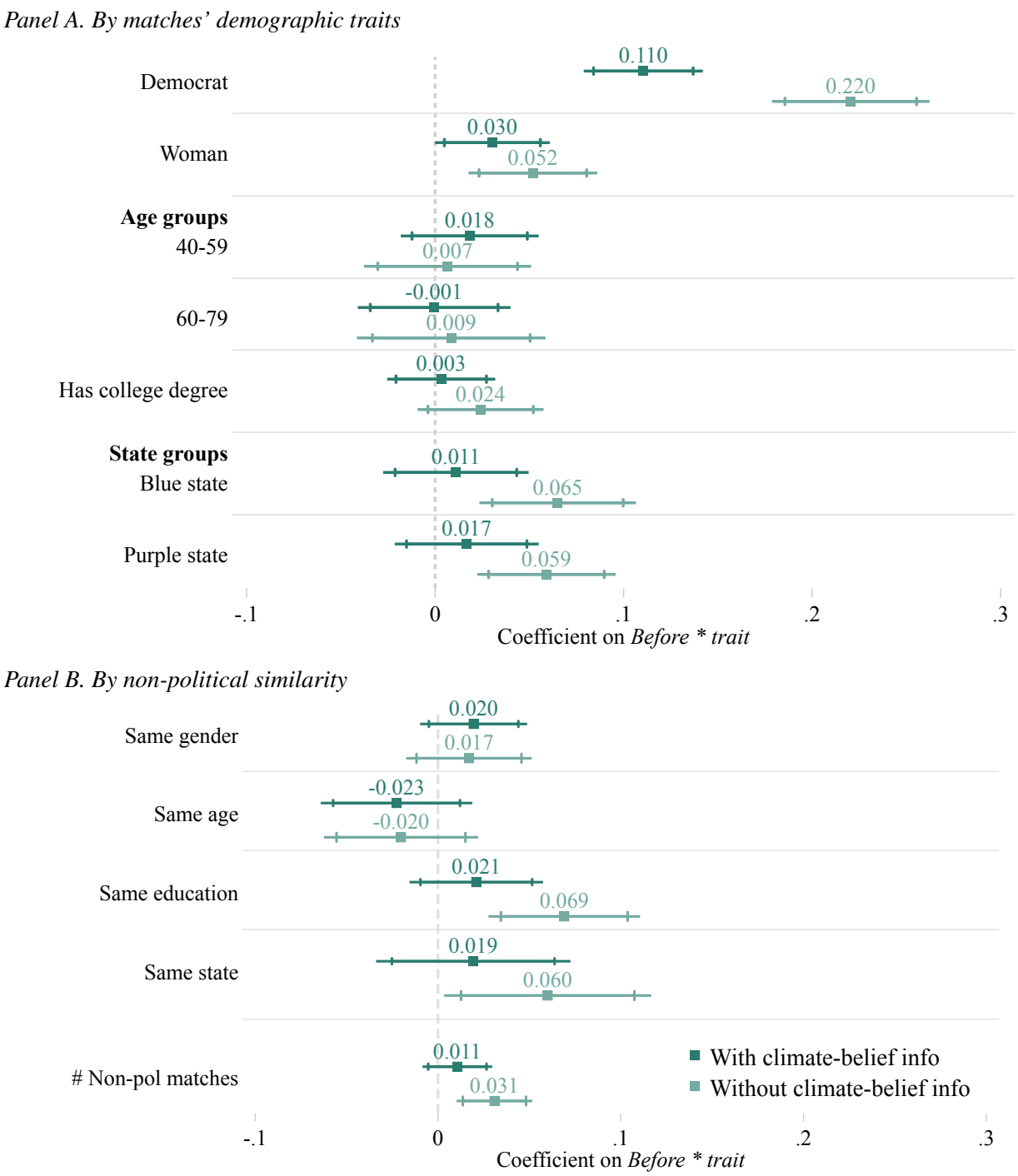

Note: This figure shows the form that we license and through which participants can email Congress about climate change. Note that the form is almost fully customizable, except that it includes a fixed subject line supporting climate action. We randomize this subject line across participants.

Figure 3: An overhead view of the project design




Note: This figure provides an overview of the interlocking experiments that contribute to this project. The experiments rest on a network connecting two waves of participants: a Wave 1 of Democrats who have oppportunities to email Congress and and to invite Wave-2 targets to join them in doing so. Wave-2 participants are split include both 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.

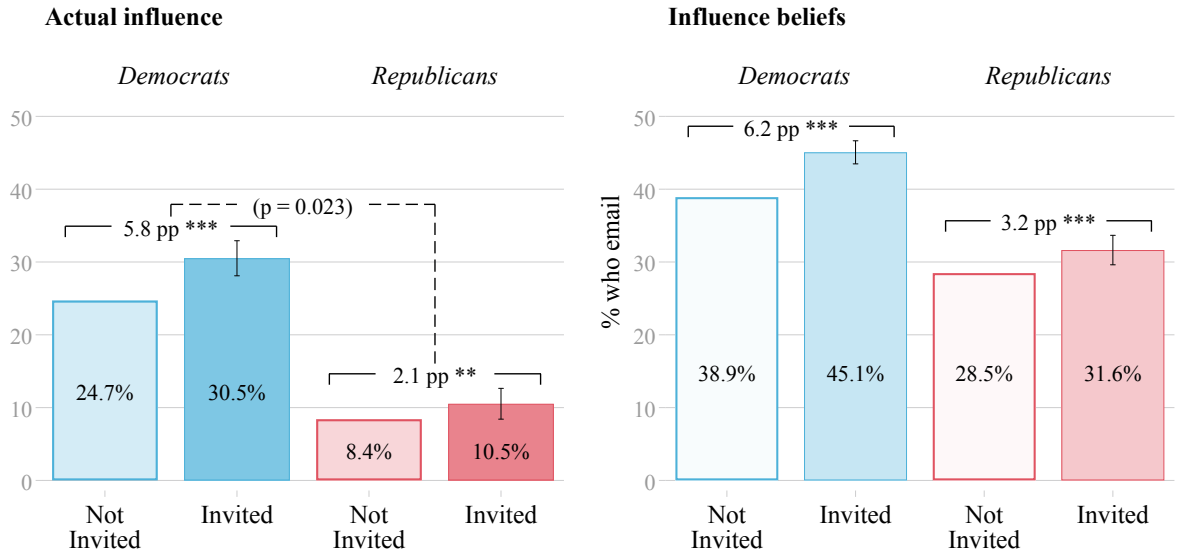
Figure 4: Differential WTP influence attempts by other match features



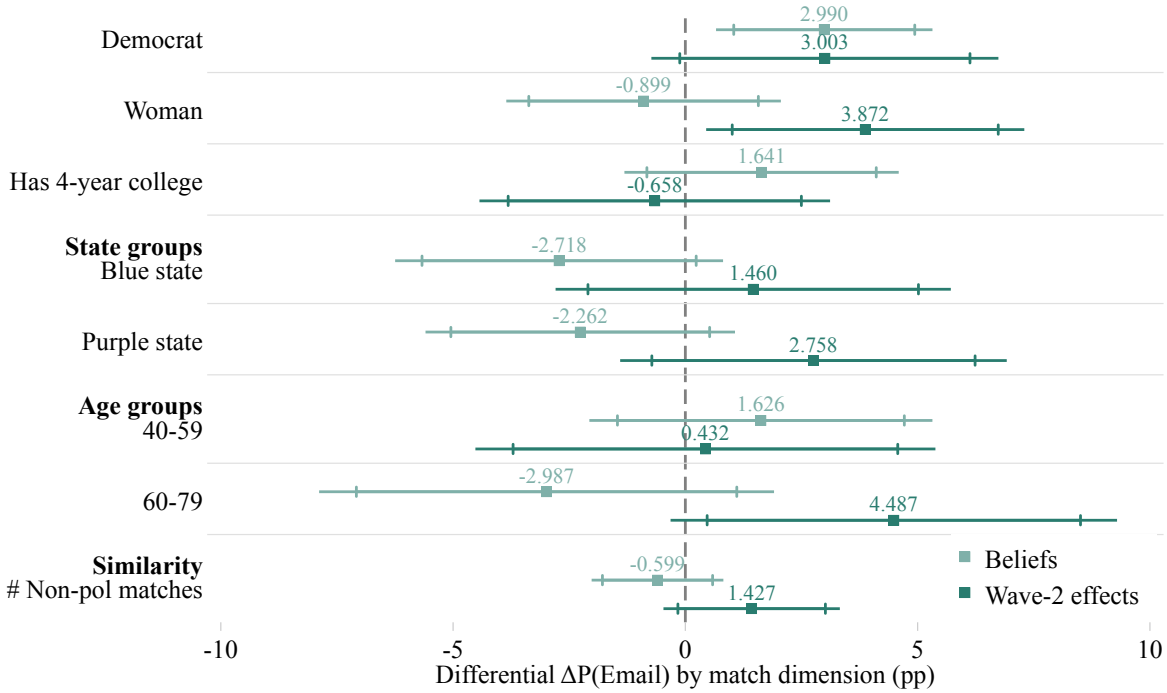

Note: Panel A tests whether WTP participants differentially seek to mobilize possible matches with certain demographic traits, estimated in a variant of equation 6 in which we jointly include indicators for each trait and their interactions with $Before_{ij}$. The omitted categories for age and state groups are ages 25-39 and Red states, respectively. Panel B tests whether WTP 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 6 in which we interact $Before_{ij}$ with the total number of non-political traits that a WTP participant and a given possible match share. Both of these similarity regressions also include an indicator that the match is a Democrat and its interaction with $Before_{ij}$; the coefficients on these interactions are quite close to that in Panel A. 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: Overall influence beliefs by party affiliation, compared with the truth

**Panel A.** Participant-estimated and true $\Delta P(Email)$ by party affiliation

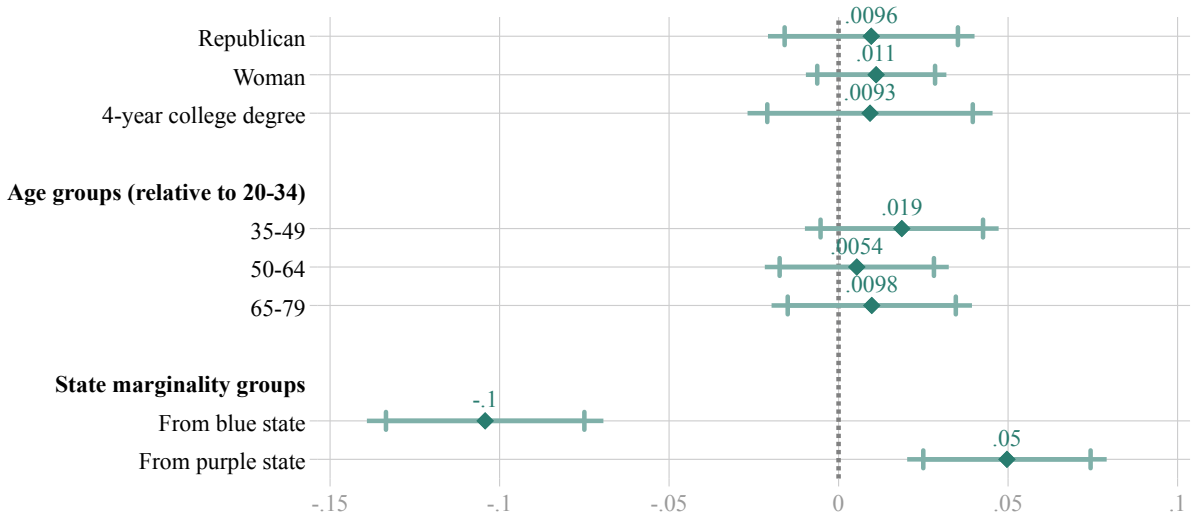



**Panel B.**




Note: The left panel of this figure plots average incentivized beliefs about the share of liberal and conservative Wave-2 participants who would email Congress if they do or do not see invitations from Wave-1 participants to join in action. We estimate these effects among 194 participants in the "belief sample," each of whom predict the share of liberal and conservative participants in their matched Wave-2 group who would email Congress if they did or did not see a Wave-1 invitation. The figure reports participants' beliefs for the impact of invitations separately among liberals and conservatives, as well as the heteroskedasticity-robust p-value from a test of the null that participants estimate the same imapct across these groups. The right panel of this figure plots the actual impacts of invitations on whether Wave-2 liberals and conservatives match to email records. We estimate these impacts among 5,027 liberals and 2,954 conservatives in the Wave-2 experimental sample (Section 6.1). As for beliefs, the figure reports the heteroskedasticity-robust p-value from a test of the null that invitations have the same true impact on liberals and conservatives. Across the true and esitmated impacts of invitations on action, we indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

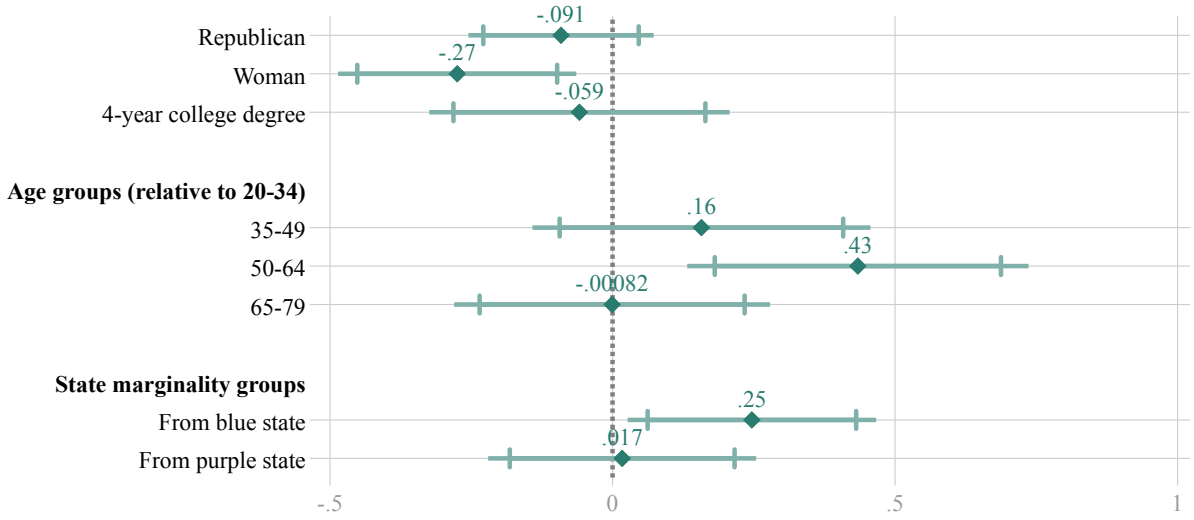
Figure 6: Determinants of choosing emails with certainty over carbon offsets



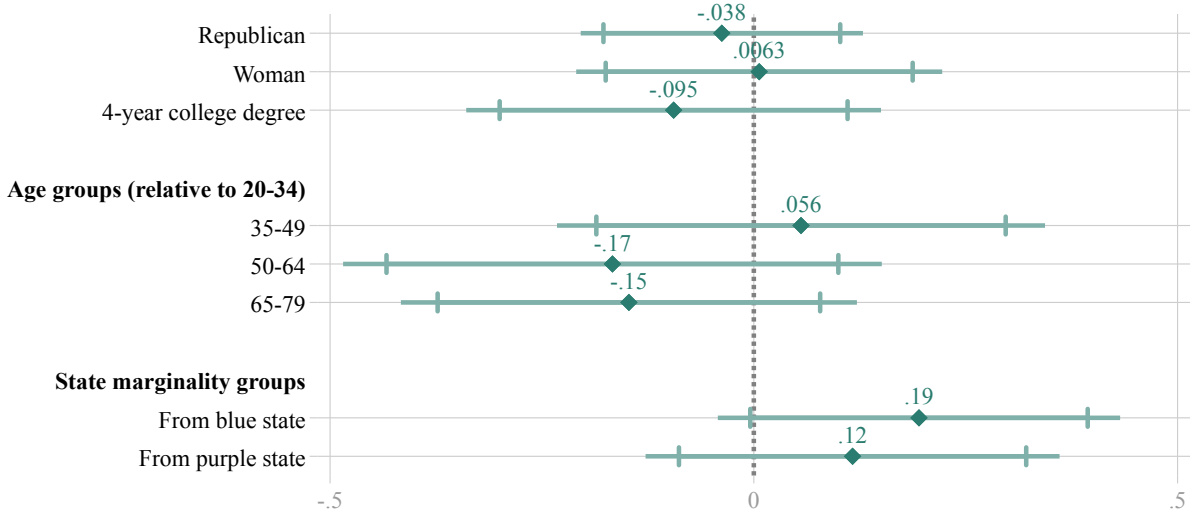

Note: In this figure, we regress whether participants choose an email enlisted with certainty from a given demographic group (instead of a $10 carbon-offset donation) on email-letter demographic traits. This regression includes 8,036 choices across 574 participants. In addition to estimating the role of the demographics shown above, we also control for indicators for choice number. We estimate the role of email-writer age groups relative to being between 20 and 34, and we estimate the role of email-writer state relative to living in a red state group. The capped and uncapped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively.

Figure 7: Perceived impact of emails on climate-policy support

**Panel A.** Whether would read email



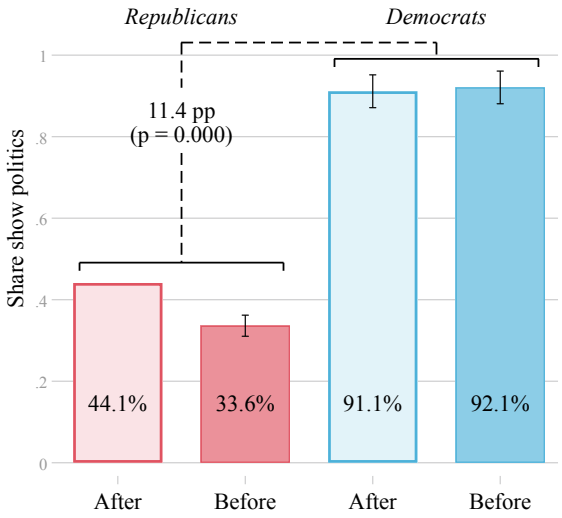

**Panel B.** Impact of emails on climate-policy support, conditional on reading



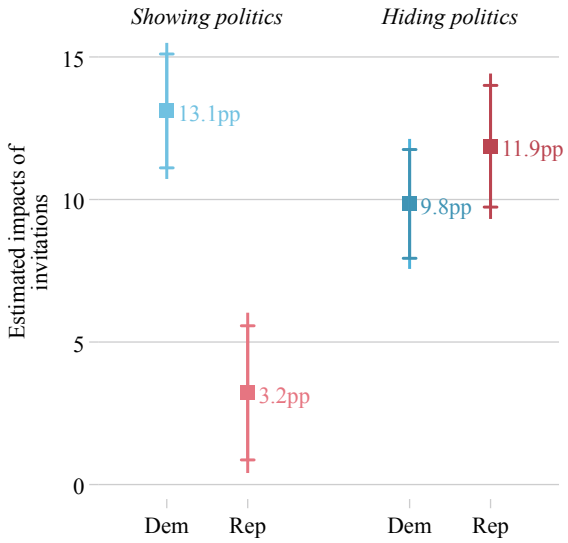

Note: In this figure, we regress two measures of email impact on email-writer traits. In Panel A, we ask participants to imagine that a climate bill were introduced to Congress in November 2023 and that someone in this group emailed their national representatives in Congress about the bill via our form. We then ask, "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). In Panel B, we ask participants to assume that 20 people in this demographic group emailed Congress about the 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 certainty-experiment participants, each of whom provides these responses for one randomly-chosen demographic group (N = 557). In addition to estimating the role of the demographics shown above, we also control for indicators for choice number. We estimate the role of email-writer age groups relative to being between 20 and 34, and we estimate the role of email-writer state relative to living in a red state group. The capped and uncapped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively.

Figure 8: Main results from round-two WTP experiment

(a) Choices to show politics




(b) Participants' estimates for invitation impacts with and without politics




Note: Subfigure (a) summarizes round-two WTP participants' choices to include their political leanings in profiles shown to possible matches; we restrict here to the 490 participants who are randomized to have the option to hide their politics, each of whom makes 20 choices. We simply regress an indicator that participant $i$ chooses to include their political leanings in a profile shown to possible match $j$ on interacted indicators that participant $j$ is a liberal and would see their profile before deciding whether to email Congress or not, WTP participant fixed effects, and indicators for binary choice number from 1 through 20. We cluster standard errors by WTP participant. Subfigure (b) plots participants' estimates for the impact of invitations that did or did not show that they are Democrats on the probability that a Democratic or Republican recipient would email Congress. 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 WTP participant; we randomize across participants whether this invitation would show that they are a Democrat. Then we 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 WTP participant.

# 9 Main-text tables

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

| | (1) | (2) | (3) |
|---|---|---|---|
| | Wave-1: | Wave-2: | |
| | Democrats | Democrats | Republicans |
| Woman | 0.638 | 0.564 | 0.520 |
| Hispanic | 0.026 | 0.055 | 0.101 |
| Has $\geq$ 4-year college degree | 0.826 | 0.758 | 0.361 |
| **Age ranges:** | | | |
| 20-39 | 0.114 | 0.213 | 0.426 |
| 40-59 | 0.341 | 0.327 | 0.318 |
| 60-79 | 0.545 | 0.460 | 0.255 |
| **Income bins (USD):** | | | |
| Less than 50,000 | 0.167 | 0.232 | 0.397 |
| 50,000-99,999 | 0.321 | 0.323 | 0.377 |
| 100,000-149,999 | 0.234 | 0.221 | 0.136 |
| 150,000-199,999 | 0.130 | 0.110 | 0.051 |
| 200,000 or more | 0.148 | 0.114 | 0.039 |
| **Residence by state marginality:** | | | |
| Red state | 0.236 | 0.270 | 0.413 |
| Blue state | 0.441 | 0.418 | 0.257 |
| Purple state | 0.322 | 0.312 | 0.330 |
| **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 baseline each 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. Appendix B.2.2 describes each of the baseline traits shown in more detail. 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 5 and Appendix A.1 detail why and how we screen participants by politics, climate beliefs, and these demographic traits.

Table 2: Main Wave-1 results

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

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

Table 3: Main WTP results

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Showed extended profile | | | |
| | *No climate beliefs* | | *Sees climate beliefs* | |
| Before | 0.416*** | 0.296*** | 0.481*** | 0.424*** |
| | (0.020) | (0.021) | (0.019) | (0.020) |
| Democrat | | 0.135*** | | 0.037*** |
| | | (0.016) | | (0.014) |
| Before * Democrat | | 0.239*** | | 0.115*** |
| | | (0.019) | | (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: This table reports the main results from the WTP experiment. Across columns, the outcome variable is whether participants choose to pass on an extended profile saying that they emailed Congress, rather than passing on a basic demographic profile and delegating a small carbon-offset donation from our research funding, to a returning participant with whom they could be matched. Columns 1 and 2 analyze the sample of WTP participants who are assigned to see profiles of possible matches that do not include information on their beliefs about climate change, while columns 3 and 4 analyze participants who see profiles including that all participants believe climate change is mostly human-caused. In both samples, we restrict to participants who make binary choices for all 20 possible matches. Columns 1 and 3 test whether WTP participants are more or less likely overall to tell a possible match that they emailed Congress when that match would see their profile before deciding whether to email Congress or not; in these columns, the table footer includes the share of cases in which WTP participants choose to show an extended profile to any match who would see their profile after deciding whether to email Congress or not. Columns 2 and 4 then test whether this "Before" effect differs significantly when matches are liberal versus conservative; the table footer in these columns includes the share of cases in which WTP participants choose extended profiles for liberal who would see their profile after deciding whether to email Congress or not. All regressions control for WTP-participant fixed effects and binary choice number, from 1 to 20. We present heteroskedasticity-robust standard errors in parentheses and indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

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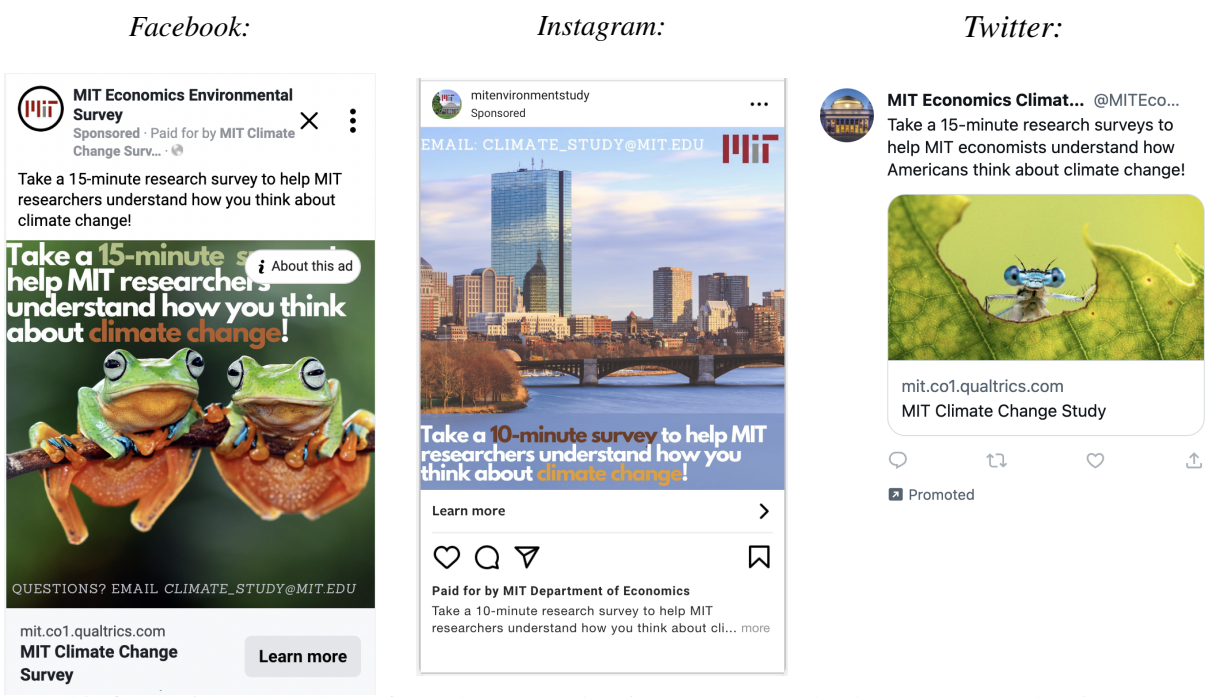
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## A Details of the experimental context

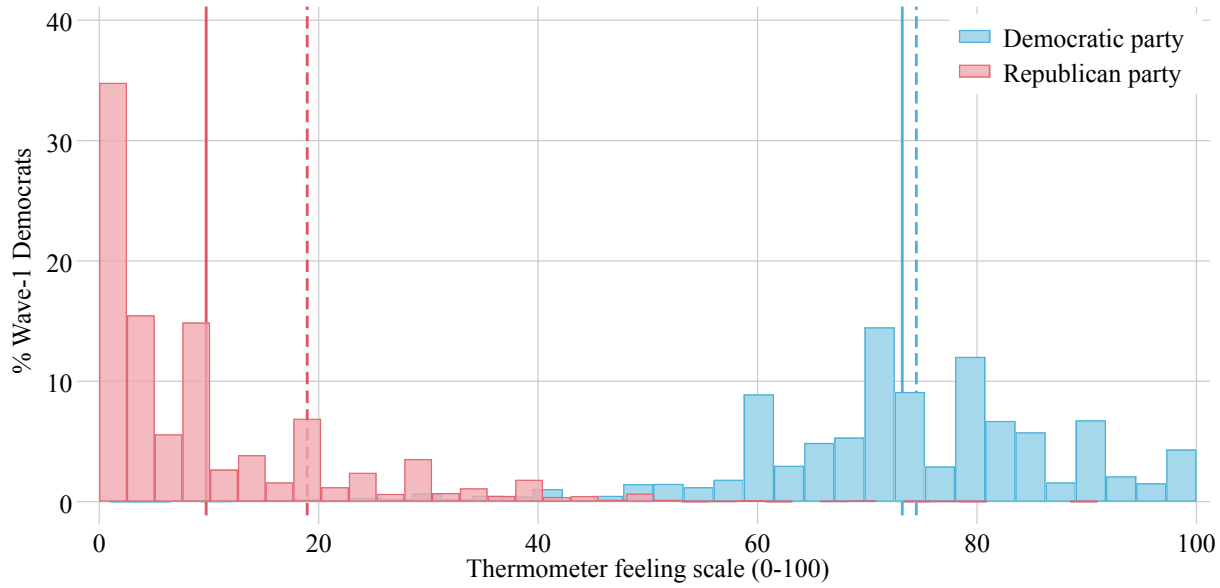
Figure A1: Sample social-media advertisements



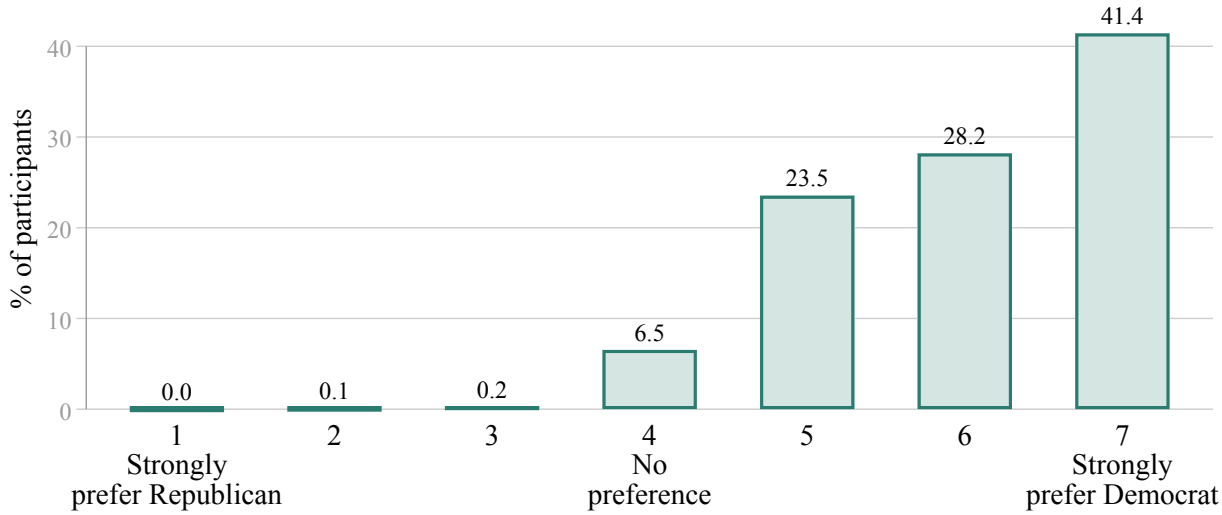

Note: This figure gives screenshots of sample survey advertisements on Facebook, Instagram, and Twitter. We used similar ads for all study recruitment except for Wave-2 participants who were recruited via Qualtrics. The stated survey length and other ad text varied slightly depending on the survey for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

Figure A2: Wave-1 Democrats' baseline affective polarization

**Panel A.** Affective polarization on the ANES "feelings thermometer"

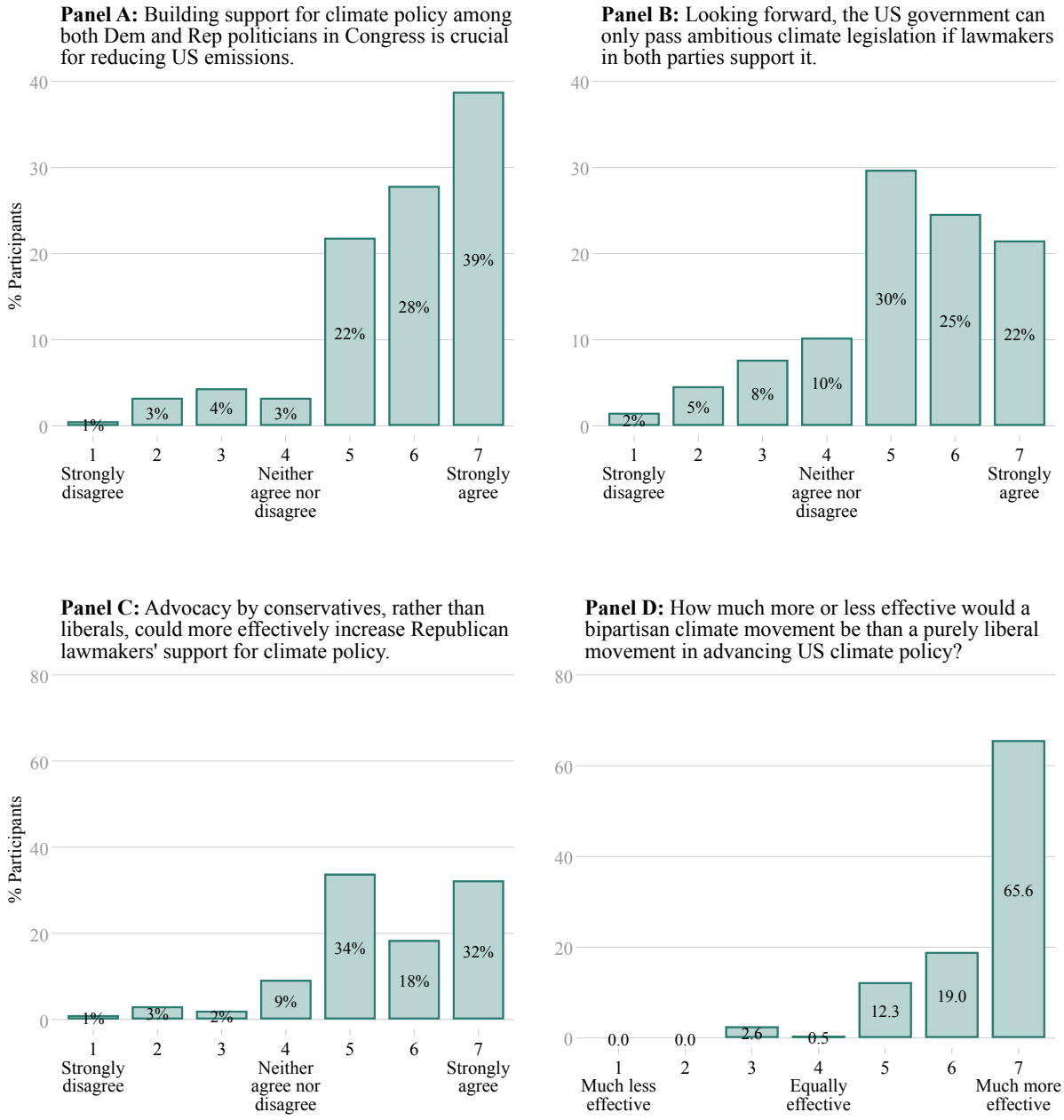



**Panel B.** Democrats strongly prefer friends of their own party



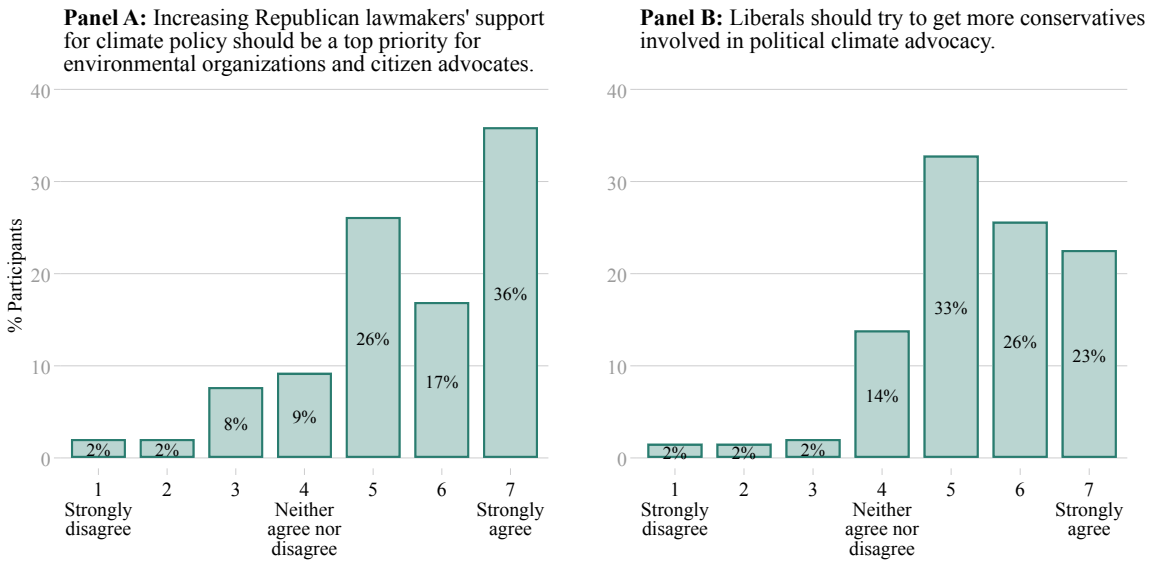

Note: Panel A measures affective polarization using the "feelings thermometer" question from the American National Election Study (ANES), in which participants report how warmly they feel toward the Democratic party and the Republican party on scales from 0 degrees (cold) to 100 degrees (warm). We elicit 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 elicited these values after participants choose whether to email Congress or not, yielding N = 4,321. The solid lines give average warmth towards the Democratic and Republican parties in our sample, while the dashed lines give average warmth values among Democrats in the 2020 ANES sample. The ANES identifies Democrats as those who say that generally speaking, they think of themselves as Democrats, while our sample includes 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.

Figure A3: Democrats think bipartisan climate action would be more effective



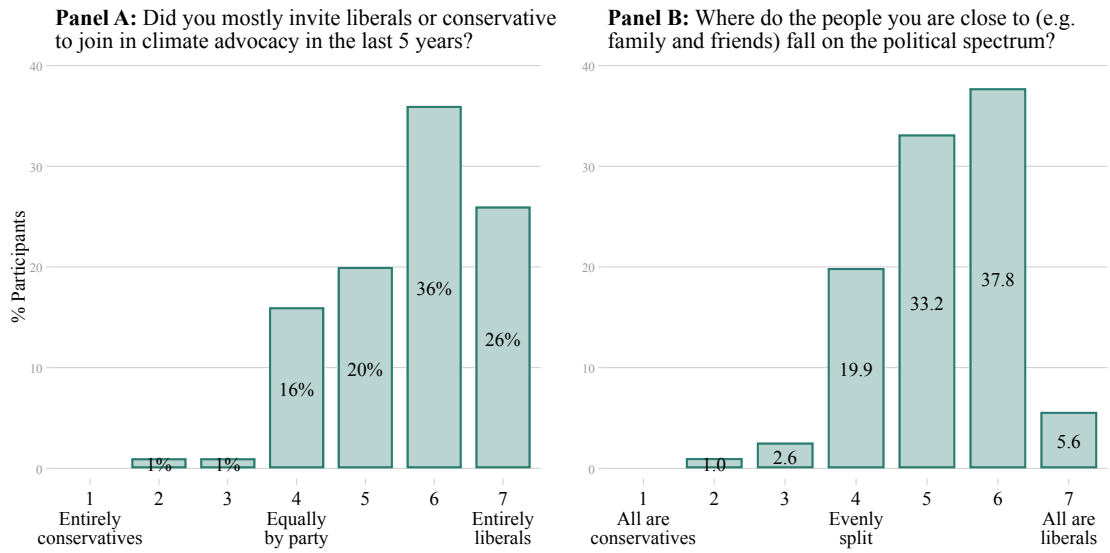

Note: This figure plots responses in a motivating sample of Democrats, who were recruited subject to the same eligibility restrictions as the Wave-1 experimental sample (Section 2.1). Panels A, B, and C plots responses to how much participants agree with the statements in the subfigure titles, while 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. We define a bipartisan climate movement to participants as one that includes both liberals and conservatives advocating for policy change, where we define liberals and conservatives as people who lean towards to or belong to the Democratic or Republican Parties, respectively.

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




Note: The 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. We define liberals and conservatives as people who lean towards to or belong to the Democratic or Republican Parties, respectively. N = 195 in both figures.

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




Note: Panel A restricts to Democrats who said that they have invited someone to join in political climate advocacy in the last 5 years via direct messages or in-person or phone conversations (N = 100). Panel B plots responses in the full motivating sample of Democrats (N = 196). Participants choose from the following: "All of them are [liberals/conservatives];" "Almost all of them are [liberals/conservatives], but I know a few [conservatives/liberals];" "Most of them are [liberals/conservatives];" "They are evenly split across the political spectrum." We convert these responses into a 7-point scale as shown in the figure.

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

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | Motivating sample | | Belief sample | | | Certainty sample | | | WTP Round 2 | | |
| | Wave-1 | WTP | Mean | Δ Wave1 | Mean | Δ Wave1 | Δ WTP | Mean | Δ Wave1 | Δ WTP | Mean | Δ Wave1 | Δ WTP |
| Woman | 0.632 | 0.761 | 0.853 | 0.221*** | 0.587 | -0.045* | -0.174*** | 0.841 | 0.209*** | 0.081*** | 0.677 | 0.045*** | -0.083*** |
| Hispanic | 0.027 | 0.039 | 0.015 | -0.011 | 0.028 | 0.001 | -0.011 | 0.024 | -0.002 | -0.015* | 0.025 | -0.001 | -0.014* |
| Has $\geq$ 4-year college degree | 0.828 | 0.862 | 0.817 | -0.010 | 0.854 | 0.026 | -0.008 | 0.871 | 0.043*** | 0.009 | 0.845 | 0.018 | -0.017 |
| **Age ranges:** | | | | | | | | | | | | | |
| 20-39 | 0.119 | 0.146 | 0.096 | -0.023 | 0.264 | 0.145*** | 0.119*** | 0.122 | 0.002 | -0.024 | 0.055 | -0.064*** | -0.090*** |
| 40-59 | 0.337 | 0.354 | 0.396 | 0.059 | 0.385 | 0.049* | 0.032 | 0.460 | 0.123*** | 0.106*** | 0.264 | -0.072*** | -0.090*** |
| 60-79 | 0.544 | 0.500 | 0.508 | -0.036 | 0.350 | -0.194*** | -0.150*** | 0.418 | -0.126*** | -0.082*** | 0.680 | 0.136*** | 0.180*** |
| **Income bins (USD):** | | | | | | | | | | | | | |
| Less than 50,000 | 0.170 | 0.166 | 0.193 | 0.023 | 0.113 | -0.057*** | -0.053*** | 0.167 | -0.003 | 0.001 | 0.172 | 0.002 | 0.006 |
| 50,000-99,999 | 0.325 | 0.338 | 0.371 | 0.045 | 0.229 | -0.096*** | -0.109*** | 0.310 | -0.015 | -0.028 | 0.350 | 0.024 | 0.012 |
| 100,000-149,999 | 0.231 | 0.238 | 0.147 | -0.083*** | 0.300 | 0.069*** | 0.062** | 0.216 | -0.015 | -0.022 | 0.234 | 0.004 | -0.003 |
| 150,000-199,999 | 0.126 | 0.118 | 0.117 | -0.009 | 0.181 | 0.055*** | 0.063*** | 0.132 | 0.006 | 0.014 | 0.117 | -0.010 | -0.002 |
| 200,000 or more | 0.147 | 0.140 | 0.173 | 0.025 | 0.176 | 0.029 | 0.037* | 0.174 | 0.027 | 0.034* | 0.128 | -0.020* | -0.012 |
| **State marginality:** | | | | | | | | | | | | | |
| Red state | 0.231 | 0.220 | 0.213 | -0.017 | 0.227 | -0.004 | 0.007 | 0.240 | 0.010 | 0.020 | 0.243 | 0.013 | 0.023 |
| Blue state | 0.438 | 0.457 | 0.386 | -0.052 | 0.481 | 0.043* | 0.025 | 0.439 | 0.001 | -0.017 | 0.454 | 0.016 | -0.002 |
| Purple state | 0.331 | 0.324 | 0.401 | 0.070* | 0.292 | -0.039 | -0.031 | 0.321 | -0.011 | -0.003 | 0.303 | -0.029* | -0.021 |
| **Climate beliefs:** | | | | | | | | | | | | | |
| Climate worry (1-7) | 6.430 | 6.547 | 6.513 | 0.083 | 6.395 | -0.034 | -0.152*** | 6.528 | 0.098*** | -0.020 | 6.605 | 0.175*** | 0.058* |
| Desire for climate action (1-7) | 6.735 | 6.823 | 6.782 | 0.047 | 6.725 | -0.009 | -0.098*** | 6.774 | 0.039* | -0.050* | 6.809 | 0.075*** | -0.014 |
| Perceived local impacts (1-7) | 5.496 | 5.576 | 5.426 | -0.070 | 5.479 | -0.018 | -0.097* | 5.495 | -0.001 | -0.081 | 5.610 | 0.114*** | 0.034 |
| **Political engage. and beliefs:** | | | | | | | | | | | | | |
| Prev. contacted reps | 0.733 | 0.803 | 0.777 | 0.044 | 0.703 | -0.030 | -0.100*** | 0.758 | 0.025 | -0.045** | 0.806 | 0.073*** | 0.003 |
| Prev. donated | 0.816 | 0.863 | 0.822 | 0.007 | 0.798 | -0.017 | -0.065*** | 0.829 | 0.014 | -0.034* | 0.841 | 0.026** | -0.022 |
| Prev. canvassed | 0.081 | 0.090 | 0.132 | 0.051** | 0.091 | 0.009 | 0.001 | 0.105 | 0.023 | 0.015 | 0.107 | 0.025** | 0.017 |
| Prev. signed petition | 0.835 | 0.880 | 0.878 | 0.044* | 0.816 | -0.019 | -0.064*** | 0.838 | 0.003 | -0.042** | 0.855 | 0.021 | -0.024 |
| Prev. phonebanked | 0.100 | 0.134 | 0.127 | 0.027 | 0.091 | -0.010 | -0.043** | 0.139 | 0.039*** | 0.005 | 0.143 | 0.042*** | 0.009 |
| Degree prefer Dem friends (1-7) | 6.040 | 6.072 | 6.020 | -0.020 | 6.033 | -0.008 | -0.040 | 6.070 | 0.029 | -0.003 | 6.005 | -0.035 | -0.067 |
| Sample size | 3616 | 1023 | 197 | | 397 | | | 574 | | | 995 | | |

Note: This figure compares the baseline traits of the main Wave-1 experimental sample (Section 4) and WTP sample (Section 5) with the motivating sample (Section 2.2), the influence-belief sample (Section 6.1.5), the certainty-experiment sample, (Section 6.2) and the round-2 WTP sample (Section 6.3). All traits are as in Table B2. Columns 4, 7, and 10 present different in means between each mechanism sample and the Wave-1 experimental sample (column 1), while columns 5, 8, and 11 compare each mechanism sample with the WTP sample. 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:

**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.

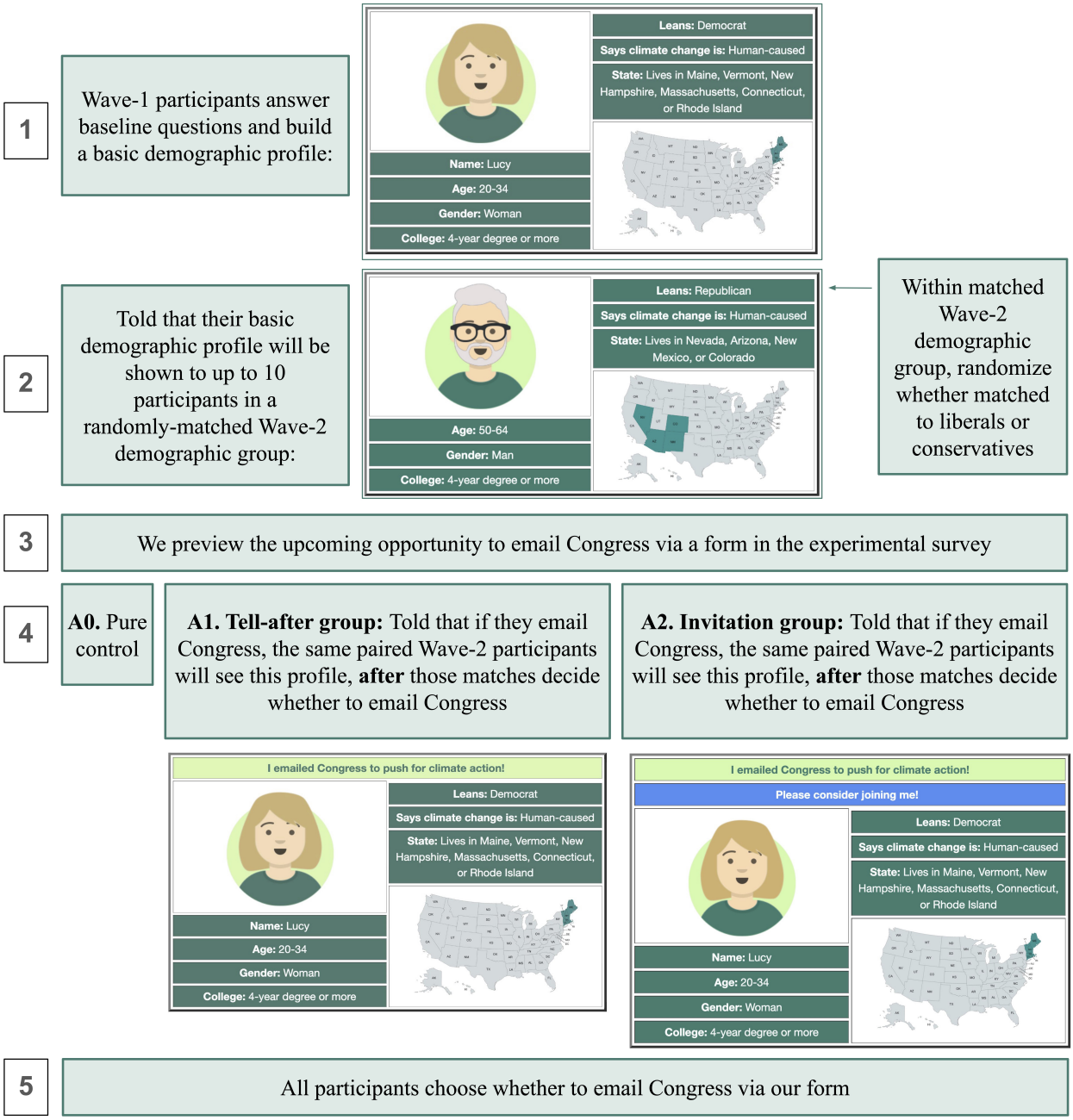
**Climate-belief screener:** We ask participants to choose whether they think: (1) Climate change is caused mostly by human activities; (2) Climate change is caused mostly by natural changes in the environment; (3) Neither, since climate change isn't happening; or (4) Other (with open-response field.)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

**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

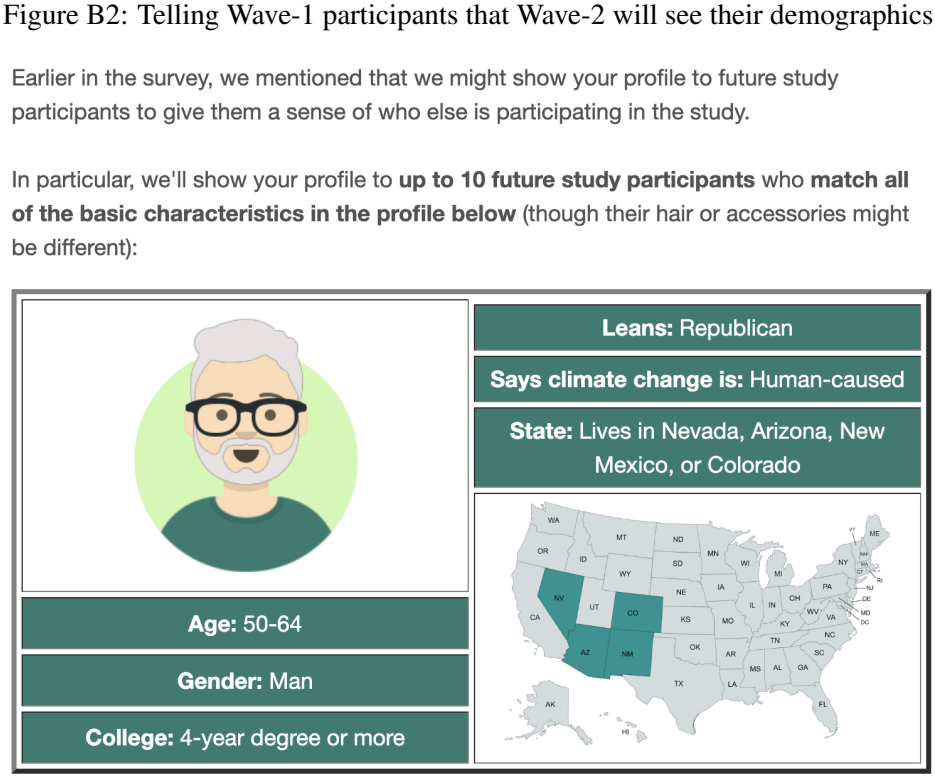
## B.1 Wave-1 additional exhibits

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 will see their demographics




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. 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.

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**.

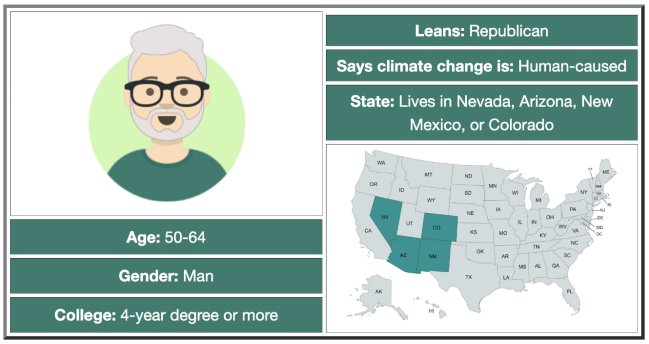
We will randomly choose **20 participants** to receive an extra **gift card worth $5** for each comprehension question they answer **correctly**, so please answer carefully!

**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.)

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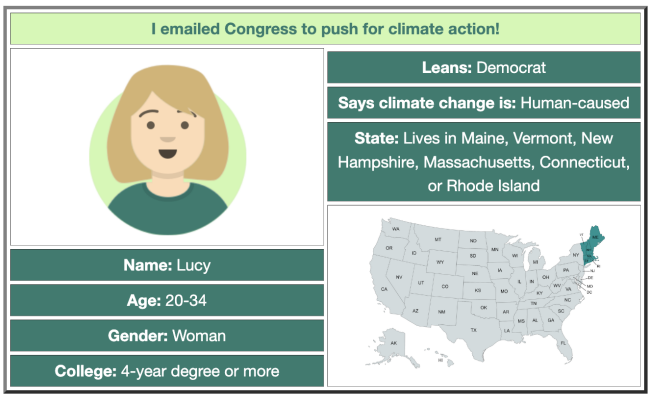
Remember that up to 10 participants in the demographic group below will see your own demographic profile when they take our survey.

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




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

The profile will say that you contacted Congress:

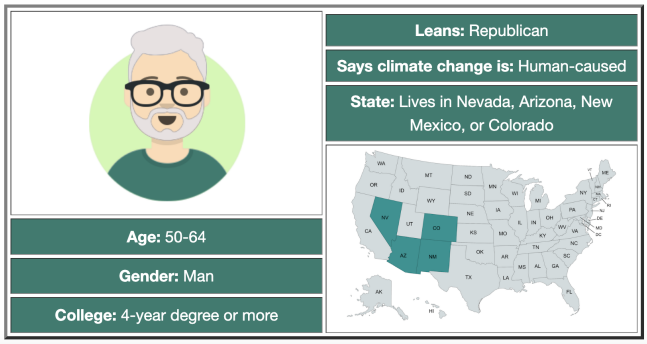



That means that they **won't know** you contacted Congress **when they decide** whether to do so or not.

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

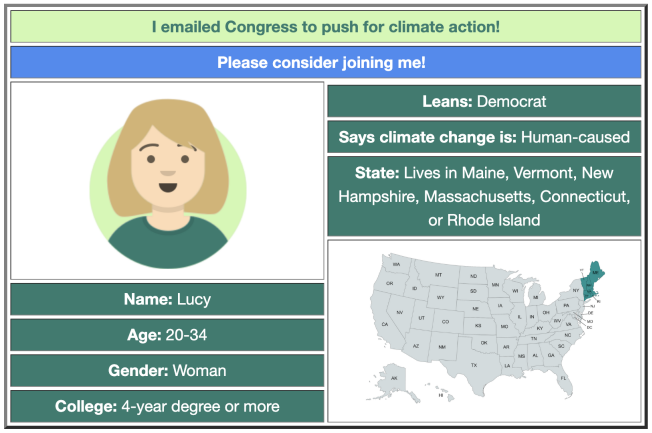
Remember that up to 10 participants in the demographic group below will see your own demographic profile when they take our survey.

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




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

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

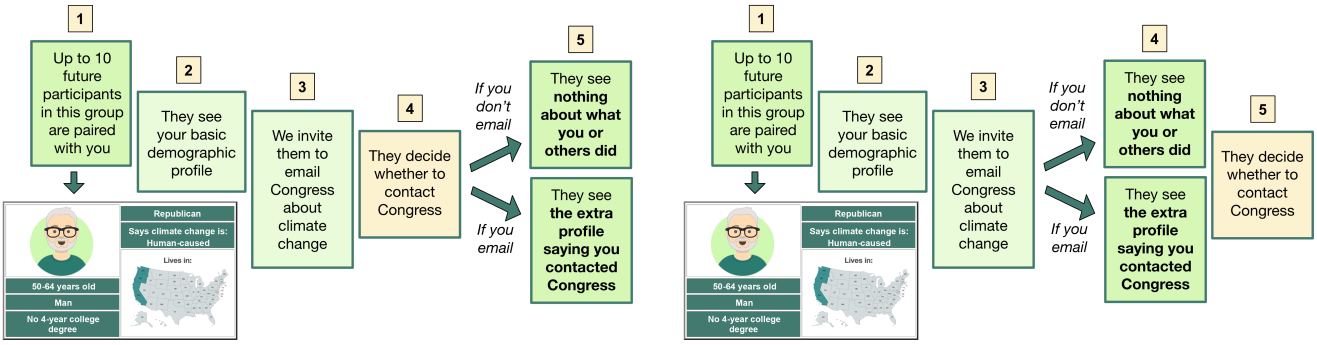



That means that they **will know** you contacted Congress **when they decide** whether to do so or not.

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

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 second profile would be their own.

Figure B4: Flowchart to reiterate profile timing




(a) For those in the Tell-after group

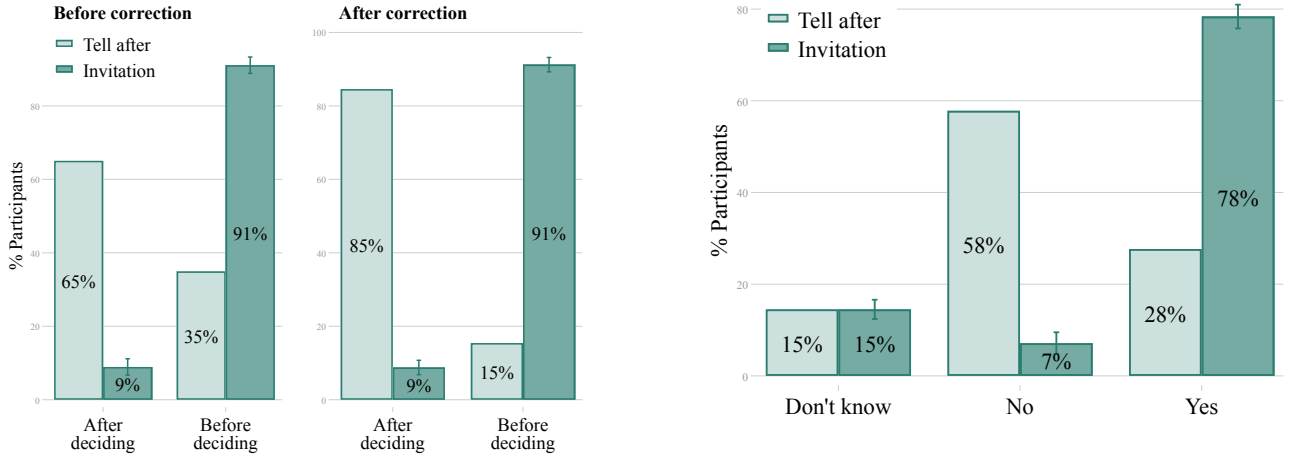
(b) *For those in the Invitation group*

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?

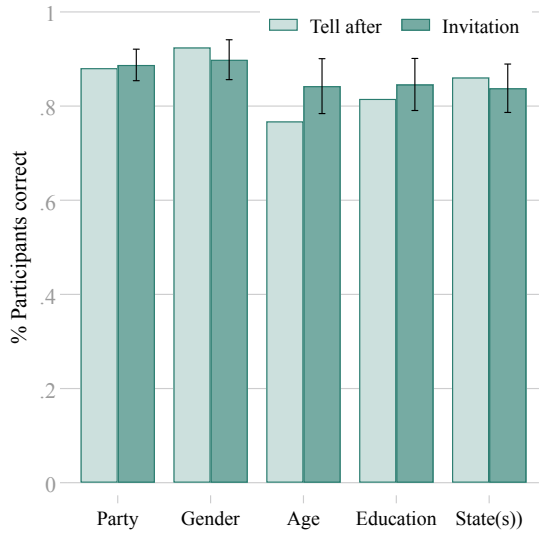
(b) In theory, could your choice to email Congress or not affect whether paired Wave-2 participants do so?



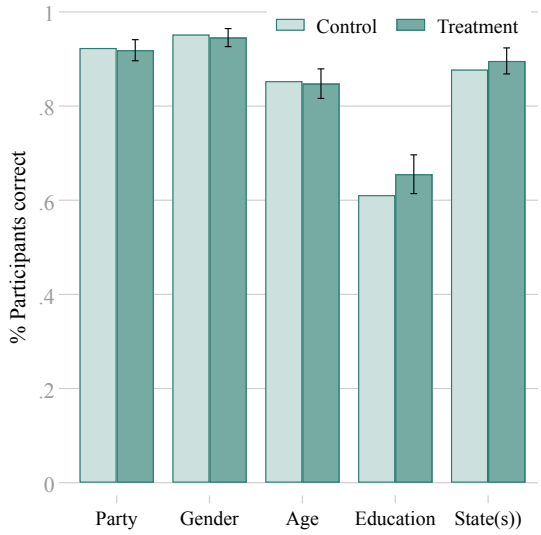

Note: After implementing the Invitation and Tell-after treatments, we ask these participants a series of incentivized comprehension checks. We tell participants that we will randomly choose 20 people to receive a gift card with $5 for each question that they answer correctly. If you choose to contact Congress, when would these future participants see the extra profile of you saying that you did so? In the second one, "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?" Participants' responses were corrected after question 2. Subfigure b plots the distribution of Tell-After and Invitation participants' responses to comprehension question: 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 subfigure of Panel A, N = 4,293 in the right subfigure of Panel A, and N = 4,291 in Panel B, corresponding to 90%, 81%, and 81% of Tell-after and Invitation participants, respectively.

Figure B6: Whether participants correctly remember paired participants' traits

(a) Wave-1 participants



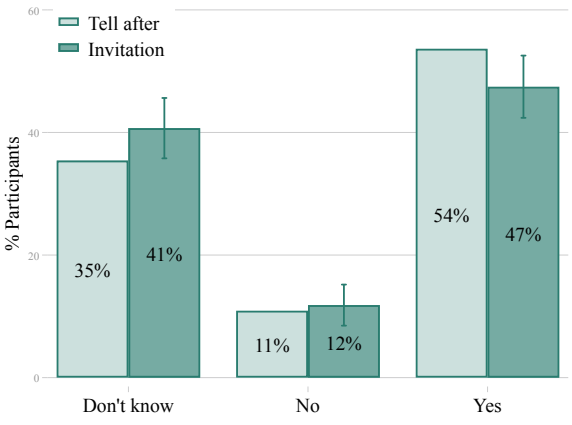

(b) Wave-2 participants



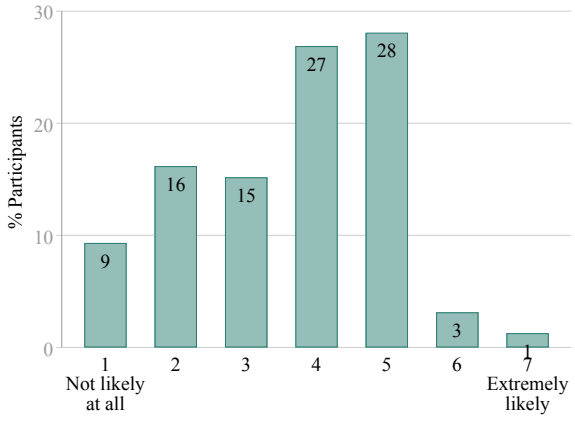

Note: This figure plots the share of Wave-1 and Wave-2 participants who correctly identified their matches' demographics. The error bars give the 95% confidence intervals for a regression of whether participants answered correctly on an indicator for being in the [Invitation/Treatment] group. In both surveys, we randomized subsets of Wave-1 and Wave-2 participants to answer multiple-choice questions for recall of each demographic trait for their corresponding match in the other wave. The sample sizes in (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.

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: Panel A plots the distribution of Tell-after and Invitation participants' responses to comprehension question 6. 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 assigned to answer question 6 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. Panel B plots the distribution of responses. N = 1,541 and N = 1,546 in Panels A and B, respectively.

Table B1: Wave 1 attrition results and other secondary outcomes

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

Note: This table estimates survey attrition by treatment status. 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 10 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 2 and 3 just after participants choose whether to email Congress or not, randomizing Control participants to answer one or the other. These regressions include all controls in our main Wave-1 regressions (Appendix B.2.2). 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) |
|---|---|---|---|---|---|---|
| | **Full sample** | **Control** | **Tell after** | | **Invitation** | |
| | Mean | Mean | Δ Mean | *p-value* | Δ Mean | *p-value* |
| Woman | 0.638 | 0.632 | 0.006 | (0.617) | 0.012 | (0.317) |
| Hispanic | 1.026 | 1.027 | -0.001 | (0.803) | 0.000 | (1.000) |
| Has $\geq$ 4-year college degree | 0.826 | 0.828 | -0.007 | (0.484) | 0.001 | (0.920) |
| **Age ranges:** | | | | | | |
| 20-39 | 0.114 | 0.119 | -0.019** | (0.018) | -0.001 | (0.901) |
| 40-59 | 0.341 | 0.337 | 0.016 | (0.182) | -0.001 | (0.934) |
| 60-79 | 0.545 | 0.544 | 0.003 | (0.817) | 0.002 | (0.878) |
| **Income bins (USD):** | | | | | | |
| Less than 50,000 | 0.167 | 0.170 | -0.004 | (0.689) | -0.007 | (0.484) |
| 50,000-99,999 | 0.321 | 0.325 | -0.009 | (0.453) | -0.008 | (0.505) |
| 100,000-149,999 | 0.234 | 0.231 | -0.004 | (0.716) | 0.015 | (0.173) |
| 150,000-199,999 | 0.130 | 0.126 | 0.007 | (0.437) | 0.007 | (0.437) |
| 200,000 or more | 0.148 | 0.147 | 0.009 | (0.317) | -0.008 | (0.374) |
| **Residence by state marginality:** | | | | | | |
| Red state | 0.236 | 0.231 | 0.017 | (0.122) | 0.002 | (0.856) |
| Blue state | 0.441 | 0.438 | -0.005 | (0.701) | 0.016 | (0.218) |
| Purple state | 0.322 | 0.331 | -0.012 | (0.317) | -0.017 | (0.157) |
| **Climate beliefs:** | | | | | | |
| Climate worry (1-7) | 6.421 | 6.430 | -0.013 | (0.516) | -0.016 | (0.446) |
| Desire for climate action (1-7) | 6.734 | 6.735 | -0.001 | (0.947) | 0.000 | (1.000) |
| Perceived local impacts (1-7) | 5.499 | 5.496 | 0.026 | (0.298) | -0.015 | (0.564) |
| **Political engagement and beliefs:** | | | | | | |
| Political-engage. index (std) | 0.000 | -0.003 | 0.005 | (0.848) | 0.005 | (0.841) |
| Prev. contacted reps | 0.731 | 0.733 | -0.009 | (0.413) | 0.004 | (0.716) |
| Prev. donated | 0.819 | 0.816 | 0.007 | (0.484) | 0.004 | (0.689) |
| Prev. canvassed | 0.081 | 0.081 | -0.001 | (0.886) | -0.001 | (0.886) |
| Prev. signed petition | 0.828 | 0.835 | -0.010 | (0.317) | -0.014 | (0.162) |
| Prev. phonebanked | 0.111 | 0.100 | 0.024*** | (0.003) | 0.013 | (0.104) |
| Political-efficacy index (std) | -0.000 | 0.008 | -0.006 | (0.810) | -0.020 | (0.442) |
| Degree prefer Dem friends (1-7) | 6.038 | 6.040 | -0.001 | (0.968) | -0.007 | (0.779) |
| Sample size | 8937 | 3616 | 2646 | | 2675 | |

Note: This table summarizes and tests for balance within the Wave-1 experimental sample, which we define as all those randomized to a Wave-1 treatment group. Column 1 presents means in the full sample on a range of baseline traits, and column 2 presents means among participants assigned to the A0 pure control group. Appendix B.2.2 describes each of these baseline traits in more detail. Variables labeled as "(1-7)" are elicited on Likert scales from 1 through 7, and variables labeled as "(std)" are indices standardized in the full Wave-1 experimental sample. Columns 3 and 4 present the differences in means between the A1 (Tell after) and A0 groups and heteroskedasticity-robust p-values testing the null of equality across these groups, respectively; columns 5 and 6 compare the A2 (Invitation) and A0 groups. In columns 3 and 5, we indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

Table 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 | Invitation | Δ Mean | *p-value* | Tell-after | Invitation |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treatment affected | 0.192 | 0.301 | 0.109*** | (0.000) | | |
| **Stated reasons:** | | | | | | |
| 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 blinded coder hired by the authors coded free-text responses for 1800 participants, split evenly between the Tell-After 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 B4: Categorization of free-text responses for participants' belief about the study purpose

| | Tell-after | Invitation | Δ Mean | *p-value* |
|---|---|---|---|---|
| | (1) | (2) | (3) | (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 study purpose, elicited soon after participants choose whether to email Congress. A blinded coder hired by the authors coded free-text responses for 1800 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*

We merge email records to individual study participants using combinations of name, email address, treatment status, state, and the time and day on which participants completed the survey. (Note that we can perfectly observe treatment status because we embed and see records from separate email forms for each treatment variation.) In total, 4,124 emails were sent by unique Wave-1 participants[45] and 1,834 were sent by unique Wave-2 participants.

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

2. Next, we merge on full name, state, treatment assignment, and the date and time at which participants took their survey. We restrict matches to those where the email was sent within 2 hours of a participant starting the survey. In Wave 1, 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 whose 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}.
- *State:* We control for indicators for the state in which participants currently live.
- *Gender:* We control for an indicator for identifying as a woman.

[45] This total includes emails sent by participants who are not included in the Wave-1 action experiment, but rather who were recruited directly for the WTP experiment (Section 5) or randomized to the belief sample (Section 6.1.5).

- *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}. Our main specifications include indicators for each income category.

- *Ethnicity:* We control for whether participants select that they identify as Hispanic or Latino.

**Climate-belief controls:**

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

- *Desire for climate action:* "How much do you want the federal government to do to slow or stop climate change, relative to what it's currently doing?" Participants select an integer 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:* "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 thier 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:

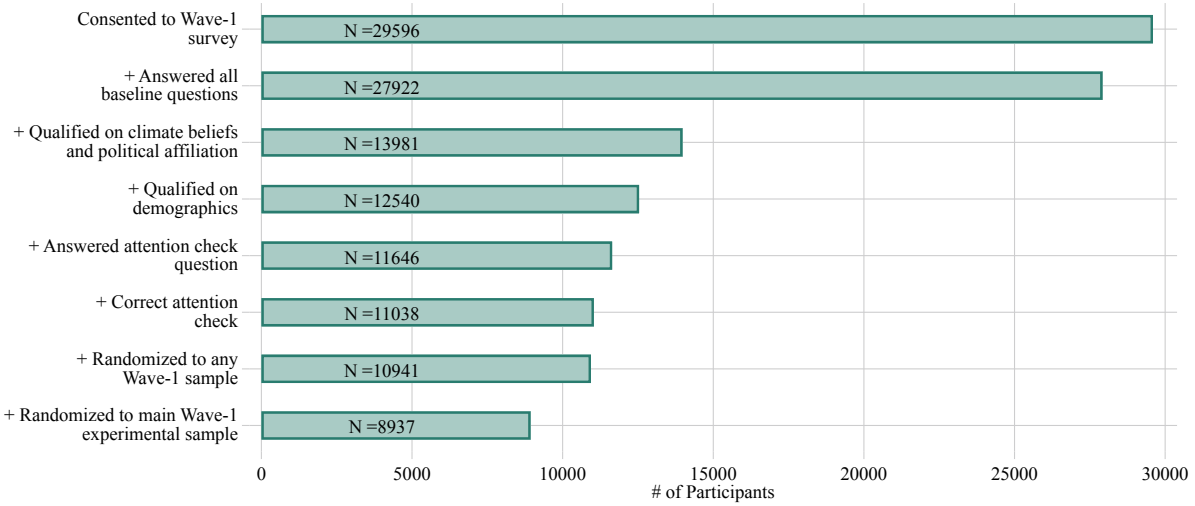
  - Emailed elected representatives about a political or social issue
  - Donated money to an organization working on a social or political issue
  - Canvassed door-to-door about a political or social issue
  - Signed a petition about a political or social issue
  - Phone-banked for a political or social 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 recruit Wave-1 participants using ads on Facebook, Instagram, and Twitter (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 5), leaving us with a sample of 12,540 participants. See Appendix A.1 for details on the screening questions.

Figure B8: Wave-1 recruitment

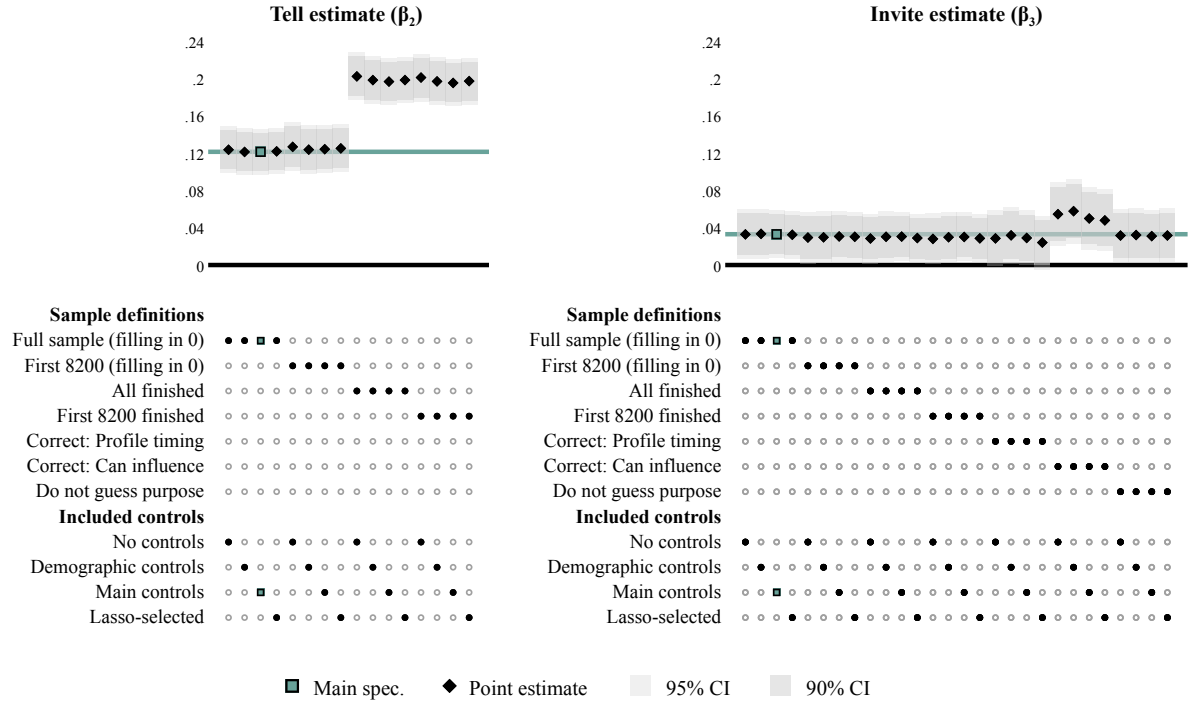



Qualifying participants then build their avatar, see that up to 10 Wave-2 participants may see their basic demographic profile, answer additional questions about their beliefs on climate policy, and answer a simple attention check. 11,646 participants answered the attention check and 11,038 did so correctly. 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 the Wave-1 experiment. 8,937 were randomized into the Wave-1 experimental sample, and the remaining 2,004 participants were randomized into parallel experimental samples described in Sections 5 and 6.

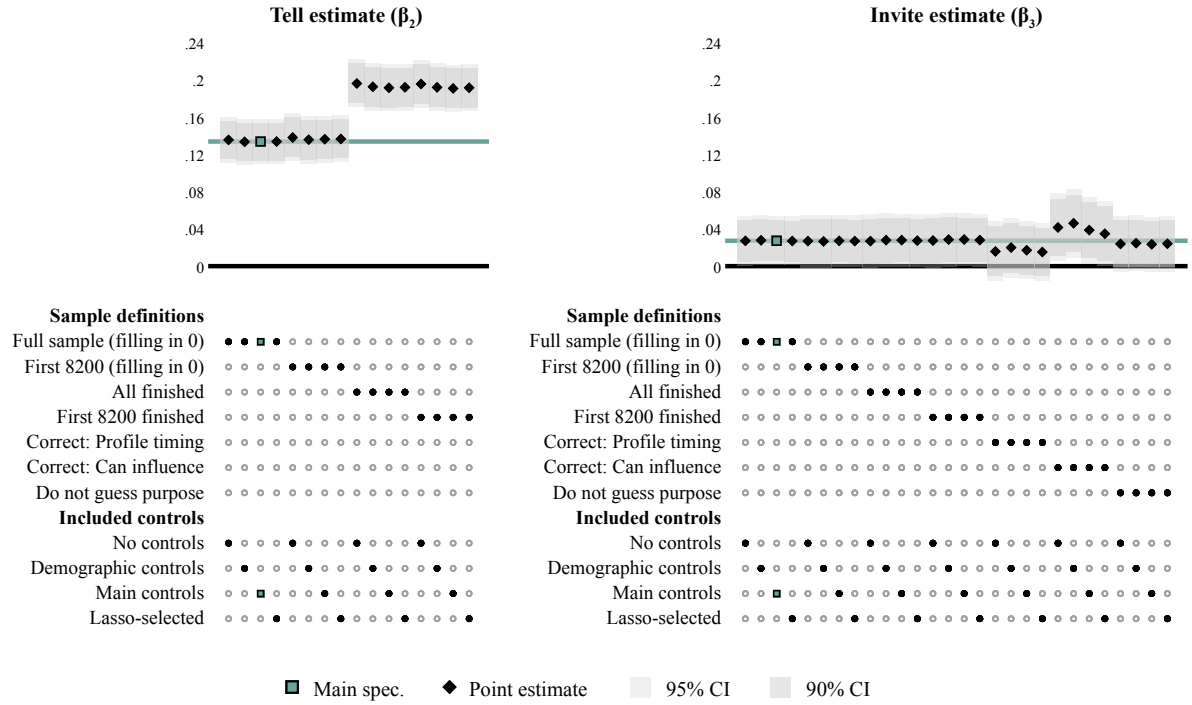
## B.4 Wave-1 robustness

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 $\beta_2$ and $\beta_3$ in Equation 5 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.

**Sample definitions:** The Wave-1 results are robust to changes in the experimental sample (Appendix Figure B9). While we pre-registered a total Wave-1 sample size of 8,200 participants, we did not specify whether this total would include participants who left the survey before explicitly choosing to email Congress or not. In practice, we stopped Wave-1 recruitment when 8,269 participants had explicitly chosen whether to email Congress, but our main regression specifications include all of the 8,937 participants who were randomized into a Wave-1 treatment arm and assume that those who attrit after randomization (and thus after they saw the upcoming chance to email Congress) 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 into a Wave-1 treatment arm, 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.[46]

**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 who correctly reported whether their email choice could affect Wave-2 action.[47]

**Control variables:** Our Wave-1 results are robust to the set of controls we include (Appendix Figure B9): 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.2.2 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 refraining from 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 3, 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

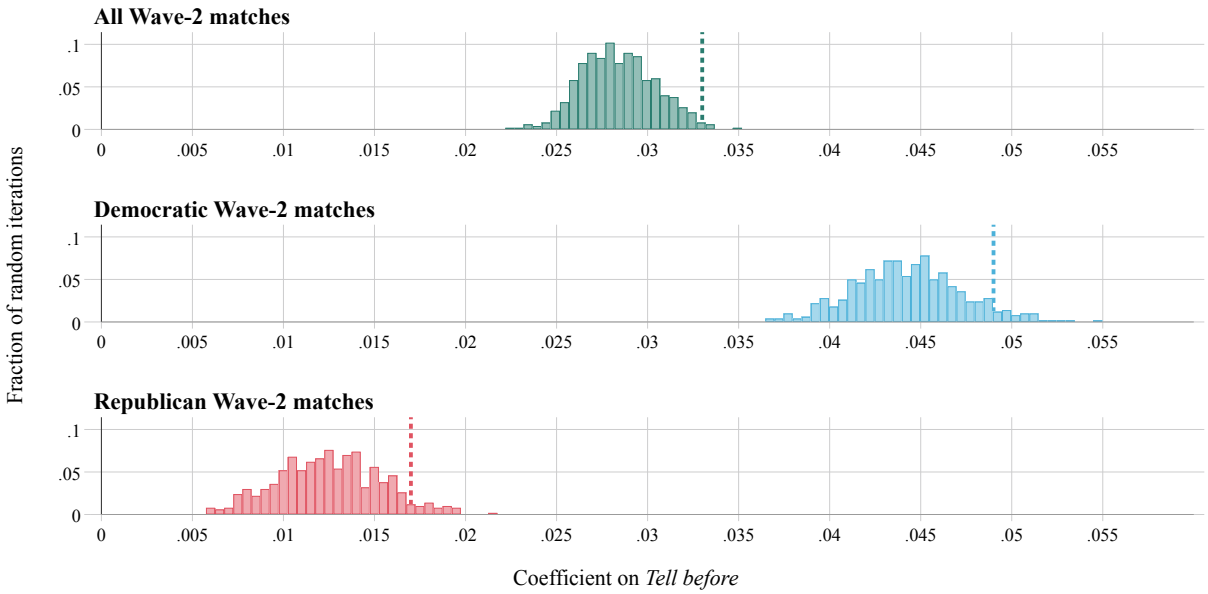
[46] As expected, restricting to those who make explicit email choices substantially increases our estimates for the impacts of the Tell-after arm relative to Control: Tell-after and Invitation participants are less likely to finish the Wave-1 survey (Section 4.3) and thus were more likely to have outcome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

[47] We 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 understood profile timing keeps 67% and 75% of the Tell-after and 75% Invitation groups, respectively, while 46% and 64% of the Tell-after and Invitation groups understood the influence structure. Thus, this robustness check is only suggestive.

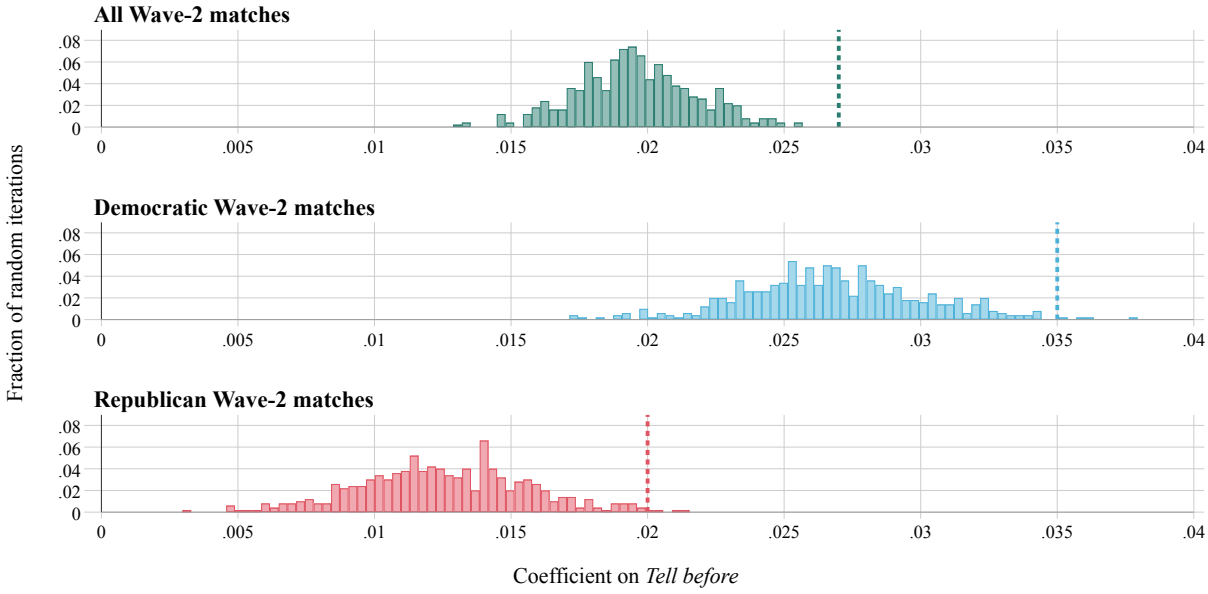
Tell-after and Invitation participants who correctly guessed that the study purpose was to test if participants try to influence others' behavior (Appendix Table B4; Appendix Figure B9).

Figure B10: Estimated Invitation effect ($\beta_3$) with adjustment for differential demand effects

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




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



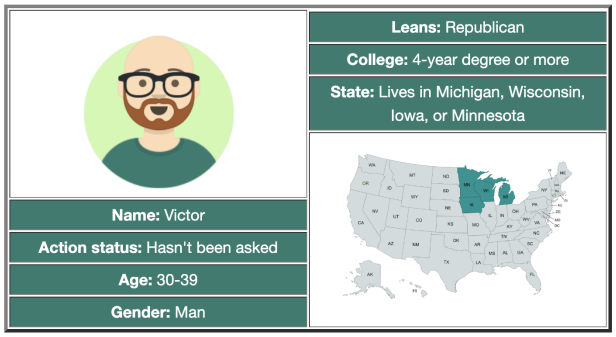

Note: This figure plots estimates for $\beta_3$ in Equation 5 when we adjust for differential experimenter demand between the Tell-after and Invitation groups. We run 500 iterations in which we drop Invitation participants who report experimenter demand at 6 on a 6-point scale until we equalize average experimenter demands between the groups. We re-estimate Equation 5 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 WTP experiment

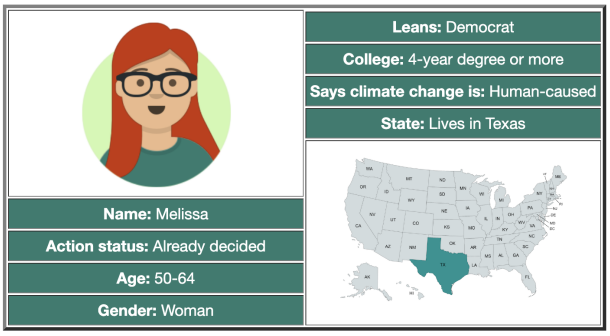
## C.1 Additional exhibits

Figure C1: Examples of profiles of possible WTP matches

(a) Before, with climate-belief info

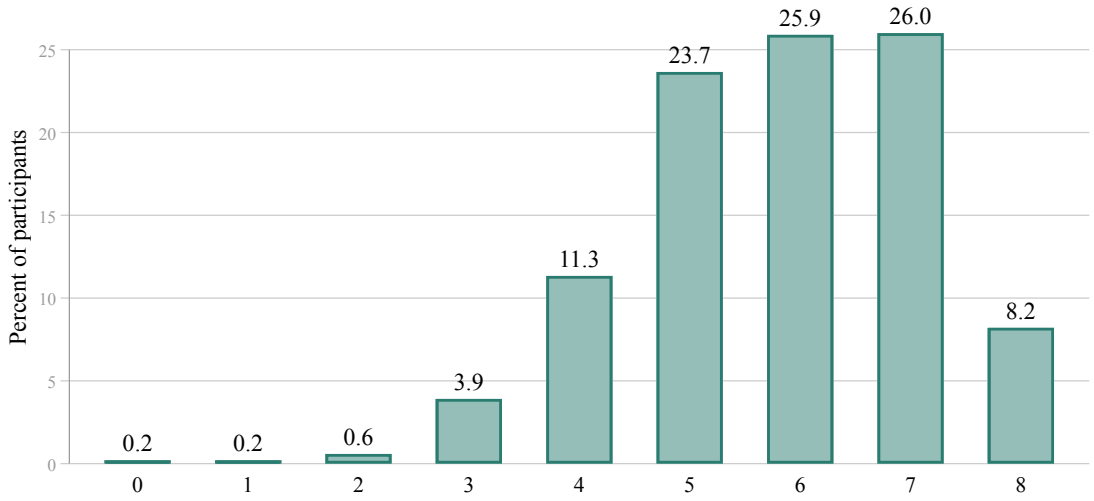



(b) After, without climate-belief info



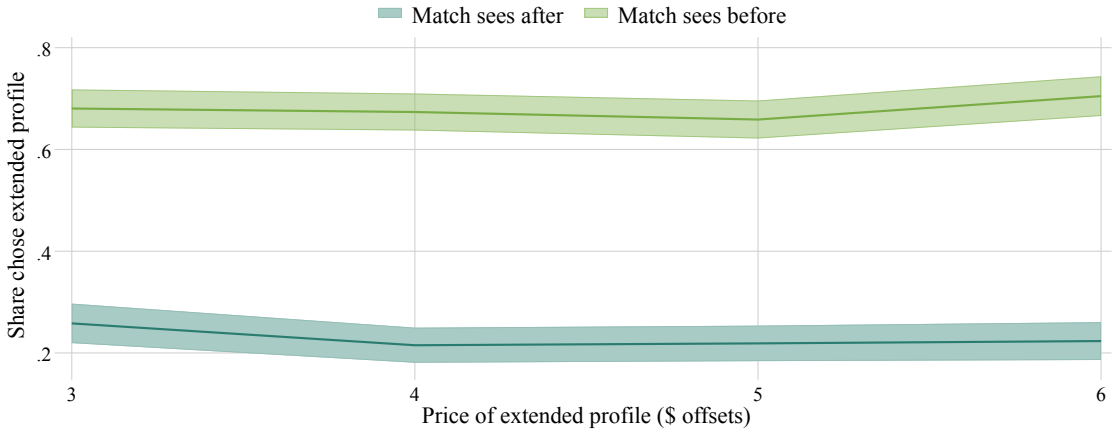

Note: This figure shows samples of the profiles shown to WTP participants of possible matches to whom their profiles could be shown. These possible matches are real people who we recruited before starting the Wave-1 and WTP experiments, and who vary in their demographics and political leanings. Within WTP participant, we randomized whether possible matches would see their profile before or after deciding for themselves whether to email Congress; this timing is shown in "Action status," where those who would see the WTP participants' profile before or after making their email choice are labeled as "Haven't been asked" and "Already decided," respectively. Across WTP 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).

Figure C2: Distribution of accuracy on WTP comprehension questions




Note: This figure plots the percent of participants who correctly answered a total of 0 through 8 comprehension questions correctly in the main WTP experiment. The figure restricts to those who finished all 20 binary choices, all of whom were asked 8 comprehension questions. See the Online Supplement for details on the comprehension questions asked.

Figure C3: 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 restricets to the main analysis sample. Profile choices are inelastic with respect to offset amounts.

Table C1: Attrition in the WTP experiments

| | (1) | (2) | (3) |
|---|---|---|---|
| | **Finish 20 WTP choices** | | |
| | *Round 1* | | |
| | Main WTP variant | Both WTP variants | *Round 2* |
| 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 analysis attrition in the WTP samples. The outcome variable is whether participants complete profile choices for all 20 possible matches. Column 1 restricts to participants randomized to the main WTP 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 WTP variant and those randomized to the gift-card WTP variant (Appendix C.4). 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 WTP experiment (Appendix D.5).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 WTP sample

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | **Comparing samples** | | | **Balance within WTP sample** | | |
| | Wave-1 | WTP | | See beliefs | Do not see beliefs | |
| | Mean | Δ Mean | *p-value* | Mean | Δ Mean | *p-value* |
| Woman | 0.632 | 0.128*** | (0.000) | 0.770 | -0.021 | (0.437) |
| Hispanic | 1.027 | 0.013** | (0.031) | 1.044 | -0.010 | (0.405) |
| Has ≥ 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 WTP sample. Columns 1-3 compare the WTP analysis sample with the Wave-1 Control group. The WTP analysis sample is all those who email Congress in the Wave-1 Control group, elect to take the WTP 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 WTP 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 WTP 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 B2 gives more detail on baseline traits.

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

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Showed extended profile | | | | | |
| | *No climate beliefs* | | | *Sees climate beliefs* | | |
| | Red | Blue | Purple | Red | Blue | Purple |
| Before | 0.225*** | 0.330*** | 0.331*** | 0.420*** | 0.466*** | 0.421*** |
| | (0.027) | (0.030) | (0.025) | (0.030) | (0.030) | (0.024) |
| Before | 0.159*** | 0.097*** | 0.153*** | 0.050** | 0.059** | 0.032 |
| | (0.026) | (0.024) | (0.023) | (0.024) | (0.024) | (0.021) |
| Before * Democrat | 0.290*** | 0.225*** | 0.201*** | 0.102*** | 0.055* | 0.133*** |
| | (0.034) | (0.033) | (0.029) | (0.031) | (0.031) | (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 |
| *p-values: Before*Dem* | (1)=(2)=(3): 0.098 | | | (4)=(5)=(6): 0.233 | | |

Note: This table estimates the main WTP 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 WTP 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: WTP participants' guesses for the study purpose

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Share | Mention non-pol sim | | Mention politics | |
| | gussed | In row | In full samp | In row | In full samp |
| Testing spillovers from seeing others act | 0.330 | 0.482 | 0.159 | | 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 WTP 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 blinded coder hired by the authors coded free-text responses for the 1,019 WTP 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—we also coded 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 unlikely to vote in favor of a climate bill, even if a fair number of state residents advocated for it;
- "Blue states," where legislators would be likely to vote in favor of a climate bill, even if not many state residents advocated for it;
- "Purple states:," where legislators are on the fence about climate policy, but could be convinced to vote for it with enough citizen advocacy.

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 policy-marginality groups above. We then group geographically close states among those that most social-media participants class in the same climate-politics tier. Using 2021 estimates for 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.[48] 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 C4)

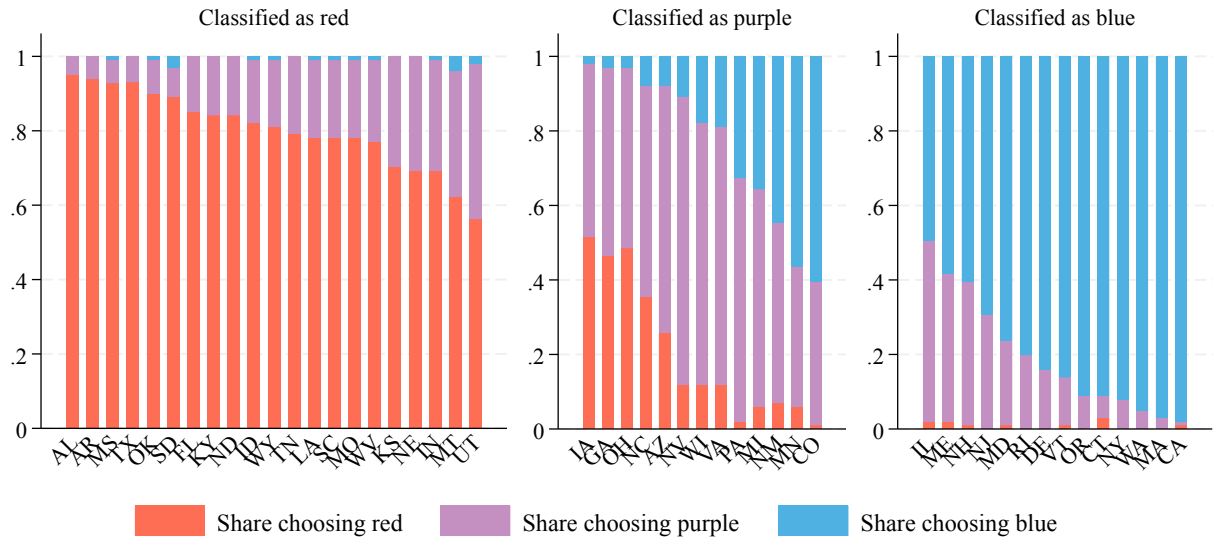
Table C5: State groups by tiers of climate-policy marginality

| **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 |
| TXta | | |

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.

[48] Rather than simply 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 WTP 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 up demographic cells defined by gender, 15-year age ranges, educational attainment, politics, and states. We pool states into groups to ensure that these demographic cells are large enough to e.g. truthfully pass on Wave-1 invitations to Wave-2 participants in a given cell.

Figure C4: Social-media sample responses by ultimate state-group classifications




Note: This figure plots the share of 101 Democrats recruited from social media who classfied each state as red, purple, or blue, separated by states that we ultimately assigned to a state group in that category of climate politics.

## C.3 WTP recruitment and completion

**Sample recruitment:** We recruit WTP participants from those who email Congress from the Wave-1 Control group. The Wave-1 experimental sample did not yield our pre-registered WTP sample size, so we also recruited a top-up sample who took the equivalent of the Wave-1 Control survey and were invited to join the WTP experiment if they emailed Congress, but who we do not include in the Wave-1 experimental sample. Across both sources, we invite participants who emailed Congress to take an extra 15- to 20-minute survey section in exchange for being entered into an additional gift-card lottery. We frame this survey section as an opportunity for them to choose how we should spend our time and funding to address climate change.

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 WTP survey, and 1,519 (92%) began it. Of these, we randomize 1,350 to the main WTP experiment and randomize 166 to a variant that we detail in Appendix C.4. Our main analysis restricts to participants assigned to the main WTP variation.

**WTP attrition:** Appendix Table C1 analyzes WTP attrition. Of the 1,350 participants who began the main WTP version, 1,239 went through the full description of the WTP set-up and began answering comprehension questions. 1,109 participants were randomized to either see profiles of matches that show or hide their beliefs about climate change, and 1,091 participants finished answering the final comprehension question after this randomization. 1,058 participants then started making binary profile choices, and 1,023 made all 20 choices. These participants compose our main analysis sample.

## C.4 WTP design with gift-card option

One threat to our interpretation of the WTP 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 WTP experiment in which participants also have the option to choose take-home gift cards.

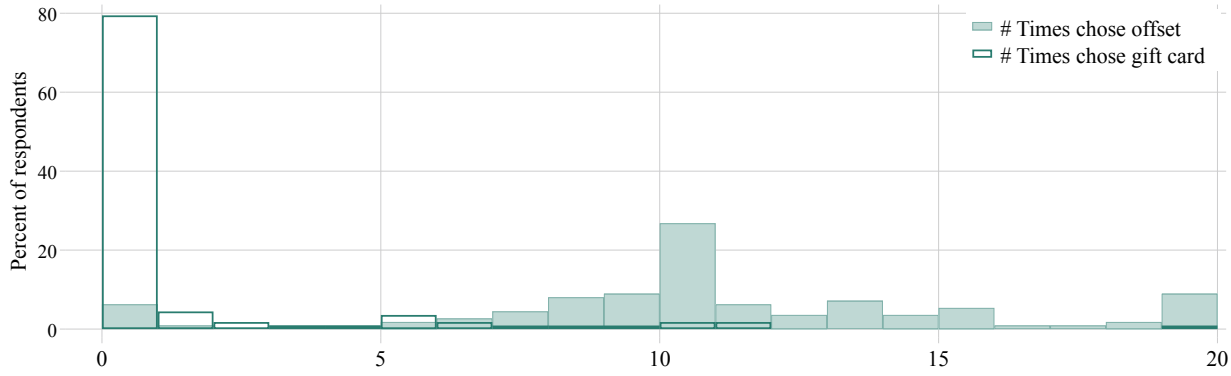
**Set-up:** While participants in the main WTP version choose between two options for each possible match, we randomize some participants to an alternate set-up (the "Gift-card group") in which they choose between these two baseline options and a third option: we will show the returning match the WTP participant's basic demographic profile and send the WTP 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 randomized 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 WTP variant (column 2, Appendix Table C1). Due to this differential attrition, we restrict to participants in the main WTP variant throughout our main analysis of the WTP experiment. However, Appendix C.5 shows that all of our results are fully robust to including all participants.

**WTP participants value carbon offsets:** While participants chose the basic profile and offset donation in an average of 10.5 choices, they chose the basic profile and gift card in an average of 1.2 choices; about 80% of participants never chose the take-home gift card. Thus, most participants value one dollar of carbon offsets more highly than one dollar in a gift card.

There are two remaining concerns. First, 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.

Figure C5: Share of respondents choosing offset donation vs. gift card




Note: These figures plot the share of WTP participants who are assigned to the Gift-card variant and complete all 20 choices 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.)

## C.5 WTP robustness

Appendix Figures C6 through C7 show the robustness of our main WTP results—coefficients $\beta_1$, $\beta_2$, and $\beta_3$ in Equation 6—to a range of perturbations.

**To including design variants:** First, the WTP results are robust to including participants who finished all 20 WTP choices in the Gift-card group, not just in the main WTP variant (Appendix C.4)

**Study comprehension:** Our results are also robust to excluding the 166 participants (16% of our main sample) who answered fewer than 5 comprehension questions correctly. Appendix Figure C2 gives the full comprehension distribution.

**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 guessed this study purpose in a free-text response at the end of the WTP survey, coded by a treatment-blinded academic hired by the authors.[49] 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 these concerns by asking participants to report how they made their binary choices between basic and extended profiles—and whether there were particular features they

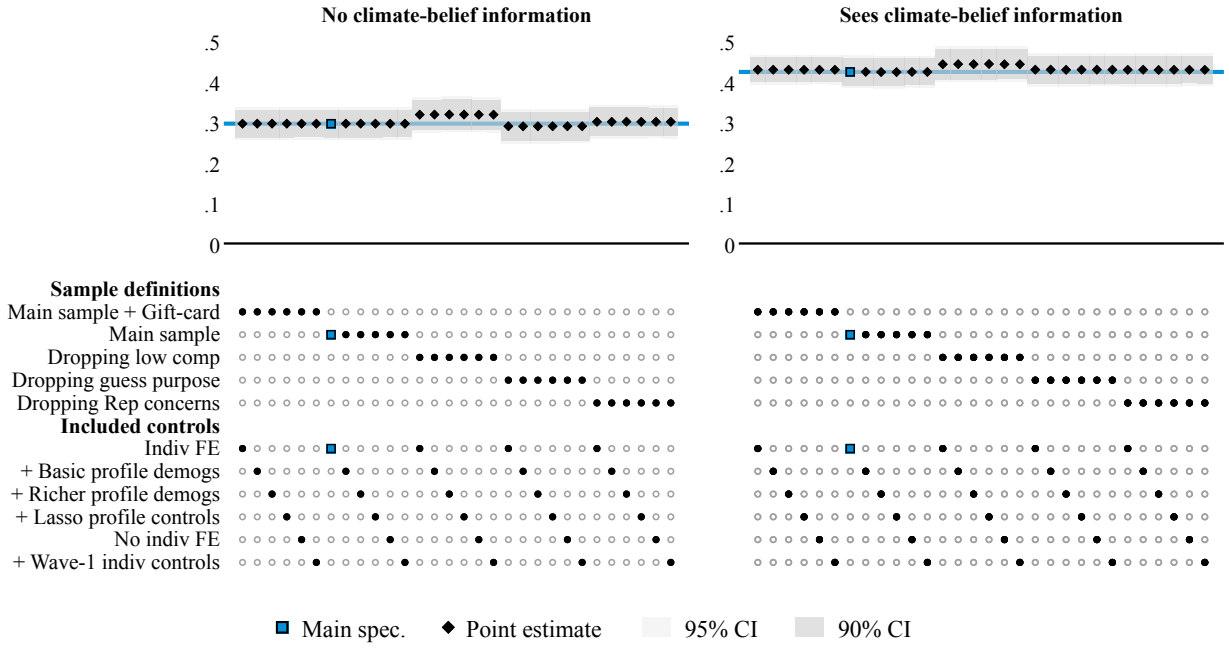
[49]This robustness check also excludes the 4 main-sample respondents who left the survey between making their 20 binary choices and answering this prompt. The same is true for the next robustness results, in which we drop those who did not report how they made binary choices.

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 WTP results are robust to the set of control variables we include. While our main specifications include only WTP 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 match state group 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.2.2).

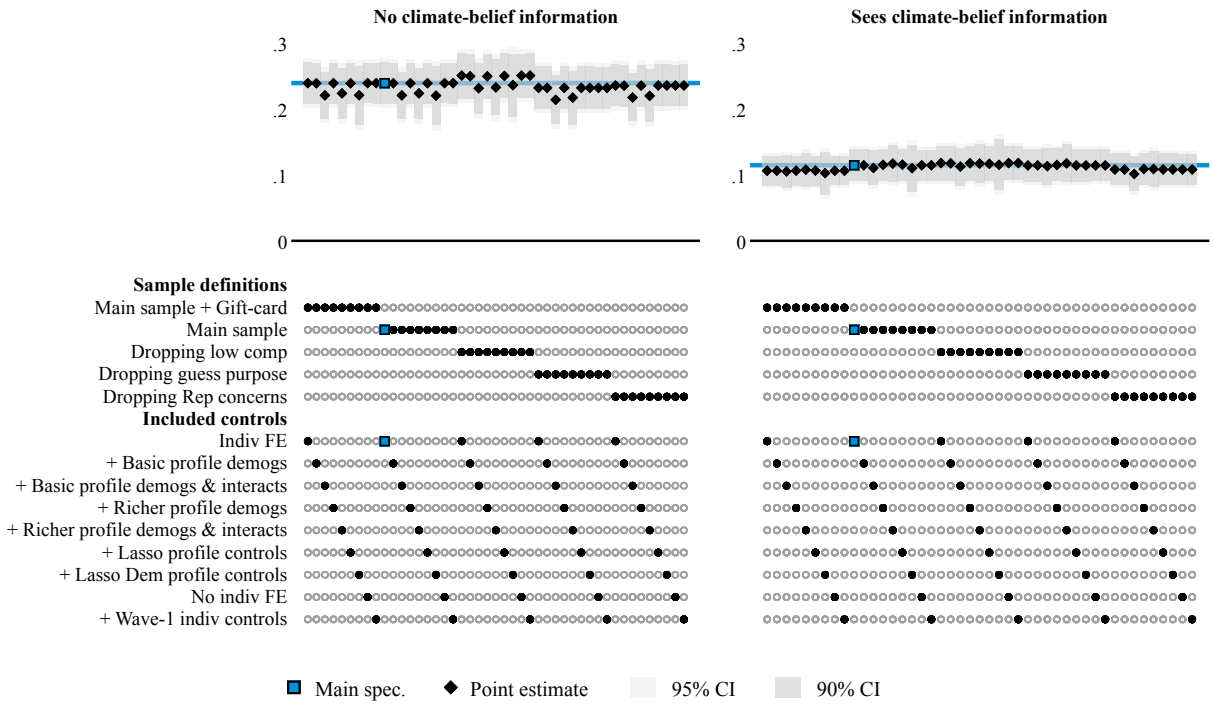
Moreover, the coefficients on $Dem_{ij}$ and $Before_{ij} * Dem_{ij}$ in Equation 6 are robust to controlling for the interactions of all match demographics with $Before_{ij}$ and for the main effects and interactions with $Before_{ij}$ of all traits selected by Lasso to predict $Dem_{ij}$. Thus, our estimates for partisan gaps in influence motives are not capturing correlations between matches' politics and other traits.

Figure C6: Specification chart: WTP experiment, $\beta_1$ coefficient on $Before_{ij}$



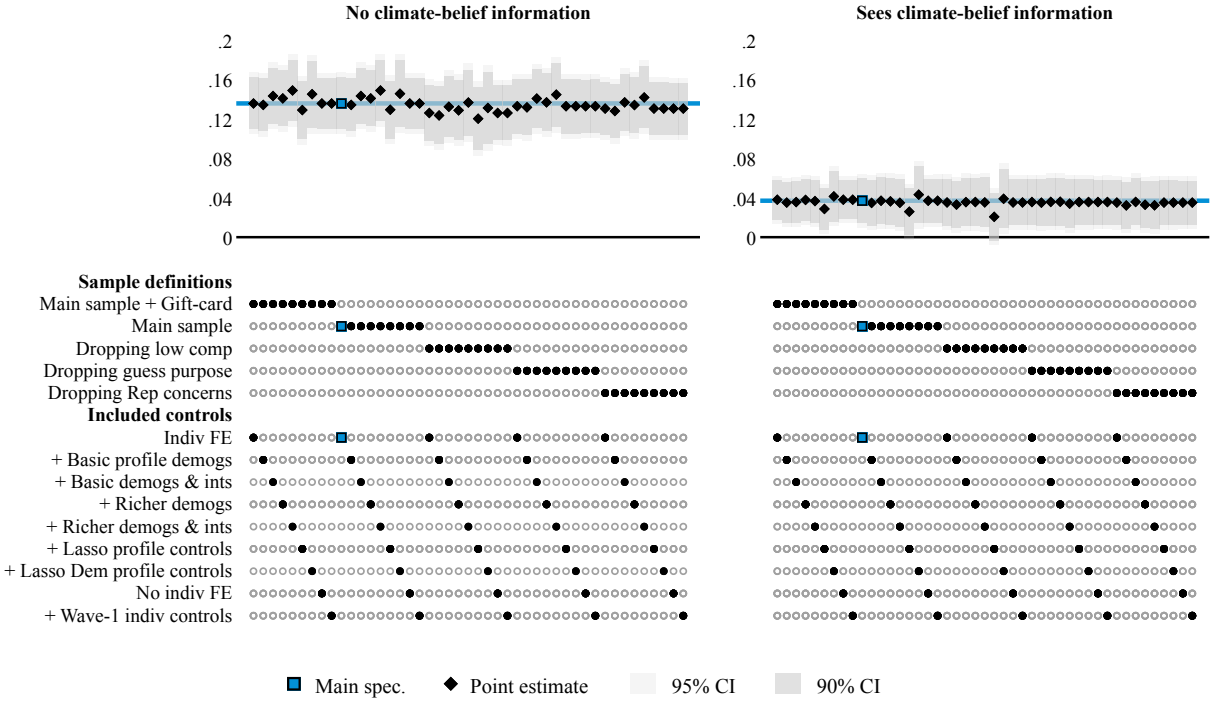

Note: This figure plots our estimates for $\beta_1$ in Equation 6 under a range of sample and control specifications (described in Appendix C.5). The panels at left and right plot coefficients in the WTP subsamples who saw profiles without and with information on matches' climate beliefs, respectively. The outcome variable is whether participants pass on an extended profile showing that they emailed Congress.

Figure C7: Specification chart: WTP experiment, $\beta_2$ coefficient on $Before_{ij} * Dem_{ij}$




Note: This figure is analogous to Appendix Figure C6, but with estimates for $\beta_2$ in Equation 6.

Figure C8: Specification chart: WTP experiment, $\beta_3$ coefficient on $Dem_{ij}$



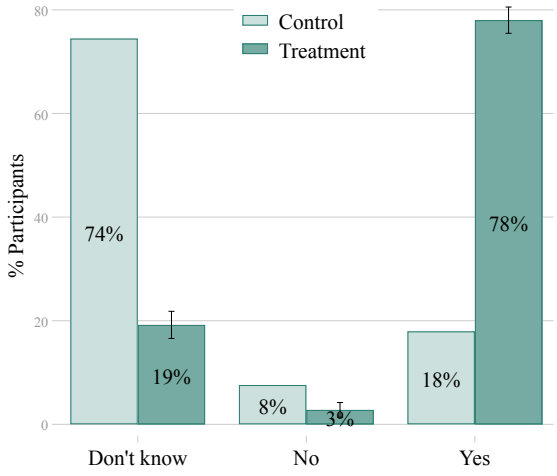

Note: This figure is analogous to Appendix Figure C6, but with estimates for $\beta_3$ in Equation 6.

# D Appendix to the mechanism experiments

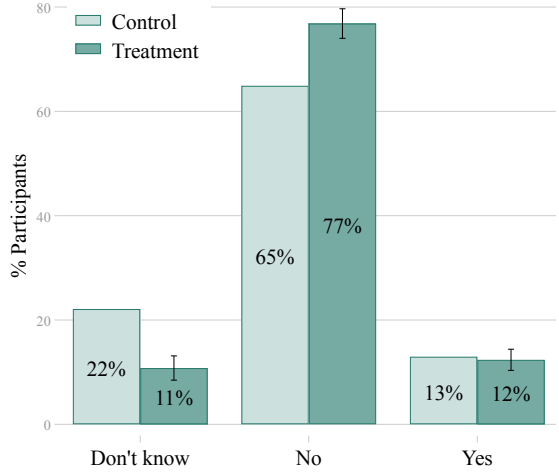
## D.1 Additional exhibits

Figure D1: 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?**

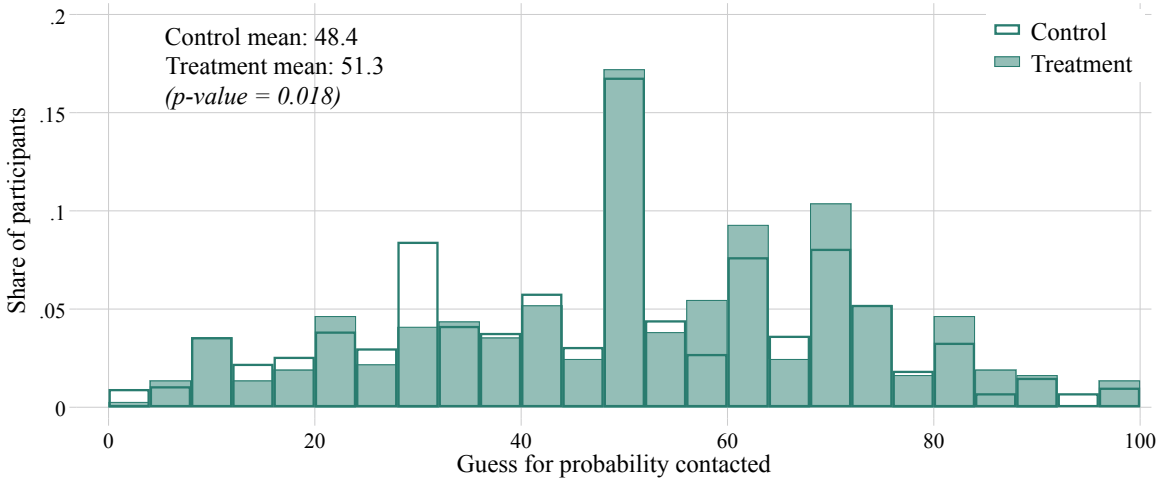
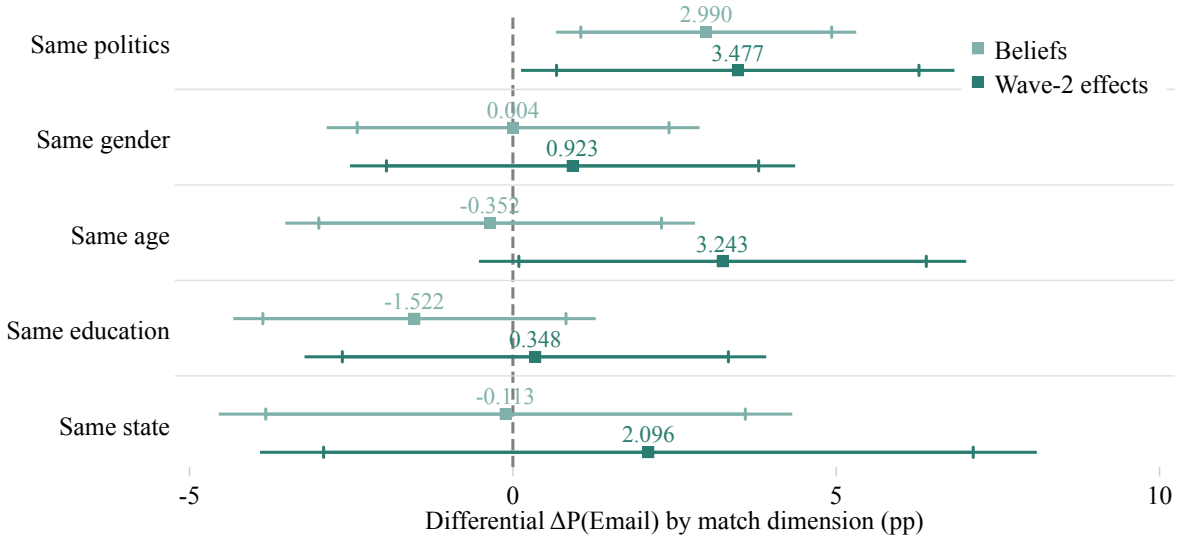
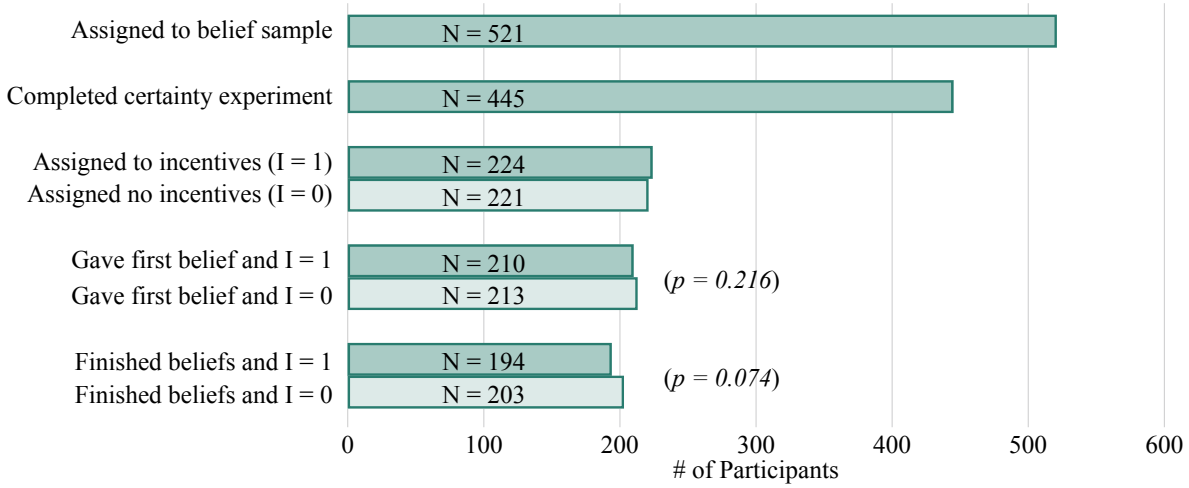


Figure D2: Differential perceptions of and actual $\Delta P(Email)$ by shared match traits




Note: The panels of this figure compare the belief sample's estimates for differential $\Delta P(Email)$ by sender-recipient similarity on particular traits. We estimate differental influence beliefs in versions of Equation 8 in which we simultaneously interact $Profile_i$ with each of the similarity traits. Likewise, we estimate actual differential $\Delta P(Email)$ by match traits in versions of Equation 7 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.

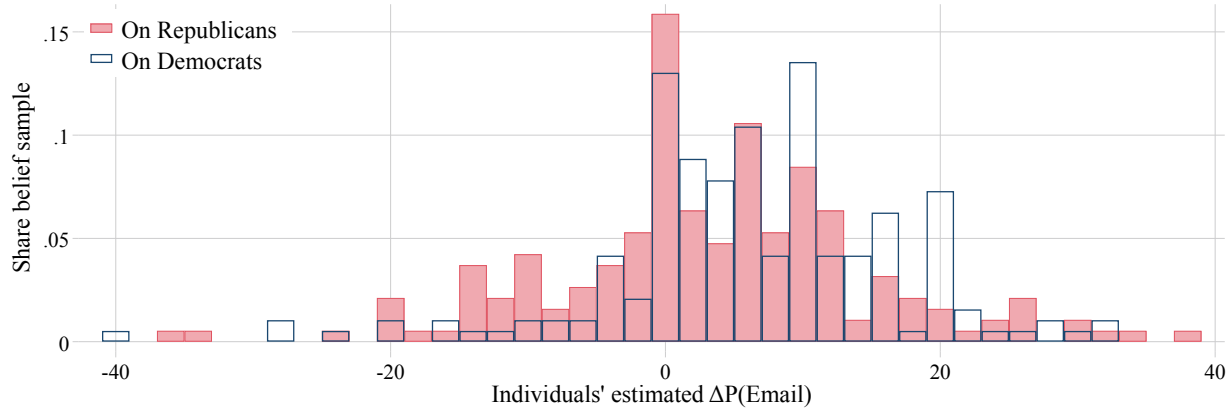
Figure D3: Belief-sample completion



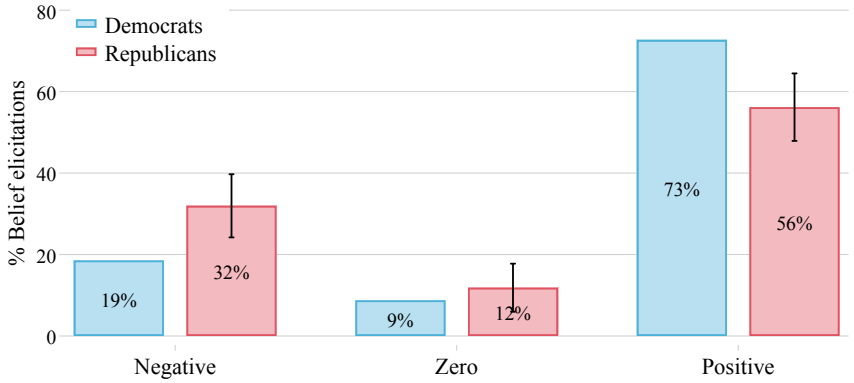

Note: This figure shows recruitment and attrition in the belief sample. About 521 of the total 10,941 participants randomized into samples associated with the Wave-1 experiment (Appendix B.3) are randomized to the belief sample. They first complete the certainty experiment (Appendix D.3) and then are 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 elicitations. Those who complete all four elicitations 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 D4: Other views on $\Delta P(Email)$ beliefs by target politics

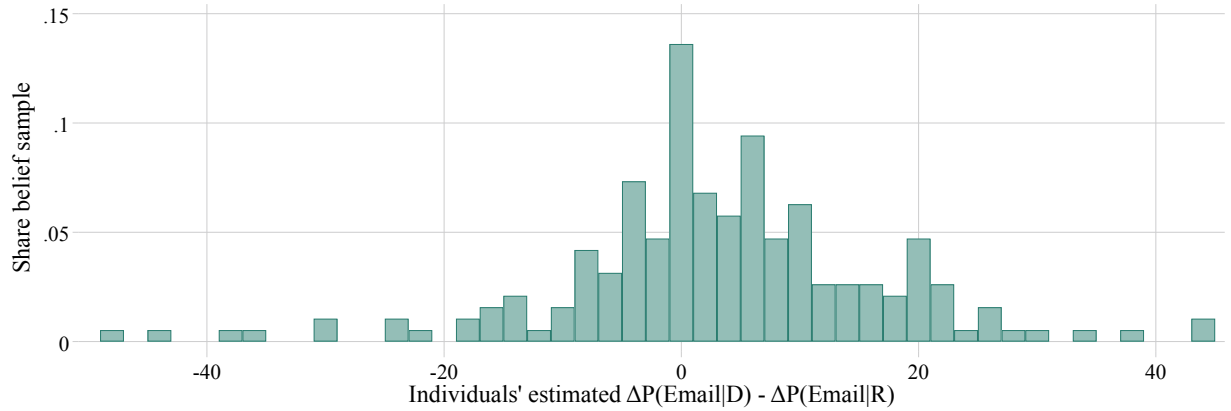
*Panel A. Distribution of $\Delta P(Email)$ influence beliefs by political party*




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

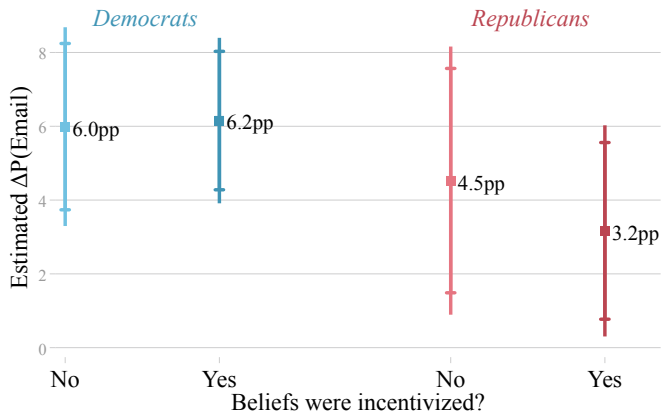



*Panel C. Distribution of individuals' beliefs for $\Delta P(Email|D) - \Delta P(Email|R)$*




Note: Panel A plots the distribution of participants' individual estimates of $\Delta P(Email)$, 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 $\Delta P(Email)$ 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 elicitations in which Democrats estimate that $\Delta P(Email)$ 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 $\Delta P(Email|D) - \Delta P(Email|R)$, 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 D5: Comparing estimated $\Delta P(Email)$ with and without accuracy incentives




Note: This figure compares estimates for participants' $\Delta P(Email)$ beliefs with and without accuracy incentives. We estimate Equation 8 separately for beliefs about Wave-2 Democrats and Republicans and by whether belief participants had accuracy incentives or not (see footnote 34). 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. We plot standard errors clustered by belief-sample participant.

Figure D6: Explanation of the certainty experiment

**Slide 1.**

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

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

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

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

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

---

**Slide 2.**

First, what are **carbon offsets**?

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

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

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

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

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

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

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

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

(Show preview of email form.)

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**Slide 4.**

Next, will your choices **matter**?

**Yes**, they will.

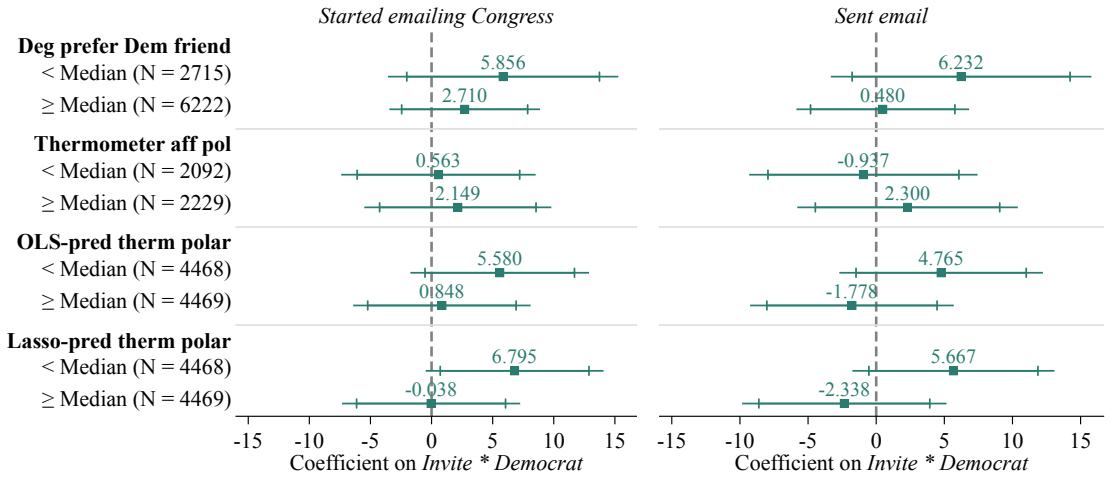
We will randomly choose 20 people and implement one of their choices. If one of your choices is selected, we'll do **whatever you picked** in that choice: We'll either donate $10 to carbon offsets, or we'll arrange for a past participant like that to email Congress.

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.

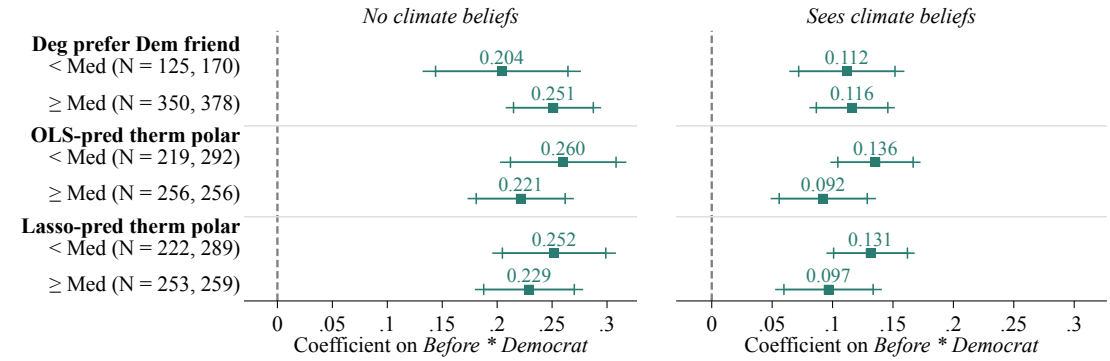
On the next page, you can start making your choices. Since there's a lot of information in each profile, the page will only advance once **5 seconds** have passed.

Figure D7: Heterogeneous outreach by affective polarization

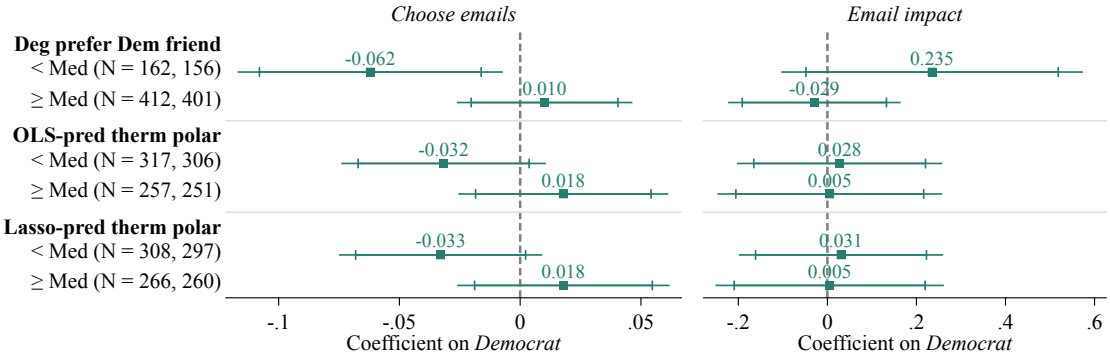
Panel A. Heterogeneity in Wave-1 estimates




Panel B. Heterogeneity in WTP estimates

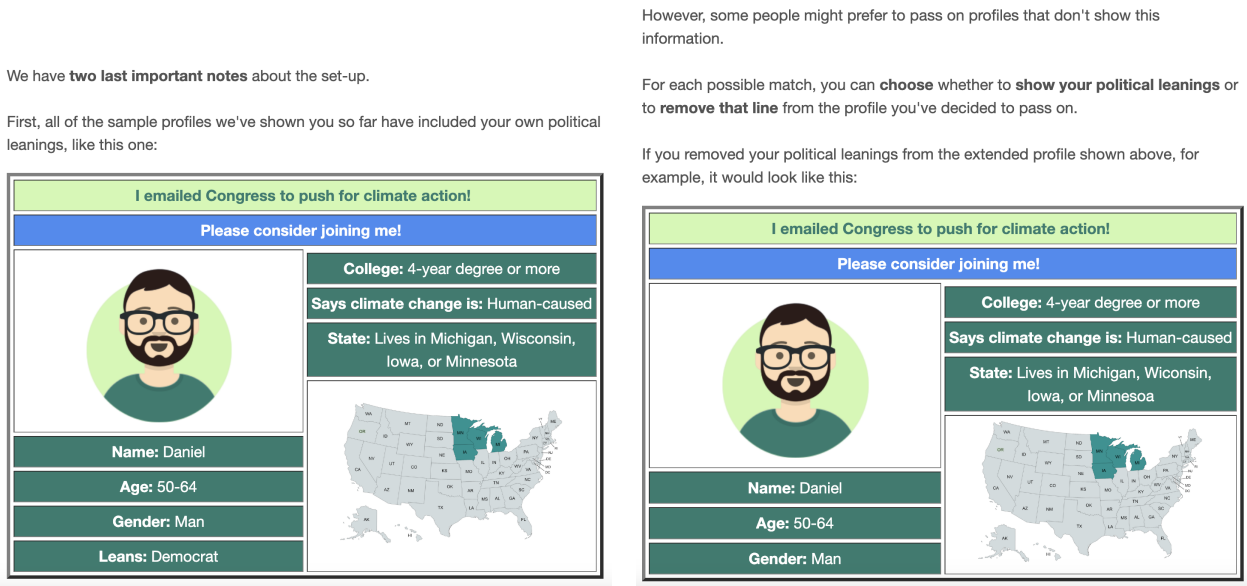



Panel C. Heterogeneneity in certainty-experiment estimates



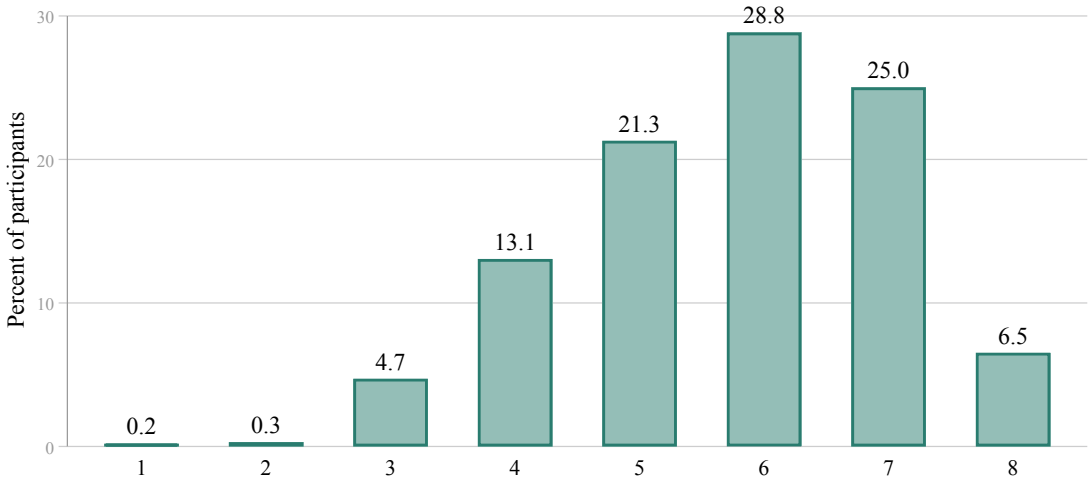

Note: Panel A plots estimates from a version of Equation 5 in which we interact $Tell_i$ and $Invite_i$ with an indicator that participant $i$'s Wave-2 matches lean towards the Democratic party, $Democrat_i$, separately for those with above- and below-median affective polarization on each measure outlined in Appendix D.4. We plot the coefficient on $Invite_i * Democrat_i$ separately for each polarization group. The regressions are otherwise identical to those in Table 2. Panel B plots estimates for $\beta_2$ in Equation 6 separately among WTP participants with above- and below-median affective polarization on each measure, where we calculate medians among the WTP sample. These regressions are otherwise identical to those in Table 3. The left figure of Panel C plots estimates for $\beta_1$ in Equation 9 separately among certainty participants with above- and below-median affective polarization on each measure, where we calculate medians in the certainty sample. The right figure in Panel C estimates the parallel regression with the outcome defined as projected impact on legislators' climate-policy support. Sample sizes with above- and below-median polarization are given in parentheses. In Panel B, number of sample participants with and without climate-belief information are shown at left and right, respectively. In Panel C, number of participants in the emial-choice and impact regressions are shown at left and right, respectively. Across all panels, capped and uncapped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively. Standard errors in Panel B are clustered by WTP participant.

Figure D8: Explanation of the option to hide WTP participants' leanings



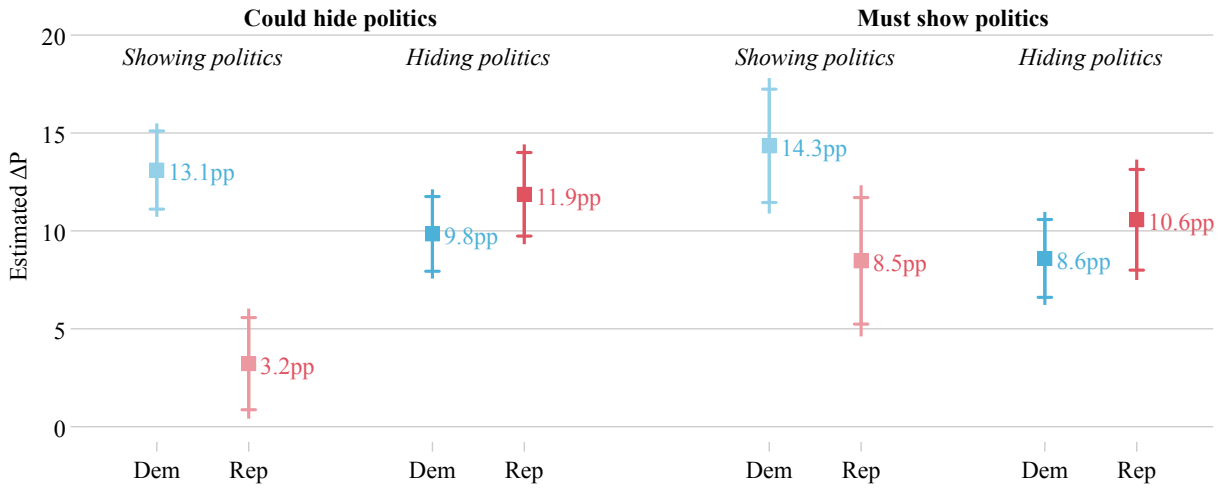

Note: This figure shows the explanation shown to participants in the round-2 WTP experiment who were randomly given the option to hide their politics from profiles they pass on to possible matches. Participants see this explanation soon before answering a set of comprehension questions, with no other changes in the explanation of the experimental set-up. Participants would see their own demographic profile here.

Figure D9: Distribution of accuracy on round-2 WTP 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-2 WTP experiment. The figure restricts to those who finished all 20 binary choices, all of whom were asked 8 comprehension questions. Note that those randomized to have the choice to hide their political leanings were asked a comprehension question in addition to the 8 questions analyzed here, which match those in the main WTP experiment. Appendix Figure C2 shows the parallel comprehension distribution in the main WTP experiment. See the Online Supplement for details on the comprehension questions asked.

Figure D10: Influence beliefs if hide politics or not: Those with and without the option to hide




Note: This figure plots participants' estimates for the impact of invitations that did or did not show that they are Democrats on the probability that a Democratic or Republican recipient would email Congress, split by whether participants had been randomized to have the option to hide their politics or not in the binary profile choices they had just made. The left half of this figure replicates Figure 8b. We estimate these beliefs as described in the note to that figure. The capped and uncapped bars plots 90% and 95% confidence intervals, respectively, with standard errors clustered by WTP participant.

Table D1: Wave-2 attrition and experimenter demand

| | (1) Observe if email Congress | (2) Observe demand effect | (3) Experimenter demand (std) |
|---|---|---|---|
| Treatment | 0.005 | 0.003 | 0.206*** |
| | (0.005) | (0.006) | (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. 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) |
|---|---|---|---|---|---|---|---|---|
| | **Full means:** | | | **Liberals:** | | | **Conservatives:** | |
| | Liberal | Conserv. | Control | Δ Treat | *p-value* | Control | Δ Treat | *p-value* |
| From Qualtrics sample | 0.171 | 0.945 | 0.177 | -0.011 | (0.317) | 0.944 | 0.001 | (0.901) |
| Woman | 0.565 | 0.521 | 0.560 | 0.011 | (0.432) | 0.522 | -0.002 | (0.912) |
| Hispanic | 0.055 | 0.101 | 0.056 | -0.003 | (0.617) | 0.102 | -0.002 | (0.856) |
| Has $\geq$ 4-year college degree | 0.758 | 0.361 | 0.761 | -0.006 | (0.617) | 0.356 | 0.010 | (0.579) |
| 20-39 | 0.217 | 0.427 | 0.215 | 0.003 | (0.803) | 0.429 | -0.004 | (0.824) |
| 40-59 | 0.326 | 0.318 | 0.340 | -0.027** | (0.038) | 0.319 | -0.003 | (0.860) |
| 60-79 | 0.457 | 0.255 | 0.445 | 0.024* | (0.087) | 0.251 | 0.006 | (0.708) |
| **Income bins (USD):** | | | | | | | | |
| Less than 50,000 | 0.232 | 0.397 | 0.228 | 0.009 | (0.453) | 0.399 | -0.004 | (0.824) |
| 50,000-99,999 | 0.324 | 0.378 | 0.329 | -0.010 | (0.442) | 0.388 | -0.019 | (0.291) |
| 100,000-149,999 | 0.222 | 0.136 | 0.219 | 0.006 | (0.617) | 0.131 | 0.009 | (0.489) |
| 150,000-199,999 | 0.108 | 0.051 | 0.108 | 0.001 | (0.912) | 0.040 | 0.022*** | (0.006) |
| 200,000 or more | 0.114 | 0.038 | 0.117 | -0.006 | (0.505) | 0.042 | -0.007 | (0.317) |
| **Residence by state marginality:** | | | | | | | | |
| Red state | 0.267 | 0.413 | 0.271 | -0.008 | (0.505) | 0.409 | 0.009 | (0.617) |
| Blue state | 0.419 | 0.257 | 0.415 | 0.008 | (0.568) | 0.260 | -0.006 | (0.708) |
| Purple state | 0.314 | 0.329 | 0.314 | -0.000 | (1.000) | 0.331 | -0.003 | (0.860) |
| **Climate beliefs:** | | | | | | | | |
| Climate worry (1-7) | 6.300 | 4.953 | 6.318 | -0.036 | (0.166) | 4.928 | 0.049 | (0.373) |
| Desire for climate action (1-7) | 6.630 | 5.216 | 6.635 | -0.010 | (0.617) | 5.155 | 0.123** | (0.031) |
| Perceived local impacts (1-7) | 5.483 | 4.836 | 5.466 | 0.035 | (0.243) | 4.873 | -0.075 | (0.157) |
| **Political engagement and beliefs:** | | | | | | | | |
| Member of resp. party | 0.319 | 0.742 | 0.321 | -0.005 | (0.701) | 0.734 | 0.015 | (0.349) |
| Political-engage. index (std) | 1.195 | -2.033 | 1.199 | -0.008 | (0.943) | -2.073 | 0.080 | (0.519) |
| Prev. contacted reps | 0.268 | 0.219 | 0.265 | 0.006 | (0.617) | 0.212 | 0.014 | (0.351) |
| Prev. donated | 0.686 | 0.241 | 0.683 | 0.006 | (0.644) | 0.241 | 0.000 | (1.000) |
| Prev. canvassed | 0.064 | 0.040 | 0.063 | 0.003 | (0.668) | 0.038 | 0.004 | (0.568) |
| Prev. signed petition | 0.746 | 0.390 | 0.748 | -0.003 | (0.803) | 0.384 | 0.011 | (0.541) |
| Prev. phonebanked | 0.078 | 0.045 | 0.079 | -0.001 | (0.901) | 0.049 | -0.009 | (0.261) |
| Political-efficacy index (std) | -0.109 | 0.186 | -0.120 | 0.021 | (0.162) | 0.193 | -0.014 | (0.505) |
| Prefer friend of own party (1-7) | 5.663 | 4.895 | 5.664 | -0.002 | (0.949) | 4.909 | -0.028 | (0.543) |
| Sample size | 5027 | 2954 | 2517 | 2510 | | 1468 | 1486 | |

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

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

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Liberals | | | Conservatives | | |
| | Social | Qualtrics | *p-value* | Social | Qualtrics | *p-value* |
| Woman | 0.581 | 0.492 | (0.000) | 0.393 | 0.528 | (0.001) |
| Hispanic | 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) |
| 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 liberals and conservatives recruited via social media (either directly or after being redirected from Wave-1 social-media recruitment) or from Qualtrics. Throughout Wave 2, we define "liberals" and "conservatives as those who lean towards or belong to the Democratic or Republican parties, respectively. All traits are defined as in Table D2. Columns 3 and 6 give 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 | | |
| | *All* | *Dem* | *Rep* | *All* | *Dem* | *Rep* |
| *Panel A: Start the process of emailing Congress* | | | | | | |
| Treatment | 0.068*** | 0.069*** | 0.208* | 0.046*** | 0.061* | 0.041** |
| | (0.015) | (0.015) | (0.107) | (0.014) | (0.032) | (0.016) |
| Control mean | 0.428 | 0.436 | 0.220 | 0.277 | 0.374 | 0.245 |
| N | 4328 | 4165 | 163 | 3653 | 862 | 2791 |
| Dem coefficient equal by source (2) = (5)? *p-value* = 0.818 | | | | | | |
| Rep coefficient equal by source (3) = (6)? *p-value* = 0.035 | | | | | | |
| *Panel B: Have a record of emailing Congress* | | | | | | |
| Treatment | 0.061*** | 0.063*** | 0.085 | 0.025** | 0.044* | 0.020* |
| | (0.014) | (0.014) | (0.074) | (0.010) | (0.026) | (0.011) |
| Control mean | 0.264 | 0.271 | 0.085 | 0.096 | 0.132 | 0.084 |
| N | 4328 | 4165 | 163 | 3653 | 862 | 2791 |
| Dem coefficient equal by source (2) = (5)? *p-value* = 0.490 | | | | | | |
| Rep coefficient equal by source (3) = (6)? *p-value* = 0.234 | | | | | | |

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). 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 affects among those recruited from each source, separately for liberals and conservatives.

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

| | Control | Invitation | Δ Mean | *p-value* |
|---|---|---|---|---|
| | (1) | (2) | (3) | (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 blinded coder 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: Choices of emails with certainty and email-impact beleifs by state politics

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Chose email over offsets | | | Impact on policy support | | |
| | *State groups* | | | *State groups* | | |
| | Red | Blue | Purple | Red | Blue | Purple |
| Republican | 0.000 | 0.022 | 0.003 | 0.074 | -0.143 | 0.036 |
| | (0.020) | (0.023) | (0.024) | (0.191) | (0.130) | (0.139) |
| Woman | -0.004 | 0.014 | -0.003 | -0.346 | 0.078 | -0.282 |
| | (0.017) | (0.019) | (0.036) | (0.228) | (0.193) | (0.283) |
| Has 4-year college | -0.037 | 0.034 | 0.010 | 0.000 | 0.128 | -0.354 |
| | (0.029) | (0.026) | (0.042) | (0.000) | (0.211) | (0.270) |
| **Age ranges:** | | | | | | |
| 35-49 | 0.047 | 0.039 | -0.063* | 0.000 | -0.156 | 0.080 |
| | (0.029) | (0.035) | (0.034) | (0.000) | (0.282) | (0.238) |
| 50-64 | 0.003 | -0.007 | -0.016 | 0.251 | -0.399 | 0.000 |
| | (0.023) | (0.031) | (0.036) | (0.228) | (0.251) | (0.000) |
| 65-79 | -0.016 | 0.027 | -0.006 | 0.000 | -0.317 | -0.028 |
| | (0.025) | (0.034) | (0.034) | (0.000) | (0.222) | (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 estimates Equation 9 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.

Table D8: Main WTP results, Round 2

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Showed extended profile | | | |
| | Must show politics | | Can hide politics | |
| Before | 0.445*** | 0.494*** | 0.408*** | 0.451*** |
| | (0.022) | (0.024) | (0.021) | (0.022) |
| Republican | | -0.044*** | | -0.064*** |
| | | (0.014) | | (0.015) |
| Before * Republican | | -0.097*** | | -0.086*** |
| | | (0.015) | | (0.015) |
| Mean: After Dem + Rep | 0.266 | | 0.293 | |
| Mean: After Dem | | 0.288 | | 0.324 |
| Num. participants | 505 | 505 | 490 | 490 |
| Num. choices | 10100 | 10100 | 9800 | 9800 |
| *p-values (2) = (4)* | | | | |
| Republican: 0.308 | | | | |
| Before * Republican: 0.561 | | | | |

Table D7: Balance table, Round 2 WTP

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

## D.2 The Wave-2 experiment

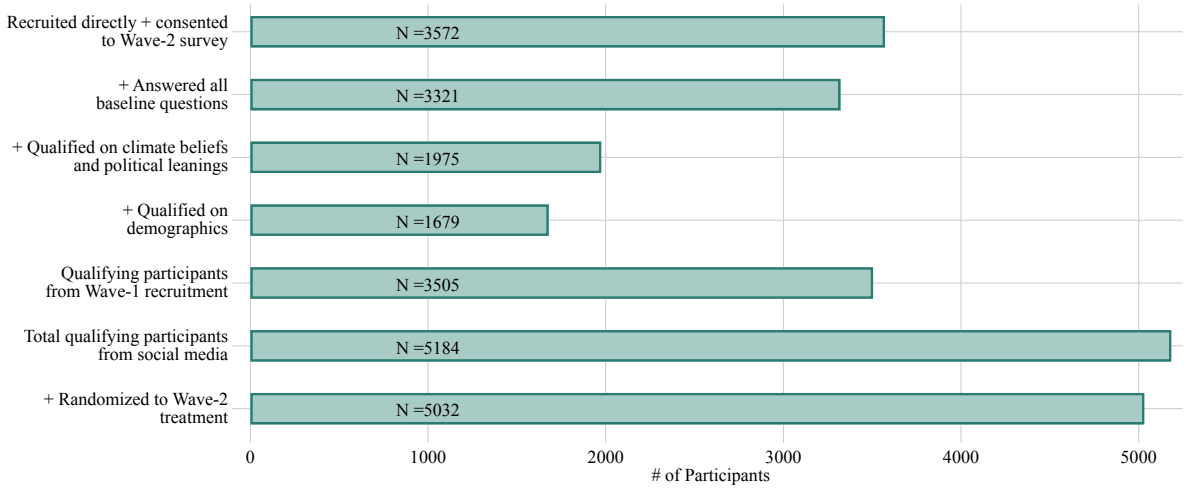
### *D.2.1 Wave-2 recruitment*

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

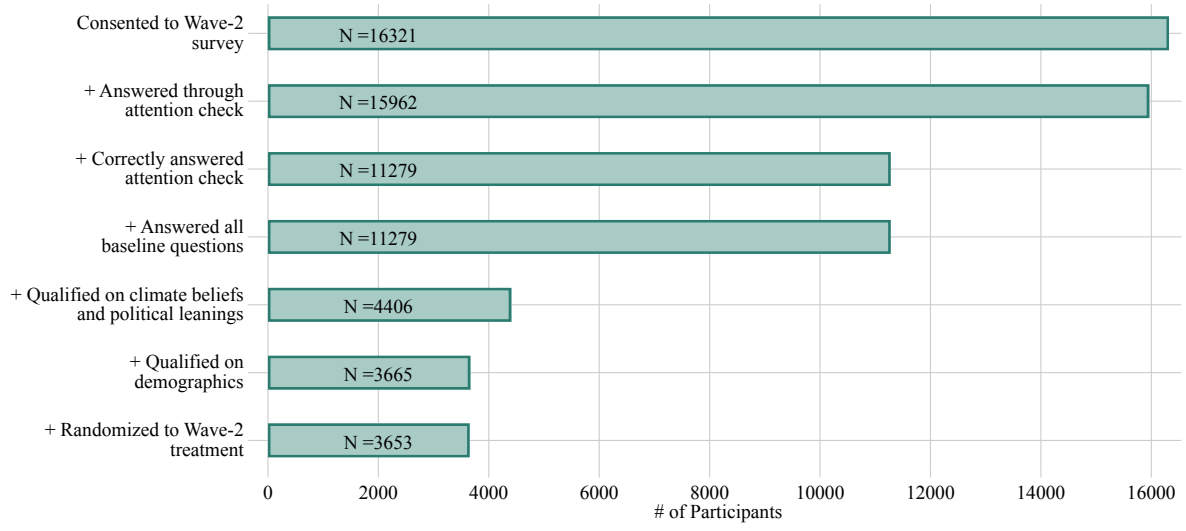
**Social-media recruitment:** We recruited Wave-2 participants via social media in two ways (Panel A of Appendix Figure D11). First, we redirected participants to Wave 2 if they initially started but were ineligible for the Wave-1 survey because they were not members of the Democratic party. Any such participant had completed the full suite of baseline questions on demographics, climate and political beliefs, and political engagement in the Wave-1 survey, and they were redirected to the Wave-2 survey at the point of constructing a basic avatar of themselves. In total, 3,505 participants started the Wave-2 survey via this route.

Figure D11: Wave-2 recruitment

**Panel A.** Recruitment from social media




**Panel B.** Recruitment from Qualtrics



Other participants were initially recruited from social media directly to the Wave-2 Qualtrics survey. In total, 3,572 unique participants from social media consented directly to the Wave-2 survey and provided an email address, which we required of all participants in order to link them with records of emailing Congress. Of these participants, 3,321 participants completed the full suite of baseline questions and 1,975 state that they believe that climate change is mostly human-caused and lean towards either the Republican or Democratic party. We then impose the feasibility-based screening criteria (Section 2.1), 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 leave 1,679 qualifying participants recruited directly to the Wave-2 survey, or 85% of those who qualify for Wave-2 on politics and climate beliefs and 51% of those who completed the baseline screening questions.

The full sample of N = 5,184 qualifying participants recruited from social media (either 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.

**Qualtrics recruitment:** While we recruited most Wave-2 Democrats from social media, we recruited nearly all Wave-2 Republicans sample directly from Qualtrics, which aggregates respondents from over 20 partnering market-research panels and other online samples. Participants recruited via Qualtrics were subject to identical screening criteria on demographics, political affiliation, and climate beliefs as Wave-2 participants recruited from social media (Panel B of Appendix Figure D11).

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

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 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 D12 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,[50] as well as to expanding the

[50]While we pre-registered a total Wave-2 sample size of about 4,250 Democrats, our social-media recruit-

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).

**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 D1 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 that we include in our main regressions, and a set of controls selected by double-post Lasso regressions from participants' demographics, baseline beliefs, and dummy variables for particular forms of past political engagement, separately by political party (Chernozhukov et al., 2018).

**Experimenter demand effects:** Next, 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 during the survey (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, experimenter 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, 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 D13), 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 D14 shows that the partisan gap we estimate is largely robust to controlling for the interaction of treatment status with key traits associated

ment yielded a total of 5,200 Democrats who saw the email preview, and thus for whom we can construct a filled-in email outcome, and 5,027 for whom we observe explicit choices to email Congress or not. Our results are 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.

with our differential recruitment strategies for Democrats and Republicans: whether a participant is recruited from Qualtrics, political engagement, and income.

Figure D12: Wave-2 specification charts

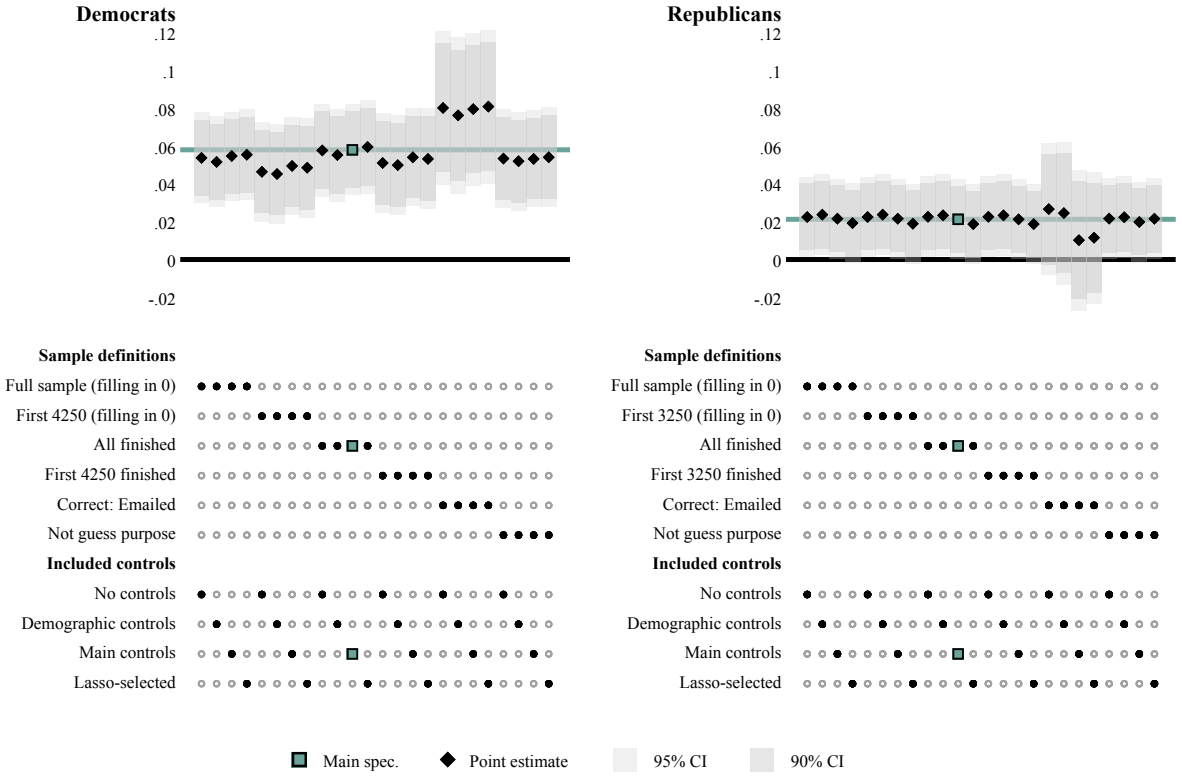



Figure D13: Robustness of main Wave-2 estimates to demand-effect adjustments

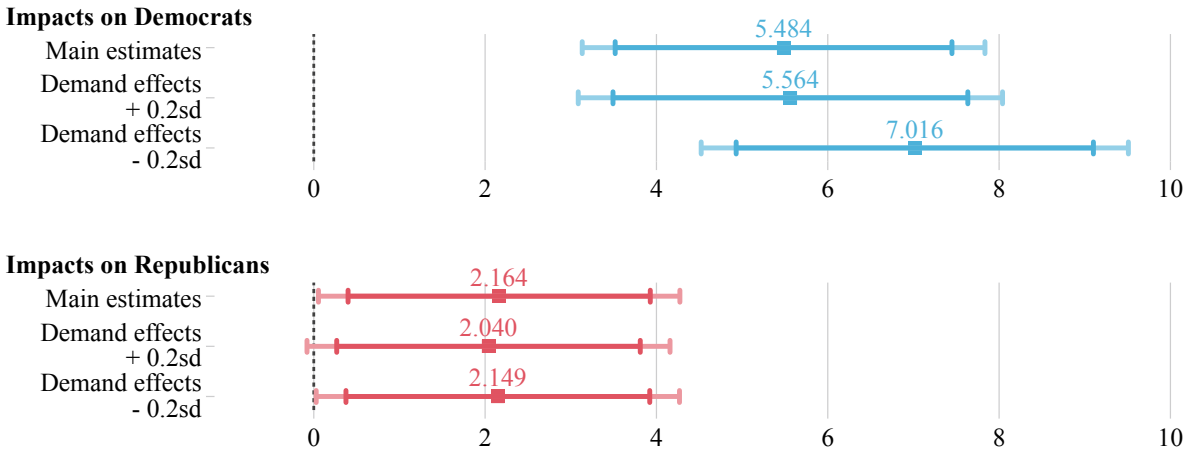
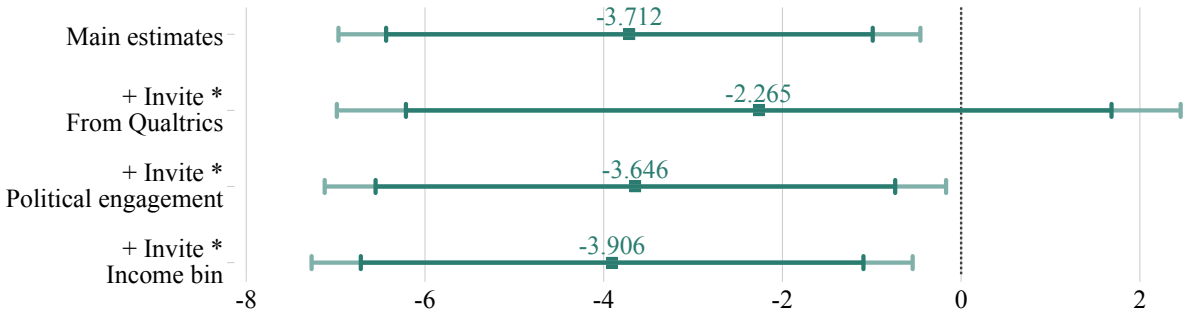


Figure D14: Robustness of partisan gaps in the Wave-2 invitation effect

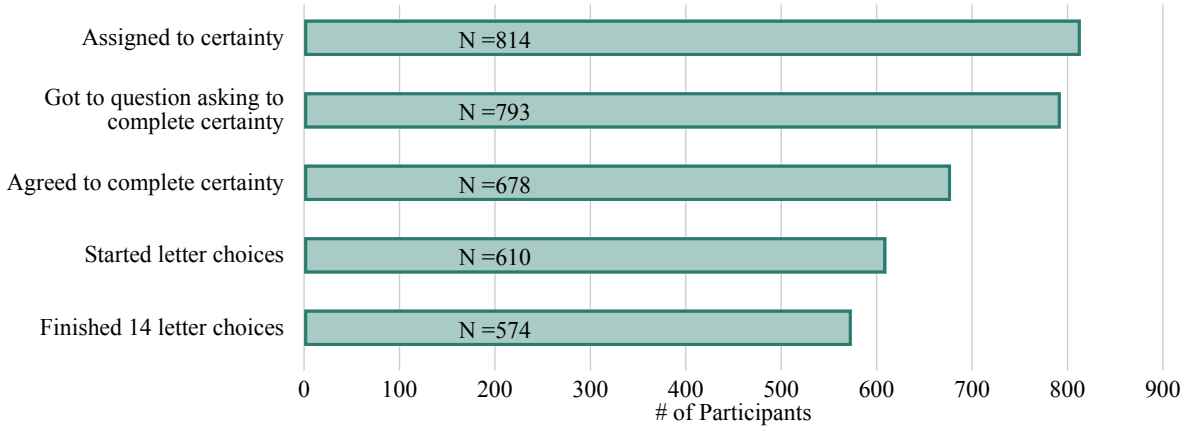



## D.3 The certainty experiments

### *D.3.1 Certainty recruitment*

We recruit participants for the second round of the certainty experiment via ads on social media (e.g. Appendix Figure A1) after finishing main Wave-1 recruitment. The sample is subject to all of the demographic, political, and climate-belief screening as the main Wave-1 sample (see footnote 5). 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 certainty survey, 793 got to the question in which we invite them to take it and 678 agreed to do so. Of these participants, 610 make at least one binary choice, and 574 complete all 14 binary choices.

Figure D15: Recruitment to the letter-certainty experiment




Note: This figure shows the recruitment totals for the certainty-experiment sample. See the text in this a

### *D.3.2 Rounds 1 and 2 of the certainty experiment*

Before our main certainty experiment, we ran an initial version of the experiment with a sample of 459 participants. We randomized these participants to the certainty sample from the main Wave-1 recruitment. After completing the certainty experiment, they went on to complete the incentivized belief elicitations described in Section 6.1.5. This first round followed almost entirely the same design as the certainty experiment described in Section 6.2, with several exceptions. First, first--round participants made choices over only 10 demographic profiles, and we randomized carbon-offsest amounts from $8 to $16 across participants.

More important, 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 certainty 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 certainty-experiment participants.

In another more substantive improvement, the second-round certainty experiment asked more precise questions to elicit participants' beliefs about the effectiveness of emails sent to Congress by different demographic 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 certainty experiment, in contrast, we asked participants two questions to decompose their beliefs about email impact:

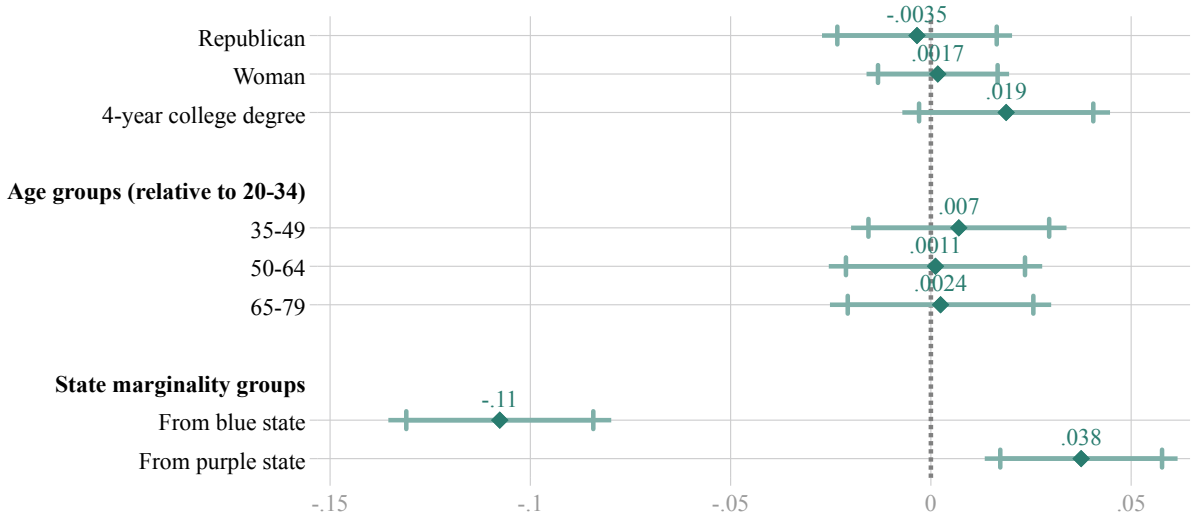
- "Imagine that a climate bill were introduced to Congress in November 2023, and imagine that someone in this group sent a personalized email to their national representatives in Congress about the bill via our form. How likely do you think it is that their national representatives would read the personalized email?" Participants answered from 1 (Definitely won't read) to 7 (Definitely will read).

- "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 second-round questions offer two substantial improvements: they decompose the concept of letter impact and allow for the possibility that emails to Congress about a climate bill could actually oppose it, letting us test our assumption throughout the project that participants expect all emails to support climate policy (Section 2.1.2).

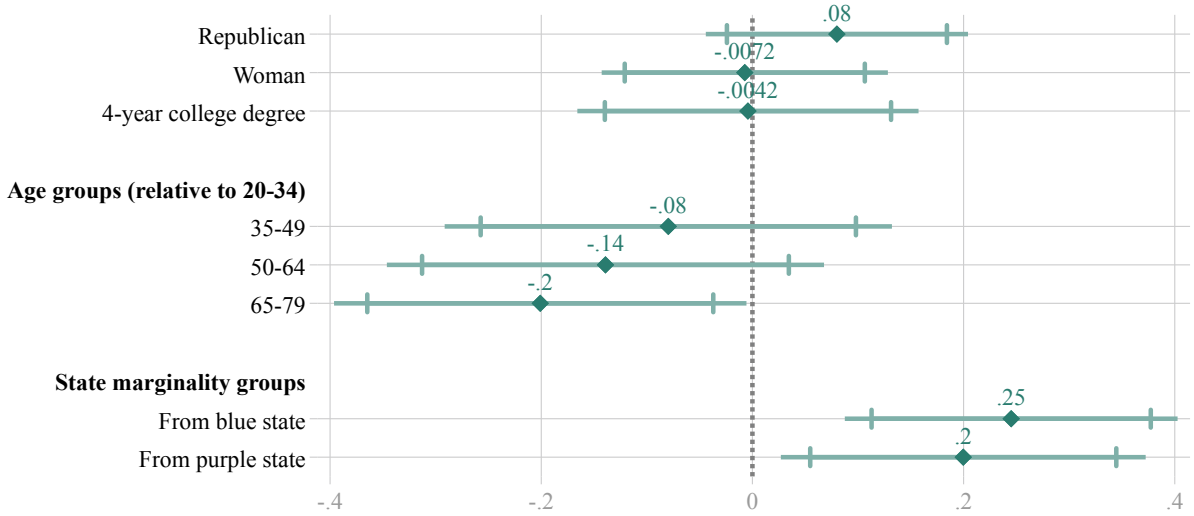
Given the methodological improvements of the second-round experiment, we present these results in the main text. However, Appendix Figures D16 and D17 show that our findings are fully robust to pooling data from both experimental rounds.

Figure D16: Determinants of choosing emails with certainty over carbon offsets, pooling across rounds




Note: This figure parallels Figures 6, but we pool data from both rounds of the certainty experiment. This combination yields a total sample of 12,626 choices across 1,033 participants.

Figure D17: Perceived impact of emails on climate-policy support, pooling across rounds



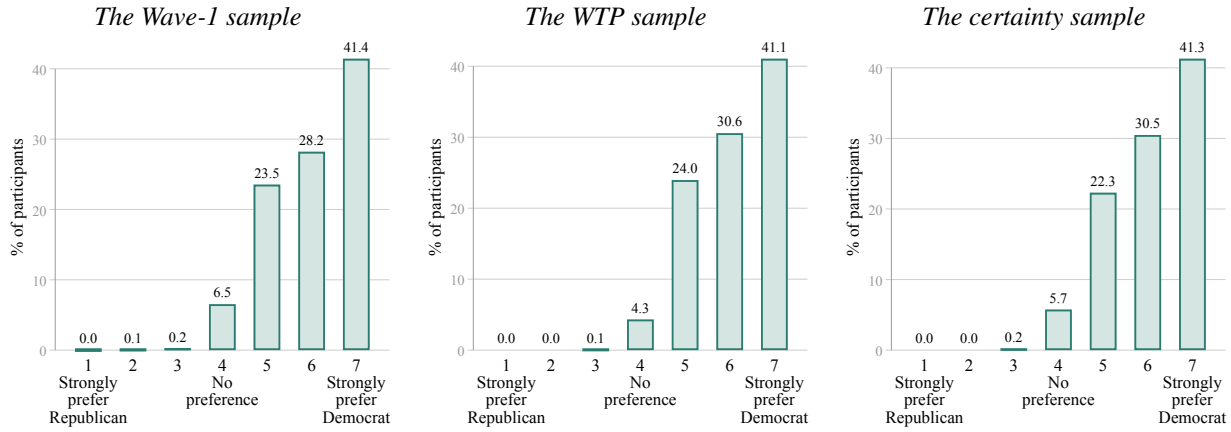

Note: This figure parallels Panel B of Figure 7, except that we pool data across both rounds of the certainty experiment. This combination yields a total sample of 1,002 responses.

## D.4 Measuring Democrats' affective polarization

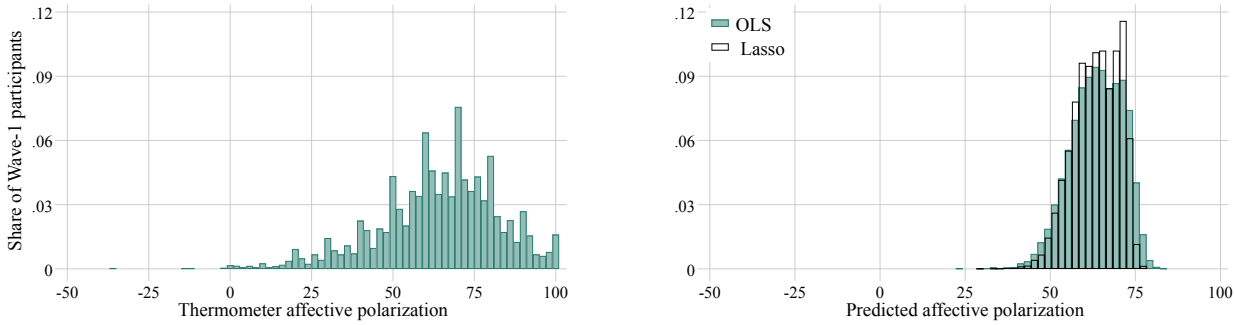
**Measuring affective polarization in Wave 1:** We first measure affective polarization by asking participants at baseline how much they would prefer to be friends with a Republican versus Democrat, from 1 through 7 (Panel A, Appendix Figure A2). At the end of the survey, we elicit a more detailed measure of affective polarization among Invitation and Tell-after participants using the ANES feelings thermometer (Panel B, Appendix Figure A2),[51] defining it as warmth towards the Democratic party minus warmth towards the Republican party. We then predict thermometer-based affective polarization in the full Wave-1 sample using OLS and lasso (Appendix Figure D18).

Figure D18: Distributions of affective polarization

*Panel A. Distributions of preferences for a Democratic vs. Republican friend*




*Panel B. Distributions of raw and predicted thermometer-based affective polarization*




Note: Panel A plots the dsitributions of participants' stated preference for being friends with a fellow Democrat over a Republican, separately in the Wave-1 experimental sample, the WTP sample, and the certainty-experiment sample. In all cases, we elicit this preference on a 7-point scale, where we randomize whether strongly preferring a Republican friend is the left or right endpoint of the scale. Panel B plots the distributions of thermometer-based affective polarization among Invitation and Tell-after participants (N = 4,321) and predicted thermometer-based polarization in the full Wave-1 sample (N = 8,937), predicted from the demographics, climate beliefs, and political beliefs included as controls in the main Wave-1 regressions (Appendix B.2.2).

**Measuring affective polarization in the WTP and certainty-experiment samples:** Unlike in Wave-

[51] We pre-registered that we would not test heterogeneity by this measure due to potential treatment effects, but we find no treatment effects on this polarization measure. See the Online Supplement for more details.

1, we don't directly observe thermometer-based affective polarization for WTP participants or certainty-experiment participants. Thus, we measure affective polarization using preferences for Democratic friends and predicted thermometer-based polarization (via OLS and lasso).

## D.5 The Round-2 WTP experiment: Hiding politics

### *D.5.1 Recruitment for round-two WTP*

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