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

Lucy Page and Hannah Ruebeck
MIT

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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 cooperation across party lines (Hetherington and Rudolph, 2015; Finkel et al., 2020).

We run a series of online experiments with 21,000 participants examining how American build political movements. We home in on three questions. First, do Democrats, members of 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 such 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 an opportunity 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 build the climate movement. 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 capture influence motives—attempts to recruit Wave-2 participants to email Congress—alone; Invitation participants may be more likely to email Congress in part 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 $p = 0.042$. While people thus 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 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

[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 Wave-2 participants' in-survey action.

recruitment gaps by any other trait.

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 $p = 0.029$. Democrats' average 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 other liberals in climate action 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

[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.

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 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); we find that influence motives complement social-image concerns in making publicizing political

[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.

action a powerful mobilization tool.

In doing so, we also add to a small literature finding evidence for influence motives in some pro-social domains but not others (Reinstein and Riener, 2012; Karlan and McConnell, 2014; Esguerra et al., 2023). 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 Motivating descriptive evidence

We start with descriptive evidence on whether and how "typical" climate advocates—Democrats who believe climate change is human-caused (Atske, 2020; Leiserowitz et al., 2023)—seek to build the grassroots climate movement. We recruit Democrats via ads on social media for a survey about climate change, yielding a sample that is highly engaged in climate action. Alongside our experimental sample (see Section 4.3), we recruit about 200 Democrats to document key motivating facts.

Democrats want bipartisanship in Congress on climate: 89% of our sample agrees that building support for climate policy among both Democratic and Republican politicians is crucial for reducing emissions, and 87% agree that the US can only pass future climate legislation if lawmakers

in both parties support it (Panels A and B, Appendix Figure A1). 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 A1). Democrats see a role for themselves in building a bipartisan climate movement: 82% agree that Democrats should try to get more Republicans involved in climate advocacy (Panel B, Appendix Figure A2).

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 A3). While this gap could arise simply because Democrats are primarily close to other Democrats (Panel B, Appendix Figure A3), it could also arise from more subtle beliefs or preferences, like affective polarization.

Democrats in our sample are highly affectively polarized: Standard measures of affective polarization define it as the gap in how warmly Americans rate their feelings toward their own party versus the opposing party on a "feelings thermometer" from 0 to 100 degrees (Iyengar et al., 2019). Democrats in our experimental sample rate the Democratic Party an average of 63 degrees warmer than the Republican Party, compared to an average gap of 56 degrees among Democrats nationally (Panel A, Appendix Figure A4). When asked to rate on a 7-point scale if they would strongly prefer a Republican friend (1) or a Democratic friend (7), 70% of our sample selects six or seven (Panel B, Appendix Figure A4).

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

We explore how Democrats expand the climate coalition in a 21,000-person online network that connects two participant waves: a Wave 1 of Democratic "influencers" and a Wave 2 of "targets" who span the political spectrum. Network participants are semi-anonymous, connected only by demographic profiles (Figure 1a).

We first recruit Wave 1 and give them the opportunity to email Congress about climate change via a form that we embed in our online survey (Section 2.3). Across several experimental designs, we then give some Wave-1 participants costly opportunities to send invitations (Figure 1b) to randomly-paired future Wave-2 participants to join in emailing Congress. We then recruit the Wave-2 targets, randomly and truthfully pass on invitations from Wave 1, and give them a chance to email Congress, allowing us to estimate the invitations' true impact on action.

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. In contrast, Wave-2

participants may belong to or lean towards either the Democratic or Republican parties, but all believe that climate change is mostly human-caused. (Sections 4.3 and 6.2.2 detail Wave-1 and Wave-2 recruitment, respectively.) 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.[5]

Characterizing climate politics by state: We also test whether Democrats strategically recruit climate advocacy by state. Using surveys of Democrats from social media, we construct 13 groups of geographically close states that most participants classify into one of three categories of climate politics: "blue states," where legislators would likely vote in favor of a climate bill even if few residents advocated for it; "red states," where legislators would be unlikely to vote for 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. (See Appendix A.2 for details.) Wave-1 influencers always see the state group in which Wave-2 participants live alongside their politics (Figure 1a), 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.

2.3 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 (Appendix Figure A5).

Whether participants email Congress via our form is an ideal measure of climate action. First, the email form has high external validity; we license it from a software company that provides nearly identical forms to advocacy organizations across the US. Second, sending customized emails is a meaningful form of political action. Politicians systematically misperceive constituents' opinions (Broockman and Skovron, 2018), and citizen lobbying can change politicians' voting behavior (Bergan, 2009; Bergan and Cole, 2015). Finally, emailing Congress in our setting takes effort and may be relatively robust to experimenter demand effects: participants who write spend an average of 8.6 minutes composing their emails. On the other hand, this form of climate action is not prohibitively costly: across our experimental control groups, about 24% email Congress.

[5]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, described below. Feasibly implementing this network structure requires ensuring that all demographic cells in Waves 1 or 2 are sufficiently large. Thus, we restrict both Waves 1 and 2 to participants who live within the contiguous US, are between 20 and 79, identify as a man or woman, and identify as white. Appendix A.1 details how we screen participants.

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.

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 via our survey and can pass on a profile (Figure 1b) inviting Wave-2 participants to do the same. Do invitation choices necessarily capture whether Democrats value recruiting others for climate action? We outline the payoffs to inviting others as follows:

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

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:

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

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, $V(Try\ Influence\ Email)$.[6]

[6]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 $V(Try\ Influence\ Email)$

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 do the same—when telling them can affect their choice—rather than *after*.

3.2 Decomposing influence motives

What beliefs and preferences underlie these influence motives, and how might they differ across party lines? We decompose $V(Try\ Influence\ Email)$ 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 impact of the target's email.

In addition to these strategic returns, participants may derive affective or emotional 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 returns to targets' action—the terms scaled by $\Delta P(Email)$—may also depend on affective returns to knowing that the target is part of the climate movement, $A(Target\ Involved)$.[7]

Predicted heterogeneity in influence motives: Equation 3 predicts key heterogeneity in influence motives: those who believe that invitations have stronger impacts on others—higher "influence beliefs," or $\Delta P(Email)$—and those who are more concerned about climate change, thus valuing others' emails to Congress more highly, will more highly value opportunities to influence others. We will verify these heterogeneity patterns throughout our analysis.

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

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 are the same; we comment further on the validity of this assumption when presenting experimental results.

[7] 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 $A(Try\ Influence)$.

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 in particular.

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 Democrats or Republicans ($A(Try\ Influence\ During|D) \neq A(Try\ Influence\ During|R)$).[8]

Role of affective polarization in strategic drivers: While affective polarization may directly drive gaps in affective returns, second-order or anticipated affective polarization may also generate gaps in Democrats' beliefs about their ability to influence others, $\Delta P(Email)$. In particular, Democrats may estimate $\Delta P(Email|D) > \Delta P(Email|R)$ precisely 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 2). 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 6.2), testing invitations' true impacts on email rates, $\Delta P^*(Email|D,R)$,

[8] Affective polarization may also drive partisan outreach gaps if Americans simply dislike interacting with counter-partisans (Chen and Rohla, 2018). 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.

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: After Wave-1 participants build a basic demographic profile (Figure 1a), 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 study participants.[9] 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 (Appendix Figure B3). We then 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 B4 through B7 shows the text of 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

[9] 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).

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.[10]

Ensuring treatment comprehension: We next ask and correct Invitation and Tell-after participants' answers to a series of incentivized comprehension questions (Appendix B.4), making them as subtle as possible by emphasizing that we ask them to ensure that participants know how we will use their basic information in future.

Can email Congress: Wave-1 participants then decide whether to email Congress: interested participants first opt into the email process, choose whether to continue after learning that the email form will require an address, and finally write and send their emails. Appendix Figures B8 through B12 show the email process.

4.2 Wave-1 empirical specifications

We first test the policy-relevant impacts of opportunities to invite others by estimating 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 the Invitation group, and X_i is a vector of control variables. 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. β_2 captures the effects of knowing that future participants will be told if one emails Congress, while β_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.2.1). 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.[11]

[10] Any Invitation and 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.

[11] 8% of those who start the email process opt out after learning that the form will ask for their address. 20% of those

Control variables: X_i includes participants' demographics, baseline beliefs about climate and politics, and political engagement. Appendix B.2.2 details these controls, and Appendix B.5.4 shows that our results are fully robust to their exclusion.

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 making this choice. 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 (Appendix Table B1, Panel A column 1).

Our main analysis assumes that participants who leave the survey between learning about the opportunity to email Congress and deciding whether to do so would not have emailed Congress had they continued, letting us define the email outcome for all randomized participants. All results are robust to restricting to participants for whom we observe active email choices (Appendix B.5.4).

Sample description: The Wave-1 sample of Democrats well approximates the mainstream environmental movement (column 1, Table 1). This sample is quite politically engaged: 73% report that they've contacted elected representatives about a political or social issue in the last two years, while only 40% of Americans say they've done so in the last 5 years (Oliphant, 2018). Wave-1 participants are also highly educated and wealthy: over 80% have a 4-year college degree or more, and 51% have annual household income over $100,000. Recall, in addition, that the Wave-1 sample is fully white by construction (footnote 5). While our sample therefore cannot represent the full environmental movement, it does reflect a mainstream movement that remains predominately white, affluent, and educated.[12]

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.

[12]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).

Balance: The Wave-1 sample is largely balanced on baseline characteristics across treatment groups (columns 2 through 6, Table 1).[13] There are several small imbalances on age categories and some forms of political engagement, but our results are fully robust to baseline controls for these traits (Appendix B.5.4).

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 B16).

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' political leanings, gender, age, education, and state group (Panel A, Appendix Figure B17).

4.4 Wave-1 results: Democrats act to mobilize others

4.4.1 Impacts of opportunities to invite others

Opportunities to invite others to join in emailing Congress substantially increase climate advocacy. 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).

Separating out influence motives: At least 20% of the total Invitation effect arises from participants' efforts to influence whether Wave-2 participants email Congress during the survey itself. Invitation participants are 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). These influence-motive effects amount to 7% and 9% of the relevant control means, respectively.

At the same time, assignment to the Tell-after arm, where others will know that you emailed Congress during the survey but it cannot affect whether they do the same, makes Democrats 12pp (26%) more likely to opt into the email process and 13pp (43%) more likely to match to an email record (column 2, Table 2). These large effects likely derive in large part from self- or social-image concerns, which have been shown to be strong drivers in other political and non-political domains (Bursztyn and Jensen, 2017; Dellavigna et al., 2017).

However, the Tell-after effects likely also reflect influence motives: attempts to affect targets' action after the survey or (mistakenly) during the survey itself. 54% of Tell-after participants

[13]The sample is also largely balanced when also split by whether participants are paired with Wave-2 Democrats or Republicans in their matched demographic group. Results are available upon request.

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 (Appendix Figure B18, Panel A), and about 60% say it would be at least somewhat likely to do so (Appendix Figure B18, Panel B). Moreover, 28% of Tell-after participants incorrectly state that their choice to email Congress could affect targets' action during the survey itself (Appendix Figure B16, Panel B), so the Tell-after affect also captures these mistaken influence motives.

Validating that we capture influence motives: Several forms of evidence help to validate that our estimates for differential Invitation effects (β_3) capture influence motives. First, we find suggestive evidence that this effect is positively correlated with influencers' baseline climate worry and estimates for $\Delta P(Email)$, the impacts of invitations on Wave-2 action (Appendix Figure B19), both of which our framework predicts should drive stronger influence motives (Section 3.2). We also find no evidence that mechanisms other than influence motives drive the β_3 effect.[14]

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Robustness: Our main Wave-1 results are robust to redefining the sample, including restricting to those who correctly understand the experimental set-up, to changing controls, and to correcting for differential demand effects (Appendix B.5.4).

4.4.2 Democrats may act to influence liberals more than conservatives

The Wave-1 experiment provides suggestive, though underpowered, evidence that Democrats differentially act to mobilize Wave-2 Democrats rather than Republicans. Section 5 explores these partisan gaps in more detail.

Partisan gaps in the total Invitation effect (β_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

[14] One 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. The β_3 coefficient could also capture a mechanism other than influence beliefs if Invitation participants expect other survey-takers to also have opportunities to influence others' action and revise their beliefs about aggregate email rates via our survey in response. The impacts of any such belief updating on action are theoretically ambiguous (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 1, Appendix Table B3).

(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 (β_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 β_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.[15,16] We find no evidence of larger partisan gaps among those who are more polarized against Republicans (Appendix Figure B22).

5 The WTP experiment: Whom do Democrats recruit?

While the Wave-1 experiment shows that Democrats act to build the climate movement overall, it lacks power to test *whom* they try to recruit. In a sub-experiment among those who email Congress in the Wave-1 Control group, participants make a series of binary choices between inviting particular survey-takers to email Congress and delegating carbon-offset donations. This "Willingness-to-pay (WTP) experiment" shows that Democrats differentially try to mobilize co-partisans, and these partisan gaps dwarf attempts to mobilize political action by any other trait.

5.1 WTP experimental structure

Each WTP participant is paired with a group of 20 past study-takers, each of whom we will re-contact to take a second survey during which they can choose to email Congress.[17] When these study-takers return, one of them will be randomly paired to see a profile of the WTP participant. (Appendix Figures C25 and C26 show how we present this and the following set-up to participants.) Participants see demographic profiles for each of their 20 possible matches (Appendix Figure C27), who are all real people that vary in their demographics and political leanings.

[15] In contrast, we find no consistent evidence that Democrats differentially act to influence Wave-2 targets by demographics or non-political similarity (Appendix Figure B19).

[16] Our results are consistent with Democrats facing differential image concerns from telling co- versus counter-partisans that they emailed Congress, as the effects of the Tell-after treatment may be larger when Wave-1 Democrats are paired with Democrats (columns 4 and 6, Table 2). We cannot distinguish differential image concerns from differential effort to influence targets after the survey.

[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 matches' behavior.

Choosing profile content: For each possible match, participants choose (at a cost) whether to include that they emailed Congress in the profile that match would see:

- *Option 1. Basic profile and carbon offsets:* We show the returning match the WTP participant's basic demographic profile (Figure 1a) and add a fixed amount, randomized at $3, $4, $5, or $6 across participants, to a carbon offset purchase that our research team would make.[18]

- *Option 2. Extended profile:* We show the returning match an extended profile that includes the WTP participants' demographics and says that they emailed Congress.[19] We detail these profiles below.

Thus, participants trade off between offset donations and telling others that they emailed Congress. Appendix Figure C28 shows a sample choice.

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 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 C27) communicates this timing: those who would see one's profile *before* versus *after* deciding whether to email Congress are labeled as "Hasn't been asked" and "Already decided," respectively.

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 the extended profile *after* deciding would see the profile shown in Figure 1c. We then identify attempts to mobilize others by testing whether WTP participants are more likely to tell possible matches that they emailed Congress when targets would see their profile *before* rather than *after* deciding themselves whether to email Congress, and thus when doing so can affect targets' action.

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

[19] One concern is that option 2 can only be implemented if the possible match in fact returns for a follow-up survey. If WTP participants expect higher attrition for some possible matches, these beliefs could drive spurious heterogeneity in participants' choices across matches. We eliminate this risk 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.

Varying climate-belief information: Finally, we randomize WTP participants either to see profiles of possible matches that show that matches believe climate change is human-caused or that hide these beliefs (Appendix Figure C27). This cross-participant variation lets us test how inferences about climate beliefs drive any gap in Democrats' attempts to recruit other Democrats versus Republicans.

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. β_1 captures whether Democrats in the WTP sample are more or less likely to tell a Republican match that they emailed Congress when they can do so before, rather than after, that match's email choice, and β_3 tests whether this "influence" gap is larger when paired with Democrats. Finally, β_3 tests whether Democrats are more or less likely to tell Democrats, rather than a Republicans, 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, α_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.6).

5.3 WTP sample recruitment, summary, and fidelity

Attrition: In total, 1,350 participants begin the main WTP survey after emailing Congress from the Wave-1 Control group (see Appendix C.2 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 treatment arm (column 2, Appendix Table C4).

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 process, 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 C5). The WTP sample is largely balanced between those who see or do not see information on matches' climate beliefs

(columns 3-5, Appendix Table C5). In our main analysis, participant fixed effects control for any remaining baseline differences.

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 (Appendix C.3). Participants initially answer an average of 5.8 questions correctly (Appendix Figure C29), and our results are robust to restricting to participants who initially answer at least half of the questions correctly (Appendix C.6).

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

Overall influence motives in the WTP set-up: As in the Wave-1 experiment, WTP participants are motivated to recruit others to email Congress. 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[20] 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]

The partisan influence gap: 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).[23]

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

[20] Unfortunately, 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 C30.)

[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 a giftcard.

[22] We estimate larger $Before_{ij}$ effects among WTP participants with higher estimates for $\Delta P(Email)$ (Appendix Figure C31), consistent with this effect capturing influence motives (Section 3.2).

[23] We find no evidence that this partisan influence gap is larger among more affectively-polarized WTP participants (Appendix Figure C32).

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.

Targeting by non-political traits and similarity: WTP participants more strongly target recruitment by partisanship than by any other match traits (Panel A, Figure 3). When they lack information on matches' climate beliefs, participants more strongly try to influence women, those who live in blue or purple states, and those with 4-year college degrees, though these gaps are much smaller than their differential efforts to mobilize Democrats. Moreover, these gaps shrink substantially or disappear when participants know that all matches believe climate change is human-caused, while the partisan influence gap remains large. Notably, there is no evidence that Democrats differentially try to mobilize advocacy in red or purple states, where they might believe this advocacy to be more impactful (Appendix A.2).[24]

Targeting by political match also dwarfs differential recruitment by non-political similarity (Panel B, Figure 3). 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 or score 1sd higher on predicted non-political similarity.[25] 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 changes in sample definition, the control variables that we include, and corrections for demand effects (Appendix C.6).

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?

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, second-order affective polarization shapes these strategic beliefs: Democrats estimate that hiding their own political leanings from invitations to Republicans would make them three times as effective.

[24] We also find little evidence that WTP participants differentially target Democrats versus Republicans by state politics (Appendix Table C6). If anything, Democrats are especially unlikely to try recruiting Republicans in red states, where Republicans could be especially impactful.

[25] We predict participants' non-political similarity to matches using a linear model calibrated on Wave-1 participants' elicited similarity to Wave-2 pairs (Appendix C.5).

6.1 Eliciting Democrats' influence beliefs: $\Delta P(Email)$

We first measure and test the accuracy of Democrats' beliefs about the effects of their invitations on whether Democrats and Republicans email Congress, $\Delta P(Email)$. We recruit a "belief sample" of Democrats alongside Wave 1 and ask each to estimate how many out of 100 Democratic and 100 Republican participants in a particular Wave-2 demographic cell would choose to email Congress if they did or did not see invitations (Figure 1b) from matched Wave-1 Democrats. Appendix Figures D36 through D38 show how we elicit these beliefs. In total, 397 participants complete all four belief elicitations, of whom 194 were incentivized for accuracy (Appendix Figure D39).[26,27] We focus throughout this section on belief elicitations from incentivized participants, estimating the following regressions:

$$ProbEmail_{ij} = \alpha_i + \beta Profile_{ij} + \varepsilon_{ij} \tag{7}$$

where $ProbEmail_{ij}$ gives participant i's guess of how many of 100 participants in group j would email Congress, and $Profile_{ij}$ indicates cases where group j would see invitations. α_i are individual fixed effects. Here, β captures Democrats' average estimates for invitations' impacts on Wave-2 participants' likelihood of emailing Congress, $\Delta P(Email)$. We estimate this regression separately on beliefs for action by Wave-2 Democrats and Republicans, then testing whether Democrats perceive equal impact on co- and counter-partisans' email rates.

Results: Democrats estimate lower ΔP for Republicans. Democrats in the belief sample estimate 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 (Figure 4).[28,29] We reject that these coefficients are equal (p = 0.01). In contrast, the belief sample predicts no differential influence by any form of non-political similarity between senders and recipients (Appendix Figure D42).

[26] We elicit participants' beliefs for the impacts of invitations between demographic groups that were actually randomly paired between Waves 1 and 2 (Section 2.2), letting us incentivize accuracy. 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).

[27] The final belief sample matches Wave-1 on climate beliefs and political engagement, while difffering on some demographics (Appendix Table D7). Like the main Wave-1 sample, the belief sample is less politically engaged and concerned about climate change than the WTP sample.

[28] 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 D40). This gap could stem from lower effort or motivated reasoning.

[29] Besides calculating average values for $\Delta P(Email)$ in Equation 7, 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 D41). 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 targets (Panel B, Appendix Figure D41). About 56% of the sample estimates that $\Delta P(Email|D) > \Delta P(Email|R)$ in their matched Wave-2 group (Panel C, Appendix Figure D41).

6.2 The Wave-2 experiment: Democrats' $\Delta P(Email)$ beliefs are accurate

We next estimate the true impacts of Democrats' emails in the Wave-2 sample. Democrats' influence beliefs along and across party lines are broadly accurate.

6.2.1 Wave-2 experimental structure

Summary: 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 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; see Panel A of Appendix Figure E1 for a sample profile and explanation. Note that we show all participants an earlier participant at this stage to ensure that impacts of invitations do not arise just from seeing details about a Wave-1 participant. To encourage Wave-2 participants to attend to their match, we ask them to rate and explain how similar they are to this recent participant.

Treatment: Show participants invitations from Wave-1 match: After a series of questions about their climate beliefs, we show all participants a preview of the upcoming opportunity to email Congress (Appendix Figure B3). 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 (Panel B of Appendix Figure E1).[30] 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.

Choose whether to email Congress or not: All Wave-2 participants then decide whether to email Congress or not.

6.2.2 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.

[30] 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. Appendix E.1 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 E.2.1 details Wave-2 recruitment.

Attrition: 92% of participants randomized to a Wave-2 treatment advance through sending an email to Congress or not, with no differential attrition by treatment (column 1, Appendix Table E1). We define the Wave-2 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, Table 4). Besides being older, more likely to live in blue or purple states, more educated, and wealthier than Wave-2 Republicans, Wave-2 Democrats report higher baseline political engagement, concern about climate change, and preference for friends of their own party.[31] Section E.5 shows 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 4).

Set-up comprehension and attention: Wave-2 comprehension is high.[32] Treated participants are 60pp more likely to state that their paired Wave-1 participant emailed Congress when they took our survey (Panel A, Appendix Figure E5). 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 (Panel B, Appendix Figure E5).

Wave-2 participants also attend to matches' demographics, so they may react differentially to invitations by Wave-1 traits. In multiple-choice questions at the end of the survey, participants correctly recall matches' party, gender, age, and state group over 85% of the time (Panel A, Appendix Figure B17). See Appendix E.2.3 for detail on Wave-2 comprehension checks.

[31] While some of these gaps, such as in educational attainment, mirror demographic gaps across parties in national samples (ANES 2020), others diverge. For example, Democratic- and Republican-leaners report about equal political engagement in national samples (Oliphant, 2018). These differential gaps in our survey in part reflect the differential selection induced among liberals and conservatives when restricting to those who believe climate change is mostly human-caused—88% of Democrats and only 35% of Republicans say so in national samples (Fuong and Skelley, 2022)—a form of differential selection that is also at work in the real-world climate movement. More artificially, the differences between liberals and conservatives in the Wave-2 sample also arise in part from the fact that we largely recruited liberals from ads about climate change on social media and recruited conservatives from Qualtrics. When we restrict to participants recruited from Qualtrics (Appendix Table E2), liberals more closely match Wave-2 conservatives on income, education, and baseline political engagement. However, they remain more highly educated, wealthy, concerned about climate change, politically engaged, and affectively polarized.

[32] As in Wave 1, participants are only included in our final sample if they successfully pass an attention check in which we explicitly ask them to choose a particular option in a multiple-choice question.

6.2.3 *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:

$$Contact_i = \alpha + \beta Treatment_i + \Phi X_i + \varepsilon_i \tag{8}$$

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

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. 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.

6.2.4 *Wave-2 results*

Panel B of Figure 4 plots true rates of action in the Wave-2 sample, with and without invitations, against Wave-1 Democrats' beliefs for these values.[33]

Larger absolute impacts on Democrats: While Democrats overestimate the overall rate of action among Wave-2 Democrats and Republicans, their incentivized beliefs for the impacts of invitations are quite close to the truth. About 25% of Wave-2 Democrats in the control group match to email records, and seeing an invitation increases this rate by 5.8pp (23%)—compared to the belief sample's estimate of 6.2pp. Only 8% of Wave-2 Republicans in the control group match to an email record, but they also react to invitations from Wave-1 participants: these invitations make Republicans 2.1 pp (25%) more likely to match to an email record—compared to the belief sample's estimate of 3.2pp.[34] We cannot reject here that the belief sample's beliefs are accurate on average.

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

[34] Appendix Table E6 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 Democrats start the email process is substantially larger among those recruited from social media (N = 163) than those recruited from Qualtrics (N = 2791), we do not interpret this small-sample result. All future Wave-2 analysis pool respondents recruited from the two sources.

We can reject (p = 0.023) that invitations have the same absolute impact on whether liberals and conservatives match to email records. Section E.5 below shows that these and the following Wave-2 results are highly robust to changes in sample restrictions, controls, and corrections for experimenter demand effects.[35]

Democrats underestimate the role of non-political similarity: Wave-2 participants respond more to invitations from senders who are like them in non-political ways, but the impact of sharing these other traits is smaller than that of shared politics (Appendix E.4.2). The impacts of invitations on email records rise by about 1.4pp for each non-political demographic trait (i.e. age, gender, educational attainment, or state) that Wave-1 senders and Wave-2 recipients share (Appendix Figure E7), though this coefficient is statistically indistinguishable from zero. Increasing a continuous measure of Wave-2 participants' perceived non-political similarity to their Wave-1 match by one standard deviation increases the invitations' impacts by a statistically-significant 1.8pp.

However, Democrats underestimate how much non-political similarity between senders and recipients increases invitations' impacts (Appendix D.1.1).

Wave-2 robustness: The Wave-2 results are highly robust to changes in the sample definition, to changes in the control variables we use, and to controlling for experimenter demand effects (Appendix E.5).

6.3 No partisan gap in preferences for emails with certainty

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

6.3.1 The certainty experiment: Experimental design

We run two rounds of a "certainty experiment," in which Democrats make incentivized binary choices between carbon-offset donations and emails to Congress, sent with certainty, from a range of demographic groups. While these rounds differ slightly in experimental design, their basic structure remains the same and we pool the data for analysis. In both rounds of the certainty

[35] Proportionally, then, Wave-1 Democrats' invitations have very similar impacts on climate advocacy by liberal and conservative recipients. While the proportional effects on action could shape the emotional returns to recruiting liberals or conservatives to act, the *absolute* impacts of invitations on emails are what matter for the "effective-altruist" value of inviting others to join in climate action (see footnote 7).

experiment, we show participants a series of profiles for 10 to 14 demographic groups (Appendix Figure B2); these profiles are evenly split between leaning towards the Republican and Democratic parties.[36] For each demographic group, certainty participants choose between two options:

Option 1. Carbon offset donation: The research team donates $8 and $16 to carbon offsets. As in the WTP experiment, we benchmark the dollar offset amount to driving distances (see footnote 18).

Option 2. Enlist an email with certainty: We enlist a participant like the demographic group shown to email Congress about climate policy through our form. We reiterate to participants that choosing this option means that we would enlist someone to write a letter with certainty, and that it is easy for us to enlist someone like each group shown.[37] (See Appendix E.6.1 for the full text of the survey explanation.)

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

Recruitment and sample summary: In total, 459 participants and 574 participants complete all 10 choices in rounds 1 and 2 of the certainty experiment, respectively. See Appendix E.6.3 for more detail on the recruitment process. Like the belief sample, participants in the final certainty sample are somewhat younger and wealthier than the main Wave-1 action sample (Appendix Table D7); certainty participants are also more likely to be women and more likely to have a college degree. However, they match the Wave-1 sample in climate beliefs, political beliefs, and political engagement.[38]

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

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

where $ChooseEmail_{ij}$ indicates that certainty participant i chooses an email from demographic

[36] While participants in the first-round certainty experiment see 10 demographic profiles, those in the second round see 14 profiles. In the first round of the certainty experiment, we constructed these sample profiles from full profiles of past study participants, including politics. In the second round, we instead cross-randomized politics with respect to non-political demographic profiles of past study participants. See Appendix E.6.2 for more details on the process of creating these profiles and for details on the design differences between the two experimental rounds.

[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 could be prohibitively costly for us to implement. For the sake of our experimental design, the only requirement is that participants expect us to enlist someone matching each demographic trait shown with enough certainty to react to those traits.

[38] Samples in the two rounds of the certainty experiment differ somewhat in demographics, climate beliefs, and political attitudes (Appendix Table E11). For example, participants in the second-round certainty sample are substantially more likely to be women and over age 40 than those in the first round.

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

6.3.2 Democrats choose emails differentially by state, but not by partisanship

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

On the other hand, Democrats show no preferences for emails by senders' other demographics conditional on state: there are no distinguishable partisan gaps in email choices, either across the full set of possible email-writers or separately in red, blue, or purple states (Appendix Table E12).

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

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

While our motivating survey suggests that Democrats expect letters from conservatives to be more impactful in achieving climate policy progress than letters from liberals in the aggregate, Democrats in our sample may not perceive or attend to this differential impact when considering emails from individual Americans across the political spectrum. To clarify these determinants, we ask participants in both rounds of the certainty experiment to rate how impactful they would expect an email to Congress to be from one particular demographic group.[39] In Appendix Table E12, we

[39]We phrase this question differently in the two rounds of the certainty experiment. In the first round, we simply ask

regress these standardized impact beliefs on the set of email-writer traits.

We find no gap in perceived impact between liberals and conservatives when pooled across state groups (column 5), though we find marginally-significant evidence that Democrats perceive emails from conservatives to be more impactful than those from liberals in red states (column 6). While we interpret these impact measures with caution,[40] they suggest that Democrats do not perceive differential impact from emails by liberals versus conservatives when making choices over individual emails. Combining the null partisan gap in email choices with the null partisan gap in perceived impacts, therefore, we find no evidence that Democrats have differential affective preferences over having a liberal versus conservative engaged in climate action.

6.4 But second-order affective polarization shapes Δ P

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

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

Revised WTP experiment: To explore these remaining questions, we run a revised WTP experi-

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

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

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

6.4.1 Experimental design, recruitment, and fidelity

Experimental structure: The round-two WTP experiment follows the same procedure as the main WTP experiment described in Section 5.1, with two key alterations. First, while the original WTP experiment randomized some participants to see profiles of possible matches without information about their climate beliefs, all participants in the round-two WTP experiment see profiles that include matches' climate beliefs. Next, we randomize half of round-two WTP participants to have the option to hide their own political leanings from the basic or extended profiles that they pass on to each possible match. (See Appendix E.7.1 for the explanation to participants). Participants with the option to hide politics choose whether to do so or not separately for each possible match on the same screen on which they choose whether to pass on a basic or extended profile (Appendix E.7.2).

Recruitment: We recruit participants for the round-two WTP experiment using ads on Facebook. We invite participants to take the additional WTP survey section if they report that they emailed Congress, and 1,420 participants start the round-two WTP survey in total. Appendix E.7.4 details recruitment.

Attrition: Midway through explaining the WTP experimental set-up, we randomize the 1,278 participants who remain to either have or not have the option to hide politics. A total of 995 participants (70% of those who start the experiment and 78% of those randomized) complete all 20 binary choices, with no differential attrition by treatment arm (column 3, Appendix Table C4).

Sample description and balance: Columns 9 through 12 of Appendix Table D7 summarize the demographics of the round-2 WTP sample. Like the main WTP sample, these participants are more concerned about climate change and politically engaged than the main Wave-1 sample (column 10). The round-two WTP sample is older and somewhat less likely to identify as women than the main WTP sample, but it matches this sample in climate beliefs, political beliefs, and political

engagement (column 11). The sample is fully balanced across treatment arms (Appendix Table E13).

6.4.2 The role of second-order affective polarization

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

Choices to hide politics: WTP participants are much less likely to show conservative matches their own political leanings (Figure 7). While participants show their politics to about 91% of liberal matches, regardless of when those matches would see their profile, they show their politics to only 34% and 44% of conservatives matches who would see their profiles before and after deciding whether to email Congress, respectively. These patterns suggest that Democrats may differentially hide their politics from conservatives both due to social-image concerns and to increase their impacts on conservatives' action.

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

Democrats with the option to hide their politics estimate that an invitation without their own political leanings would make conservatives 11.4pp more likely to email Congress, while an invitation identifying them as a liberal would increase conservatives' action by only 3.4pp. On the other hand, they estimate that hiding their political leanings would somewhat decrease the impact of invitations on liberals' action from 13.3pp to 9.8pp. These patterns suggest that second-order affective polarization plays an important role in Democrats' beliefs about their ability to spread the climate movement across the political aisle: they expect conservatives to respond less to their invitations precisely because they are liberals.[41]

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

Under reasonable assumptions, the ability to hide one's politics should only impact the partisan influence gap in the WTP experimental set-up (coefficient β_3 in equation 6) through its effects

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

on $\Delta P(Email|D) - \Delta P(Email|R)$, the gap in Democrats' beliefs about their ability to influence conservatives versus liberals. Then, we could in theory use the round-two WTP experiment to benchmark the role of this partisan belief gap in Democrats' differential efforts to reach out to liberal versus conservative matches. Unfortunately, we find that allowing WTP participants to hide their politics in each profile does not substantially affect the partisan belief gap; thus, we cannot use this strategy.

The effective first stage on beliefs: Even while participants with the option to hide their politics predict that invitations with and without their politics would have substantially different impacts, we do not find that giving people the option to hide politics shrinks the partisan belief gap. This null effect arises because participants who cannot hide their politics are more optimistic about the impacts of baseline profiles on conservatives: while participants with the option to hide their politics estimate that an invitation showing their politics would make a conservative only 3.2pp more likely to email Congress (Figure 8), participants who cannot hide their politics estimate that the same invitation would make conservatives 8.5pp more likely to email Congress (Appendix Figure E18).[42] Taking these belief elicitations at face value and assuming that they are homogeneous within treatment groups, we would estimate that the partisan belief gap only falls from 6.1pp among those without the option to hide politics to 4.5pp among those who can hide politics. Moreover, the large divergence in beliefs about baseline profiles' impacts across the treatment groups suggests that it may be unreasonable to directly compare the elicited beliefs across these groups.

No impact on influence attempts: Consistent with the null first-stage effects on participants' beliefs about their relative impact on liberal versus conservative matches, the option to hide one's politics does not significantly reduce the gap in WTP participants' efforts to mobilize liberal versus conservative matches (Figure 6). With or without the option to hide one's politics, Democrats are about 19% less likely to try to influence conservatives than liberals. We cannot distinguish whether this null effect arises from a true null effect on how much influence participants believe they can have on liberals versus conservatives, or because Democrats have differential preferences over directly trying to engage liberals versus conservatives in action during the survey, $A(Try\ Influence\ During|D,R)$. Nevertheless, our results here are fully consistent with Democrats' beliefs about their impacts on liberals versus conservatives driving their reluctance to reach across the aisle.

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

6.5 Summing up: Drivers of the partisan influence gap

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

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. Rather, Democrats' reluctance to reach across party lines can be fully explained by their accurate beliefs that their invitations will have less impact on action by Republicans. However, the broader context of strong affective polarization still plays a key role. *Second-order* 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 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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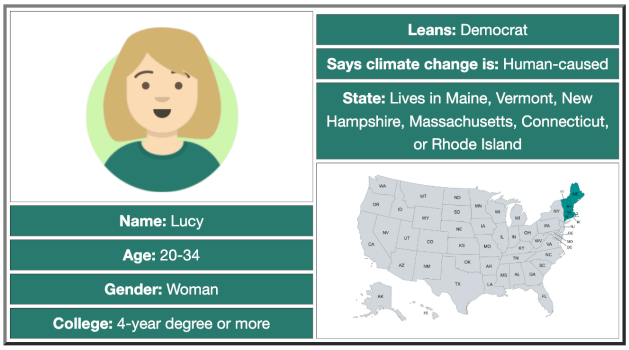
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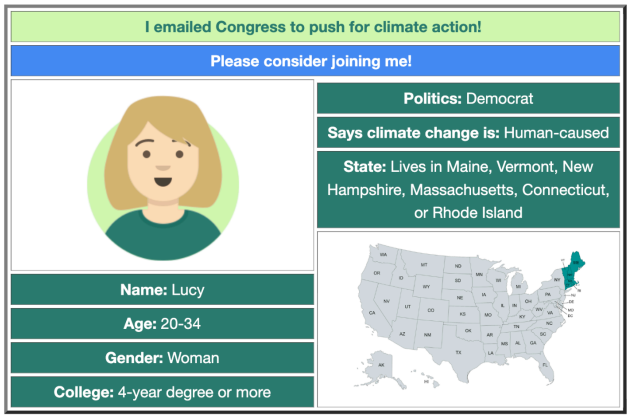
8 Main-text figures

Figure 1: Demographic profiles and invitations to act

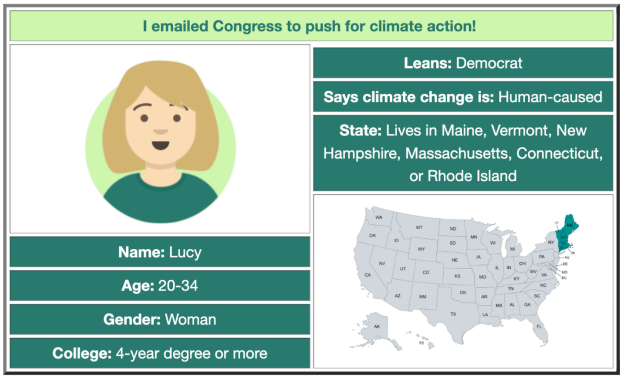
(a) Sample basic demographic profile



(b) Sample invitation for Wave-2 participants

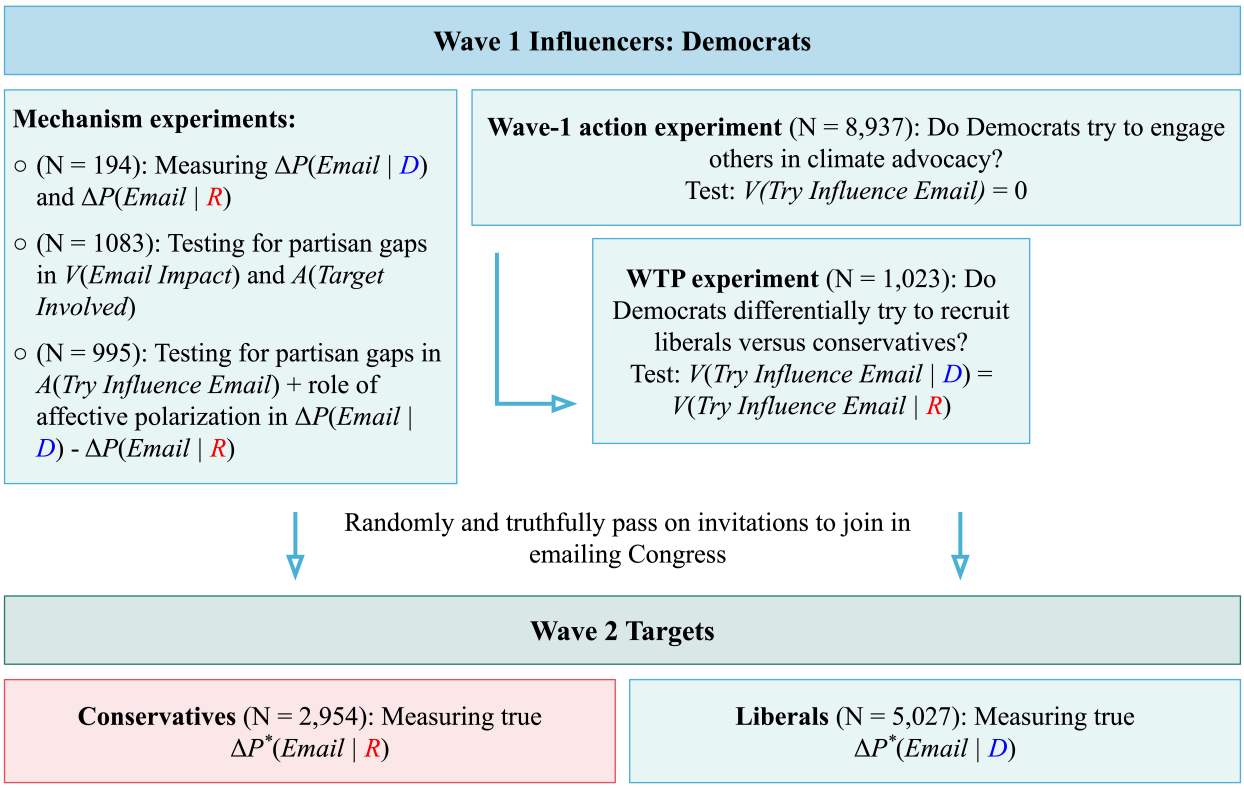


(c) Sample profile saying emailed Congress



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.

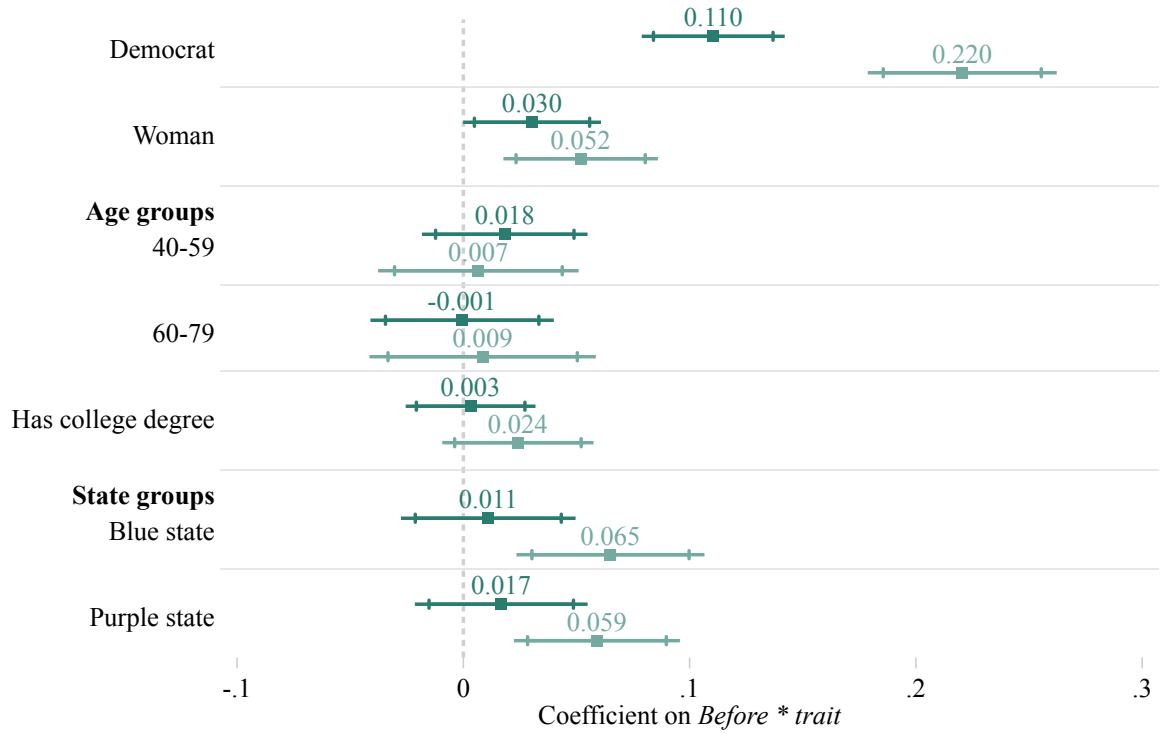
Figure 2: An overhead view of the project design



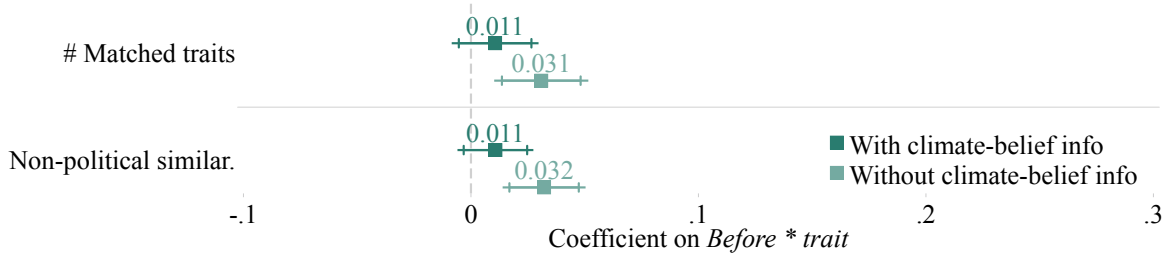
Note: This figure provides an overview of the interlocking experiments that contribute to this project. The experiments rest on a network connecting two waves of participants: a Wave 1 of Democrats who have oppportunities to email Congress and and to invite Wave-2 targets to join them in doing so. Wave-2 participants are split include both those who lean towards the Republican party ("conservatives") and the Democratic party ("liberals.") 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 liberals or conservatives, 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 liberals and conservatives email Congress.

Figure 3: Differential WTP influence attempts by other match features

Panel A. By matches' demographic traits

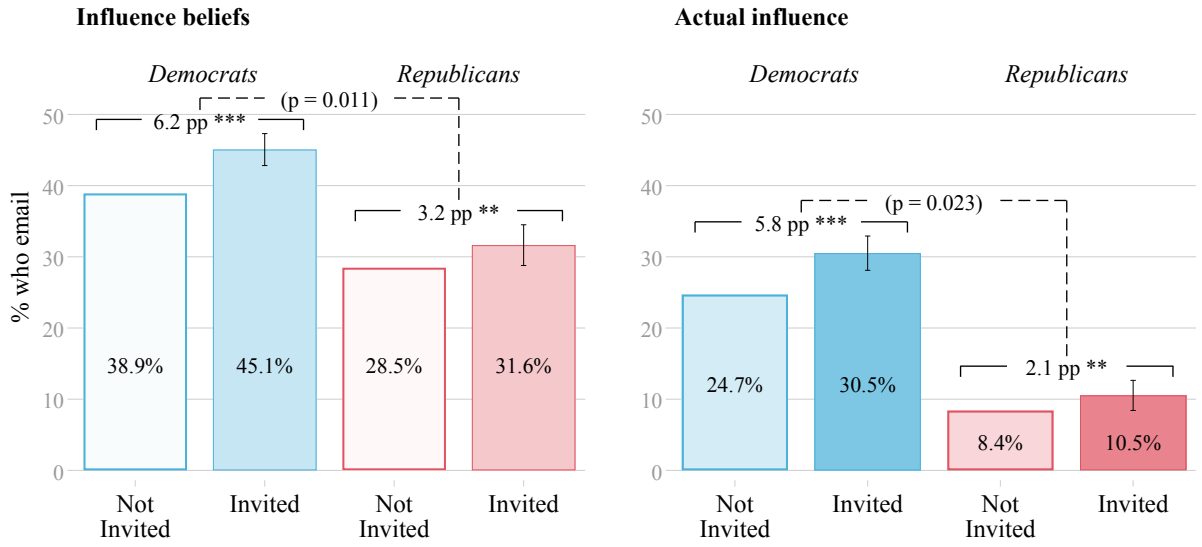


Panel B. By non-political similarity



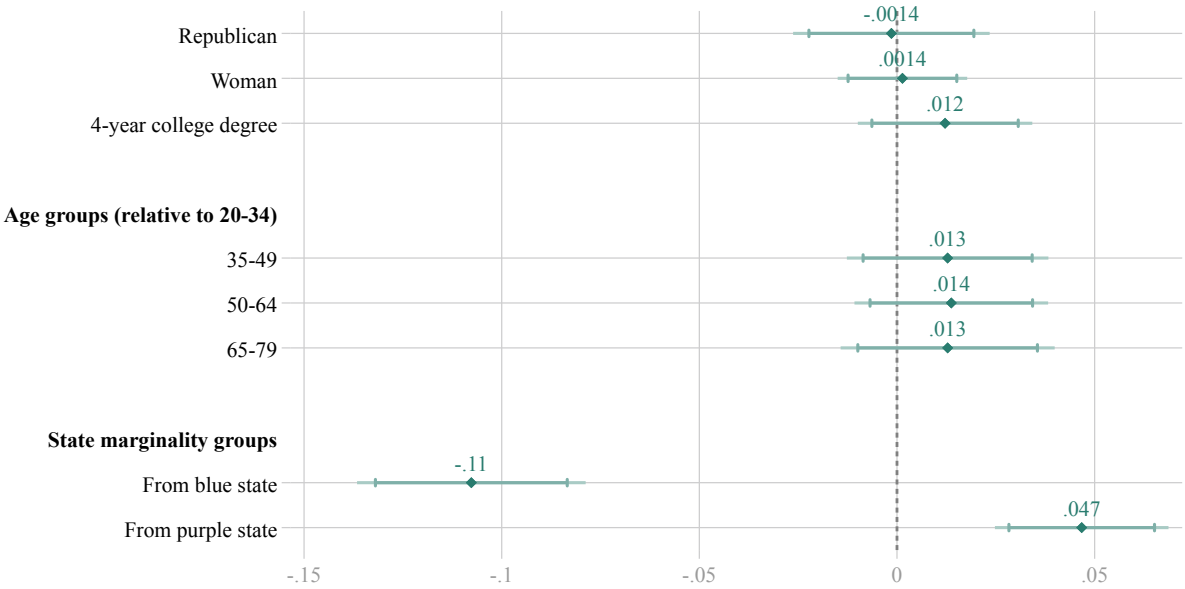
Note: Panel A tests whether WTP participants differentially seek to mobilize possible matches with certain demographic traits, estimated in a variant of equation 6 in which we jointly include indicators for each trait and their interactions with $Before_{ij}$. The omitted categories for age and state groups are ages 25-39 and Red states, respectively. Panel B tests whether WTP participants differentially seek to mobilize possible matches with higher non-political similarity, measured as the number of non-political traits on which they match or with an index of predicted non-political similarity, calibrated on Wave 1's reported similarity to Wave-2 matches (Appendix C.5). We estimate these effects in regressions also controlling for an indicator that the match is a Democrat and its interaction with $Before_{ij}$; coefficients on the political-match interaction here are quite close to that in Panel A. Appendix Figure C31 estimates differential influence motives when WTP participants and matches share particular demographic traits. 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 4: Overall influence beliefs by party affiliation, compared with the truth



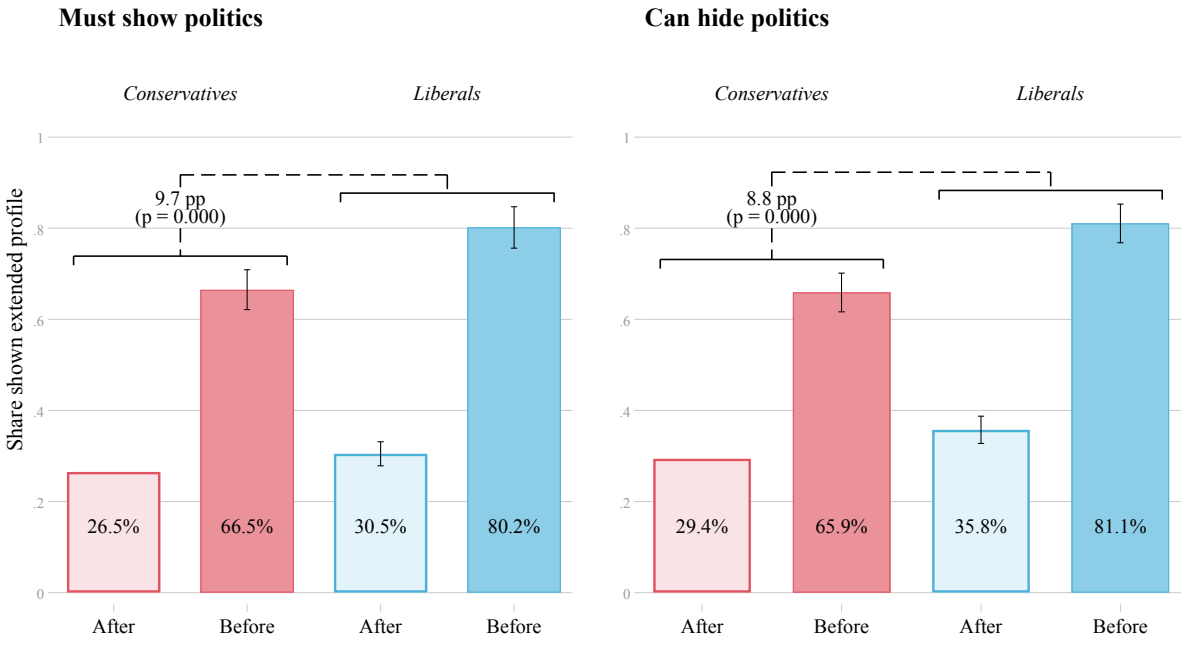
Note: The left panel of this figure plots average incentivized beliefs about the share of liberal and conservative Wave-2 participants who would email Congress if they do or do not see invitations from Wave-1 participants to join in action. We estimate these effects among 194 participants in the "belief sample," each of whom predict the share of liberal and conservative participants in their matched Wave-2 group who would email Congress if they did or did not see a Wave-1 invitation. The figure reports participants' beliefs for the impact of invitations separately among liberals and conservatives, as well as the heteroskedasticity-robust p-value from a test of the null that participants estimate the same imapct across these groups. The right panel of this figure plots the actual impacts of invitations on whether Wave-2 liberals and conservatives match to email records (Table 5). We estimate these impacts among 5,027 liberals and 2,954 conservatives in the Wave-2 experimental sample (Section 6.2). 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 5: Determinants of choosing emails with certainty



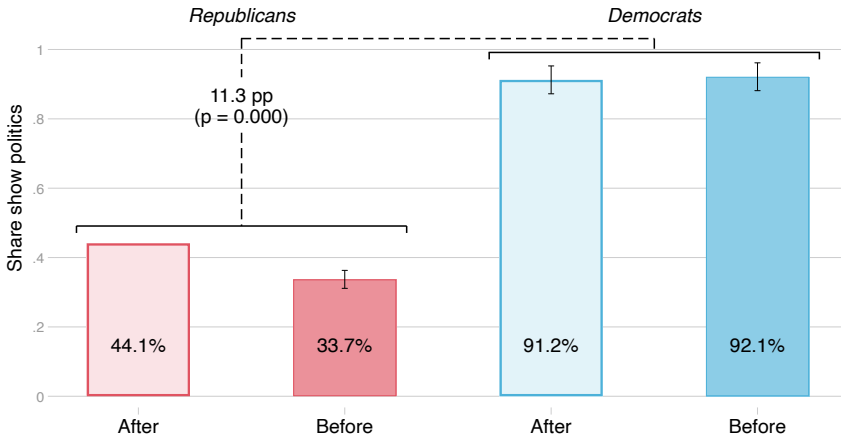
Note: In this figure, we regress whether participants choose an email enlisted with certainty from a given demographic group (instead of a carbon offset donation) on email-letter demographic traits. This regression includes 12,626 choices across 1,033 participants. In addition to estimating the role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

Figure 6: Choices over extended and basic profiles in round-two WTP



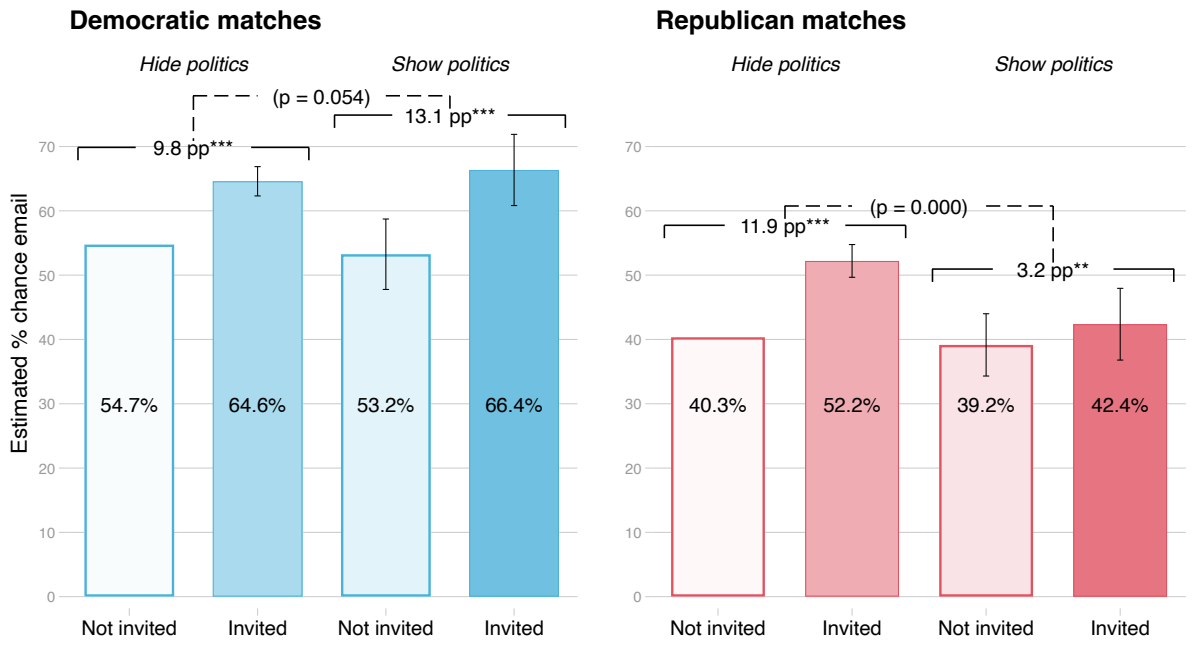
Note: This figure summarizes participants' choices to tell possible matches that they emailed Congress in the round-two WTP experiment. The graphs on the left and the right plot the share of the time in whcih participants chose to show possible matches extended profiles saying that they emailed Congress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

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



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

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



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

9 Main-text tables

Table 1: 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 2: Main Wave-1 results

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

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

Table 4: Wave-2 sample summary and balance

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

Note: This table summarizes and tests for balance within the Wave-2 experimental sample. We define the Wave-2 experimental sample as those who remained in the survey through choosing whether to email Congress or not. Column 1 and 2 present means among liberals (those who belong to or lean towards the Democratic party) and conservatives (those who belong to or lean towards the Republican party), respectively, on a range of baseline traits. Appendix 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

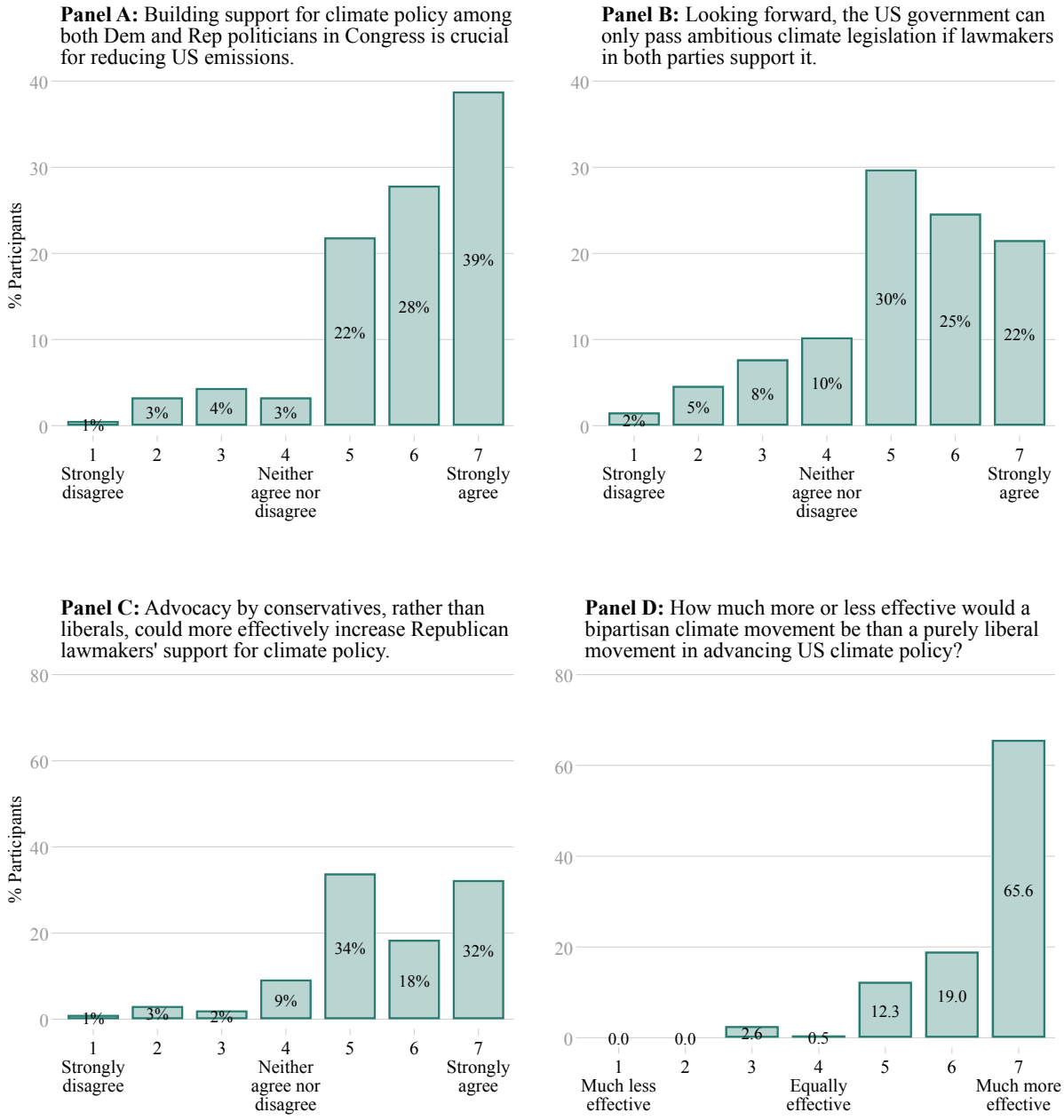
Table 5: Impacts of Wave-1 invitations on Wave-2 political action

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

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

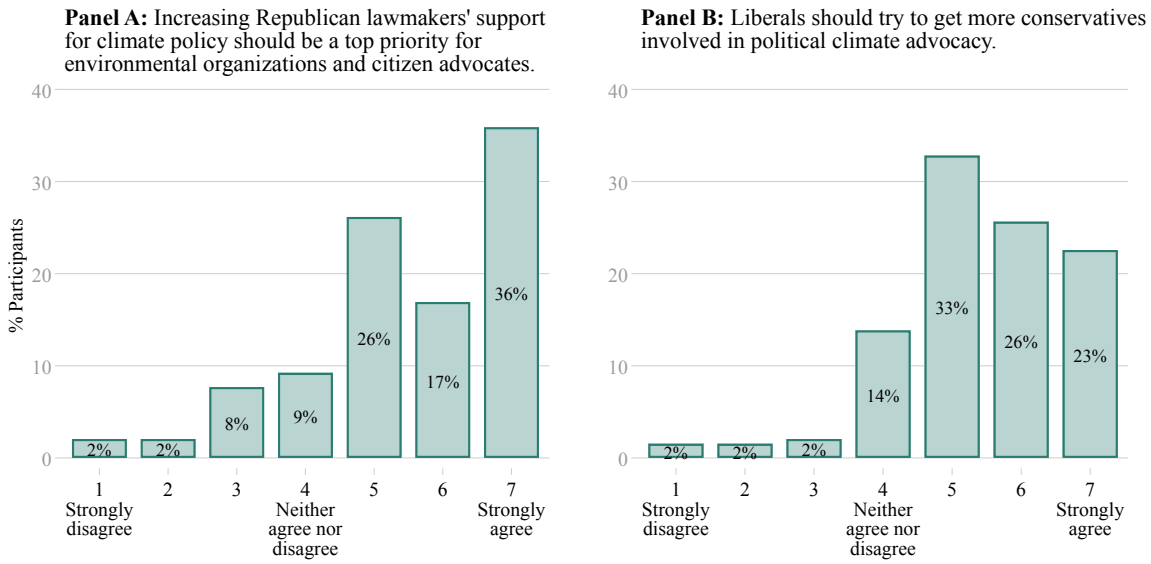
A Details of the experimental context

Figure A1: Democrats think bipartisan climate action would be more effective



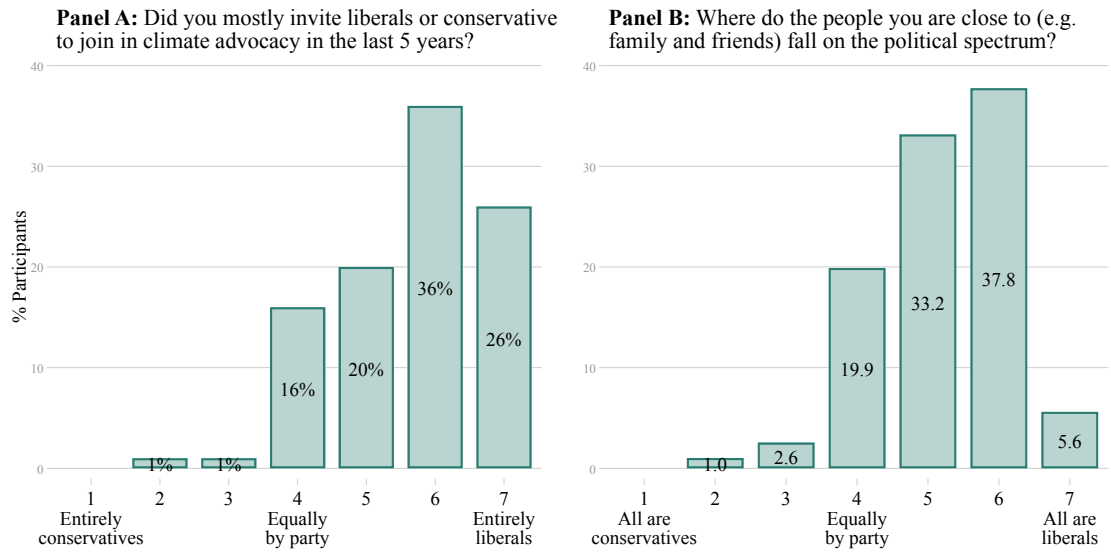
Note: This figure plots responses in a motivating sample of Democrats, who were recruited subject to the same eligibility restrictions as the Wave-1 experimental sample (Section 2.2). Panels A, B, and C plots responses to how much participants agree with the statements in the subfigure titles, while 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 A2: 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 all figures.

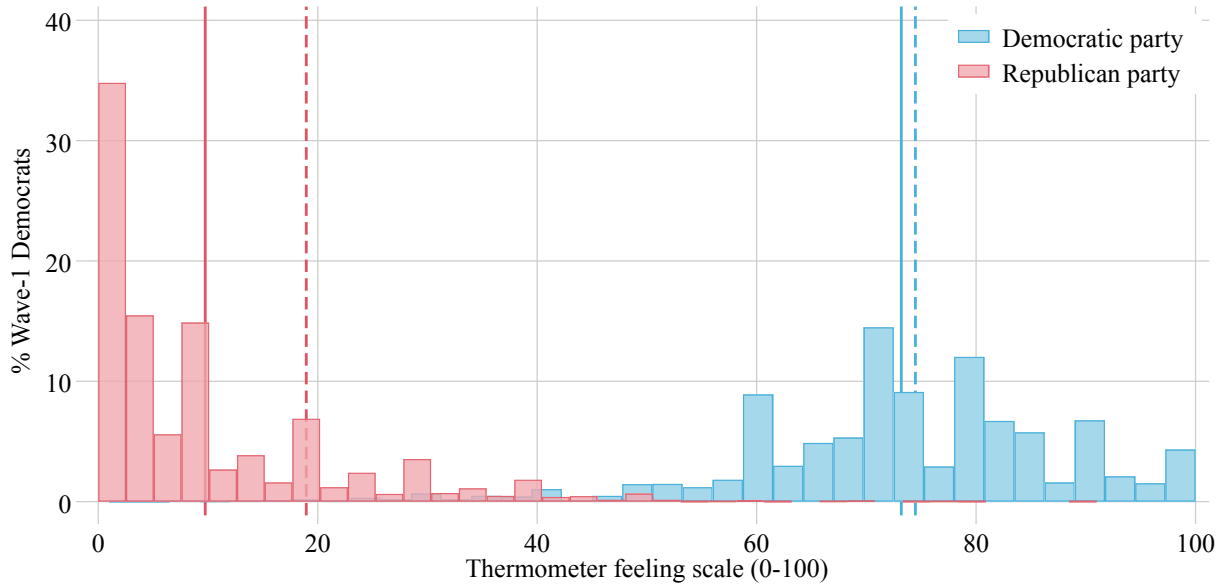
Figure A3: Few Democrats say they've previously invited conservatives



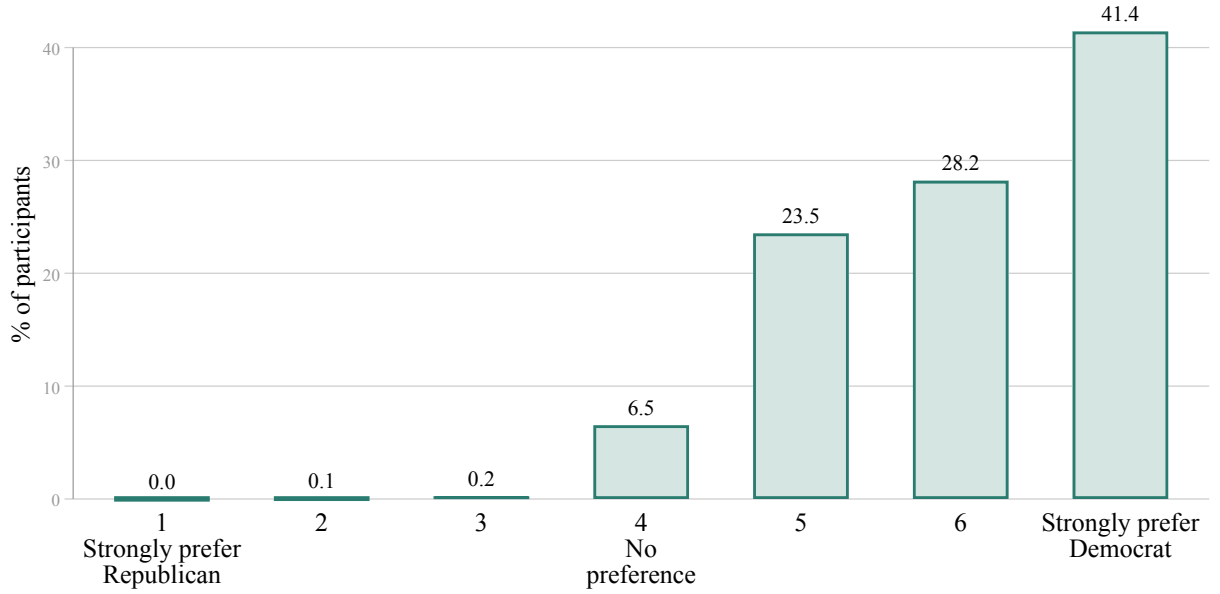
Note: Panel A restricts to Democrats who said that they have invited someone to join in political climate advocacy in the last 5 years via direct messages or in-person or phone conversations (N = 100). Panel B plots responses in the full motivating sample of Democrats (N = 196). Participants choose from 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.

Figure A4: Wave-1 Democrats' baseline affective polarization

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

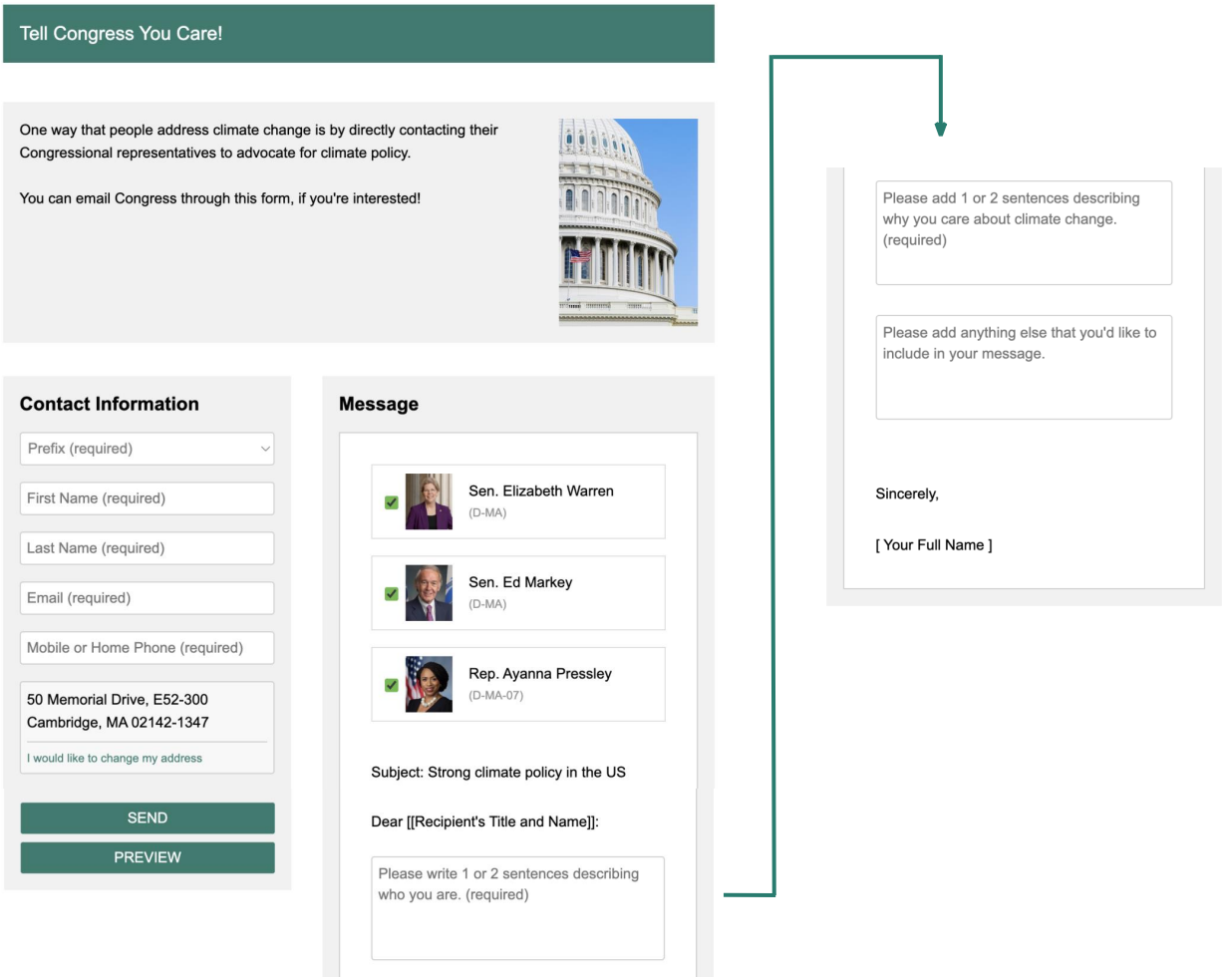


Panel B. Democrats strongly prefer friends of their own party



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 A5: Our 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. Appendix Figures B10 through B12 show screenshots from the full sequence of emailing Congress via this form. 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.

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 elicit participants' political affiliation in two steps.

1. First, we ask participants whether, generally speaking, they would say they lean towards the Democratic Party, the Republican Party, or neither.
2. If participants 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 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.

A.2 Forming groups of politically similar and nearby states

A key piece of information included in participants' demographic profiles is the state or group of states in which they live. We cluster states to protect participants' anonymity and to ensure that Wave-1 and Wave-2 demographic cells are large enough. We create groups of states that are geographically close and have similar 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 first ask 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 derived from Howe et al. (2015), we vary the number of states in each group to ensure that the projected number of eligible study participants is roughly equal across groups.

Table A1 presents our final state groups by the three climate marginality tiers, as well as the percent of eligible Americans that we estimate live in each group and the share of Wave-1 and Wave-2 participants who live in each group. No state group is strikingly over- or under-represented in our experimental samples. As expected, Wave-1 Democrats are more likely to live in Blue states than the bipartisan Wave-2 sample. Social media participants were very likely to classify each state in the tier to which their group is ultimately assigned (Appendix Figure A6).

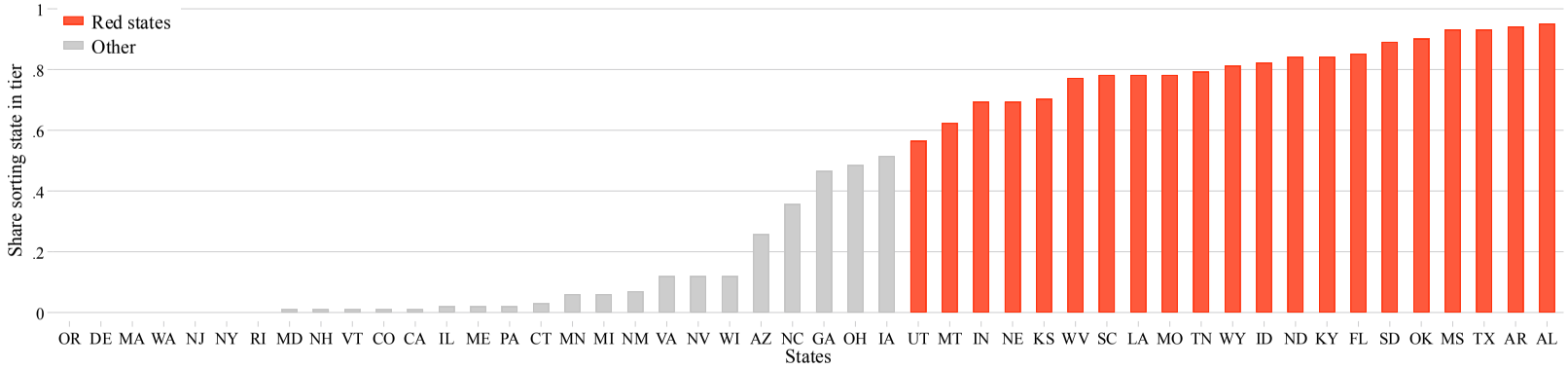
Table A1: State groups by tiers of climate-policy marginality

		% Eligible	% Wave 2	% Wave 1
Red groups:	SC, LA, MS, AL	6%	4%	2%
	MT, WY, ID, UT, ND, SD, NE, KS, OK	4%	6%	4%
	MO, AR, TN, KY, IN, WV	9%	10%	7%
	FL	7%	6%	5%
	TX	8%	7%	4%
Blue groups:	ME, VT, NH, MA, CT, RI	4%	8%	10%
	NY, NJ, MD, DE	13%	10%	12%
	WA, OR, CA	14%	14%	18%
	IL	5%	4%	4%
Purple groups:	OH, PA	7%	9%	9%
	VA, NC, GA	11%	9%	8%
	WI, MI, MN, IA	8%	9%	9%
	NV, AZ, CO, NM	5%	6%	6%

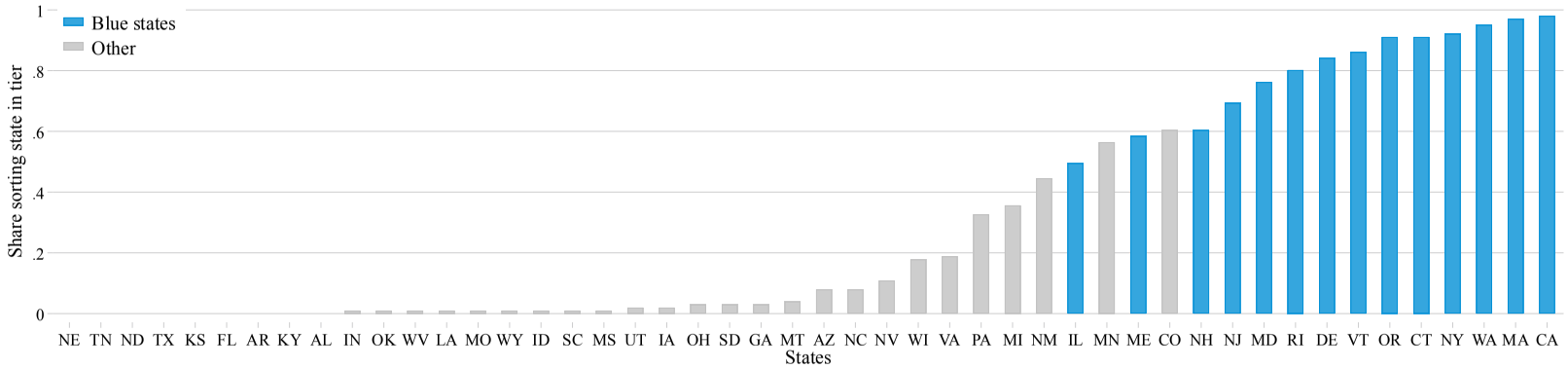
Note: This table lists the thirteen clusters of geographically close and politically similar states that we construct. Column 3 give our estimates for the share of total eligible study participants—Democrats or Republicans who believe climate change is human-caused—who we estimate live in each group (Howe et al., 2015). Columns 4 and 5 give the shares of the Wave-2 and Wave-1 experimental samples, respectively, who live in each state group.

Figure A6: Social-media sample responses by ultimate state-group classifications

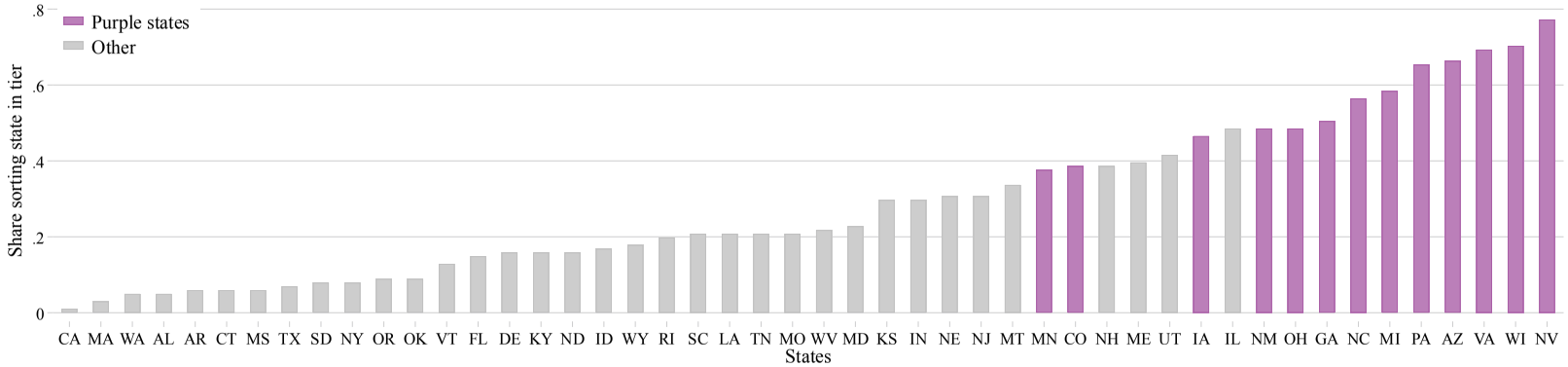
Panel A. Red states: Always very unlikely to support climate policy



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



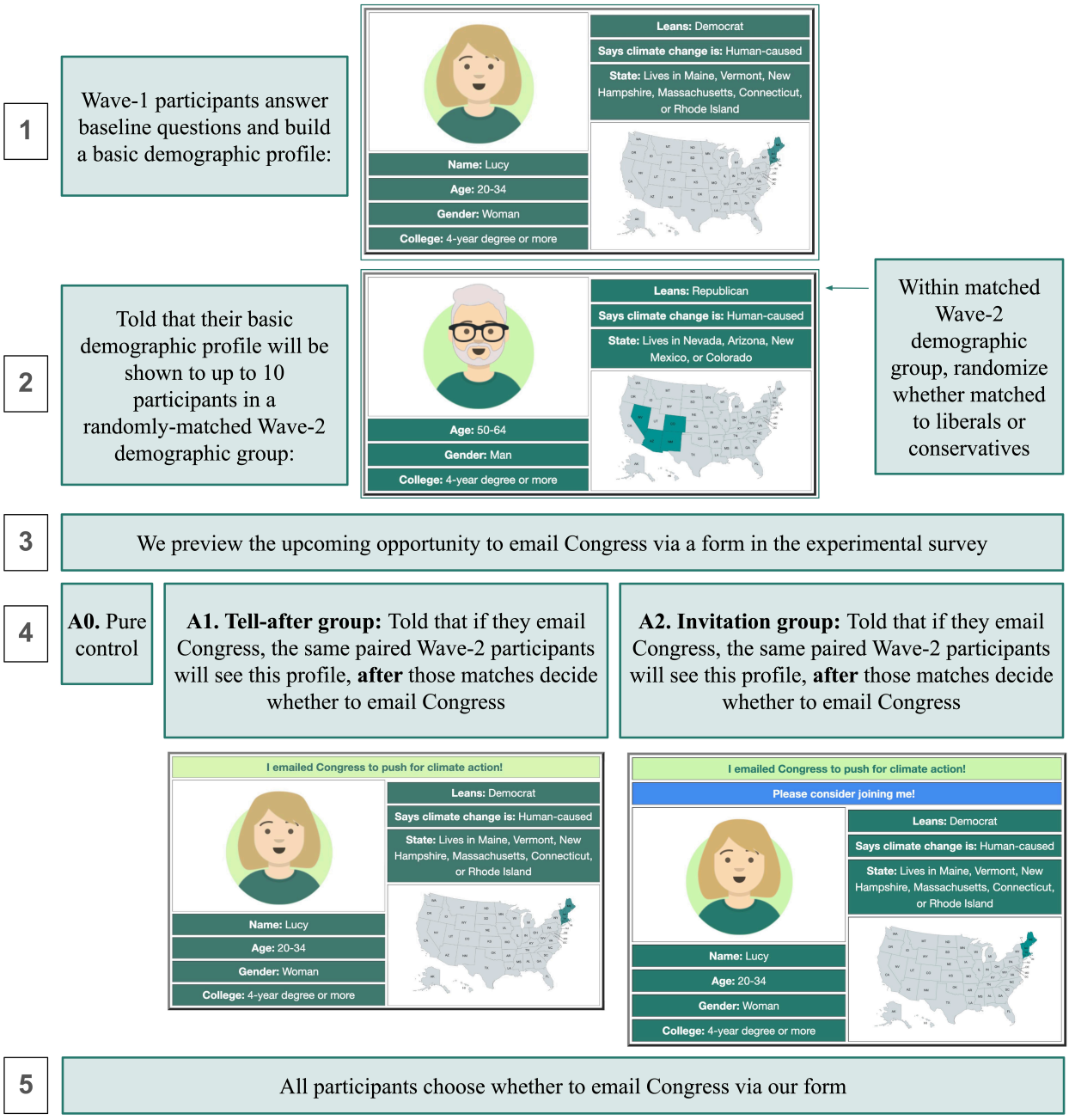
Panel C. Purple states: May or may not support climate policy, depending on citizen support



Note: Each panel sorts the 48 contiguous US states by the share of 101 Democrats recruited from social media who classified them as "red," "blue," or "purple," respectively. Each panel then highlights the states that we ultimately place in clusters classified as red, blue, or purple.

B Appendix to the Wave-1 experiment

Figure B1: Flowchart of Wave 1 experimental structure



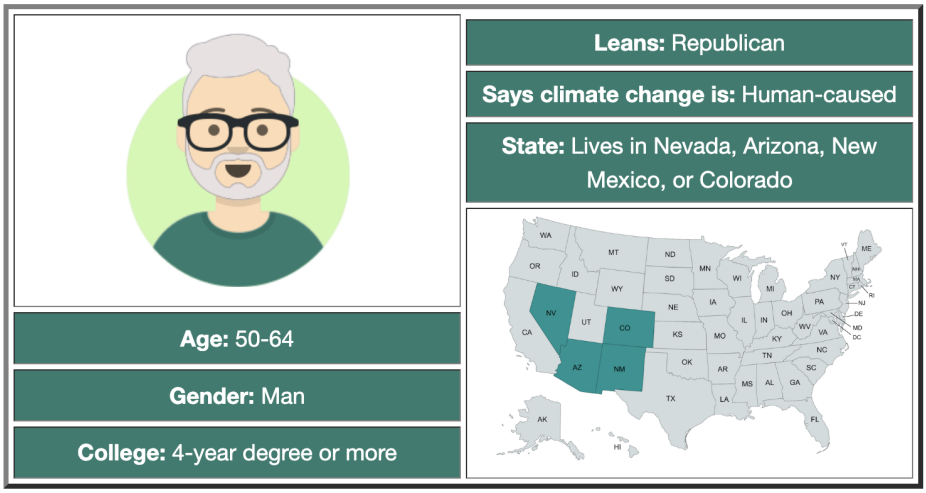
Note: This figure lays out the structure of the Wave-1 action experiment, which we describe in detail in Section 4.1.

B.1 Wave-1 survey and treatment details

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

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

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



Note: This figure shows how we tell Wave-1 participants that their basic demographic profile will be shown to up to 10 Wave-2 participants in a certain demographic group. Wave-1 participants see this slide after seeing their own demographic profile on the previous page. We generate 208 demographic cells from the interactions of two gender categories, two education categories (with or without a 4-year college degree), four age groups (20-34, 35-49, 50-64, 65-79), and 13 state groups. 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: Previewing the upcoming opportunity to email Congress

(a) Initial intro to action

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

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

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

(b) Description of the SoftEdge form

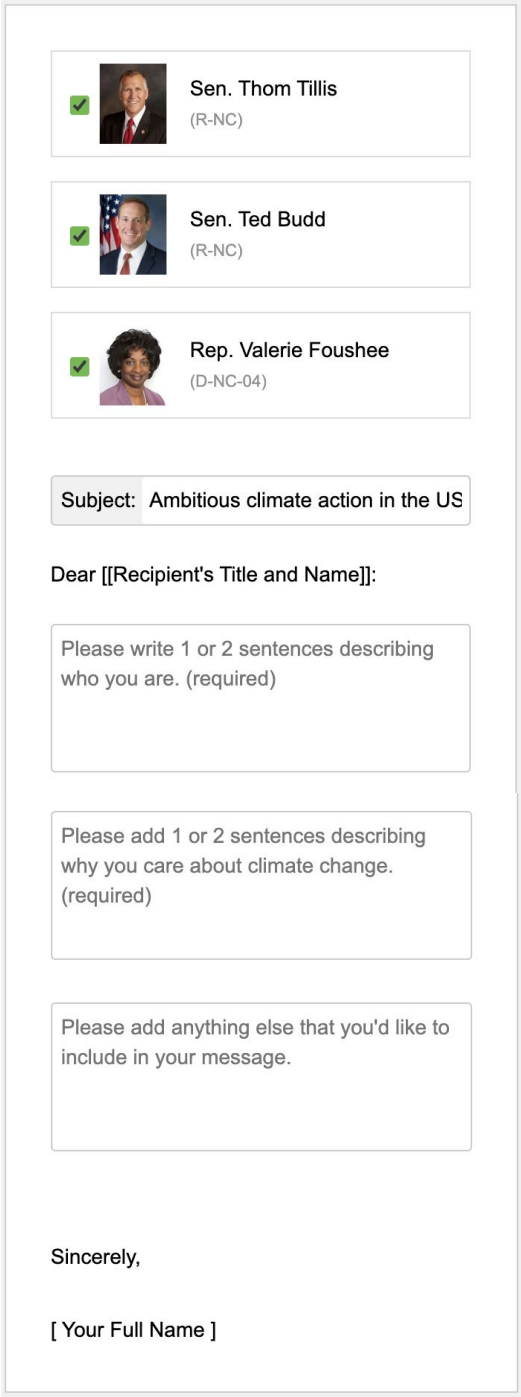
Here's a screenshot of what the form to contact Congress will look like. Based on your zip code, the form will identify your Senators and House Representative and address the email to them. (Right now it's filled in like you live in **North Carolina,** but this is **just an example.**)

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

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

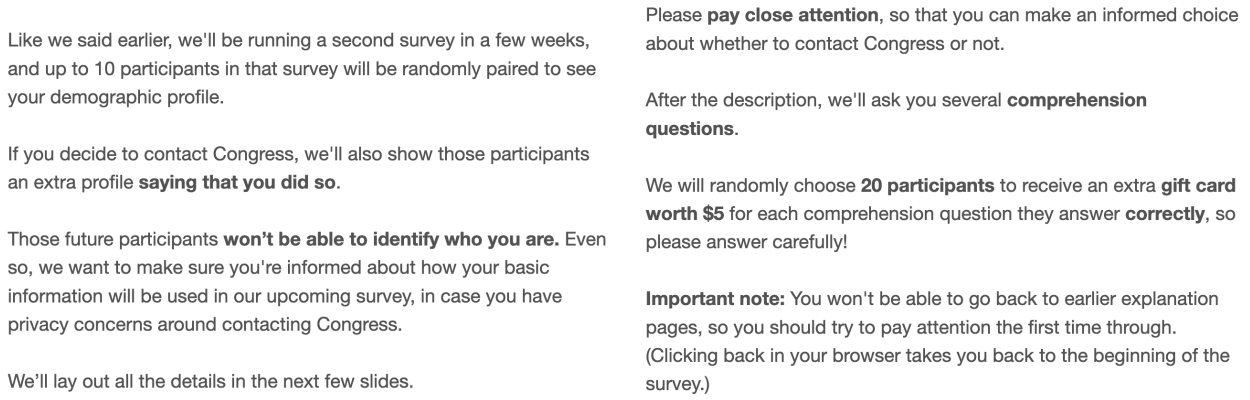
(Surveys of political staffers show that emails are much more effective when they're personalized!)

(c) Preview of the email form



Note: In both the Wave-1 and Wave-2 surveys, we first introduce the upcoming opportunity to email Congress as neutrally as possible (Sub-Figure B3a). Next, we describe the SoftEdge email form in more detail (Sub-Figure B3b), alongside a preview of the email form itself (Sub-Figure B3c). This description points out that the email form will have an un-editable subject line supporting climate policy. Participants assigned to either the Invitation or Tell-after groups in the Wave-1 experiment see this preview and description of the SoftEdge form before learning that their decision to email Congress will be passed on to others, so they know that no Wave-2 participant can use the email form to uniformly oppose climate policy.

Figure B4: Introduction to the Invitation and Tell-after treatments

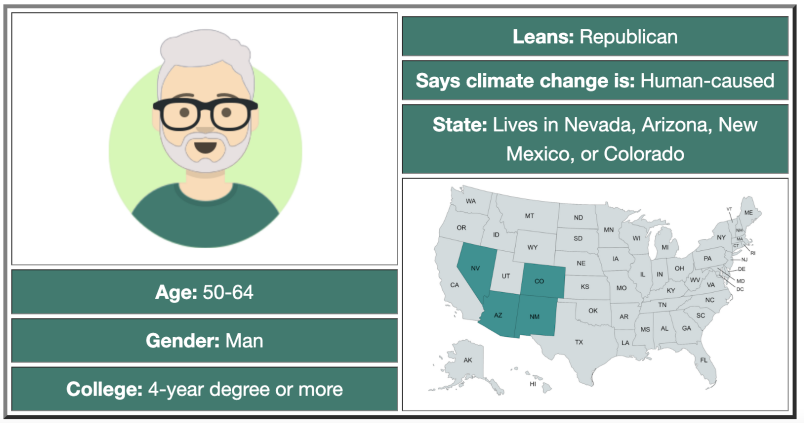


Note: This figure shows how we initially introduce the Invitation and Tell-after treatments to Wave-1 participants after showing them the upcoming opportunity to email Congress.

Figure B5: Explanation of the Invitation treatment

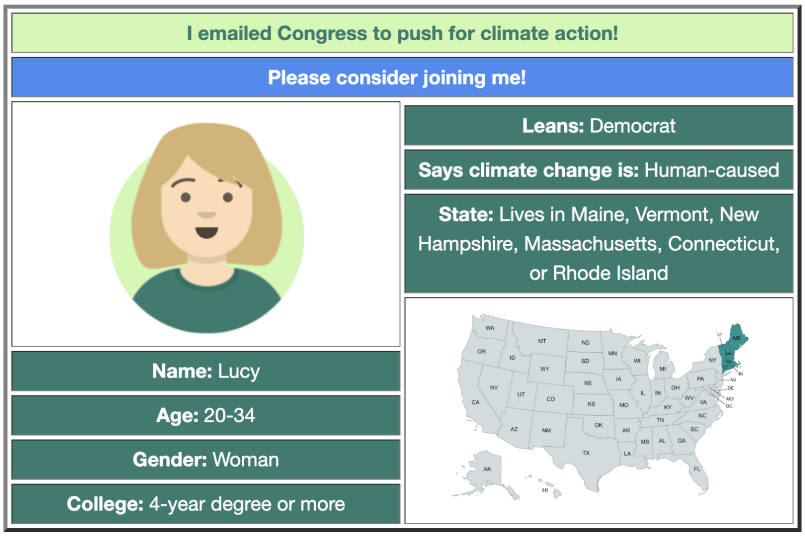
Remember that up to 10 participants in the demographic group below will see your own demographic profile when they take our survey.

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.

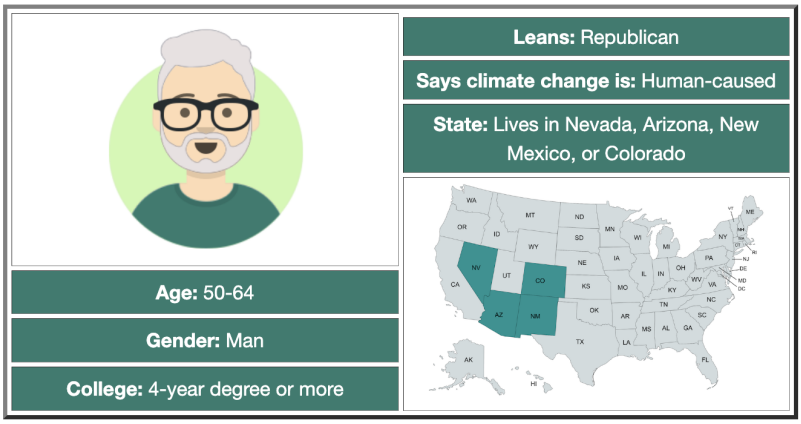
If you **decide not to contact Congress**, we won't show them this profile or give them any information about whether you or others contacted Congress.

Note: This figure reproduces the treatment survey pages that participants see if they are assigned to the Invitation group. The demographic profile in the top panel would show the Wave-2 group to which they are assigned (Appendix Figure B2) and the profile in the second panel would be their own.

Figure B6: Explanation of the Tell-after treatment

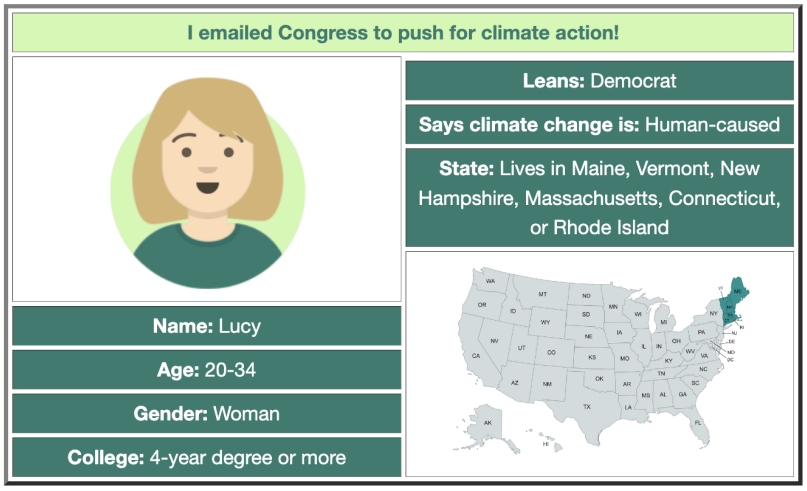
Remember that up to 10 participants in the demographic group below will see your own demographic profile when they take our survey.

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:



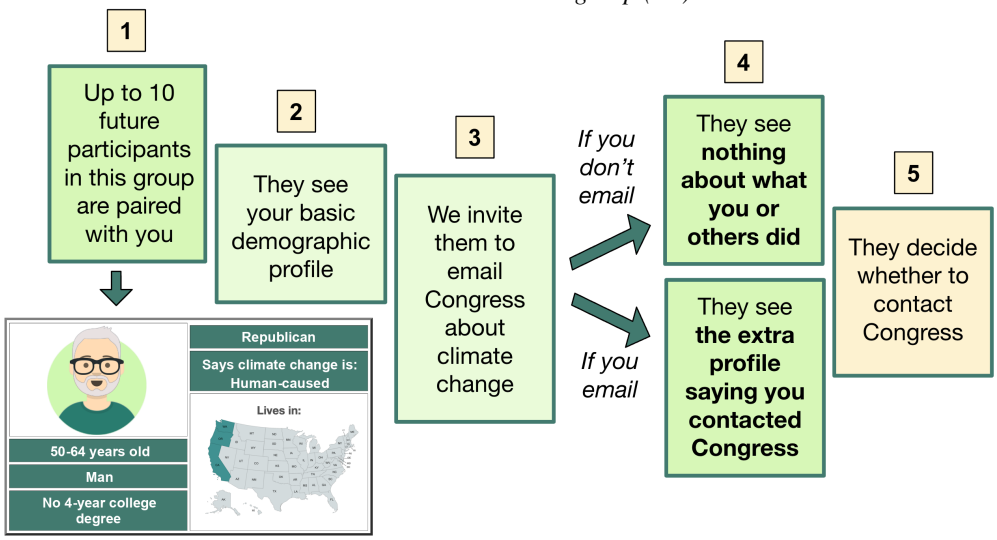
That means that they **won't know** you contacted Congress **when they decide** whether to do so or not.

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.

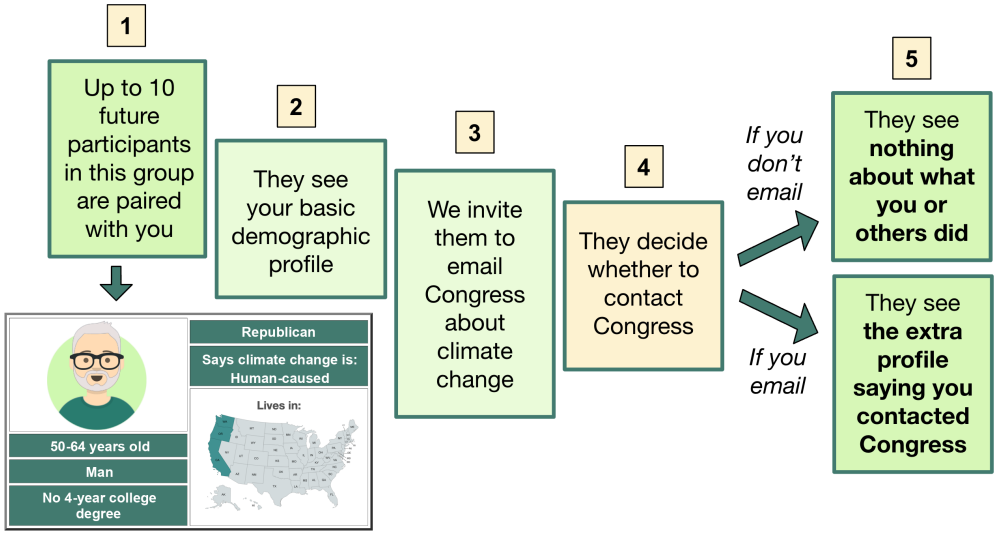
Note: This figure reproduces the treatment survey pages that participants see if they are assigned to the Tell-after group. The demographic profile in the top panel would show the Wave-2 group to which they are assigned (Appendix Figure B2) and the profile in the second panel would be their own.

Figure B7: Sample flowcharts

For those in the Invitation group (A2):



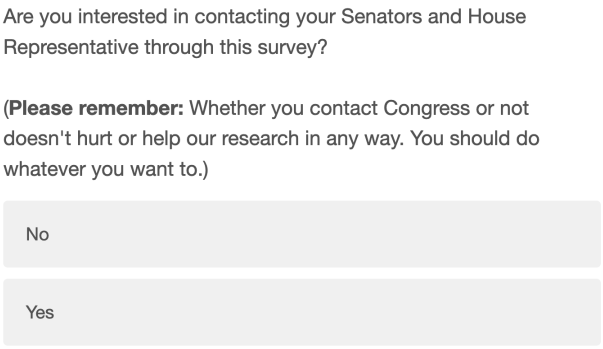
For those in the Tell-after group (A1):



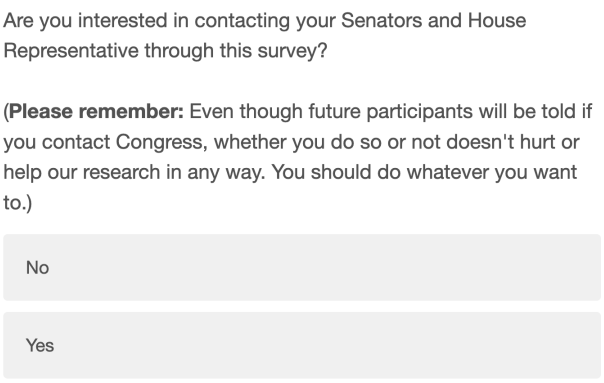
Note: These figures shows the flowcharts that we show to Invitation and Tell-after participants to reiterate the timing of when future participants will be told that they emailed Congress. Note that the flowcharts fill in the demographic profiles of each Wave-1 participant's paired Wave-2 demographic group, shown first in Appendix Figure B2.

Figure B8: Initial invitations to opt into the email process (Waves 1 and 2)

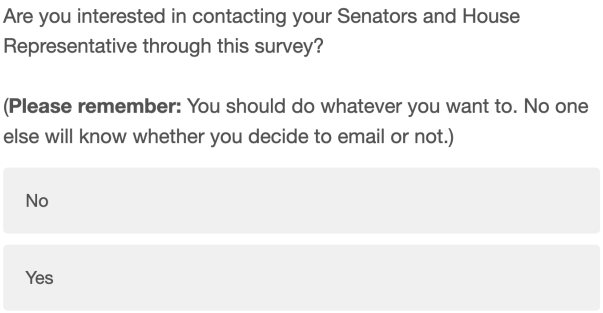
(a) Wave 1: Control group



(b) Wave 1: Invitation and Tell-after groups



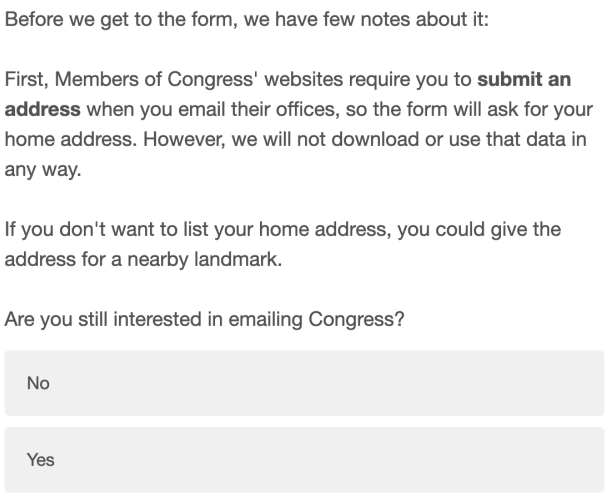
(c) Wave 2



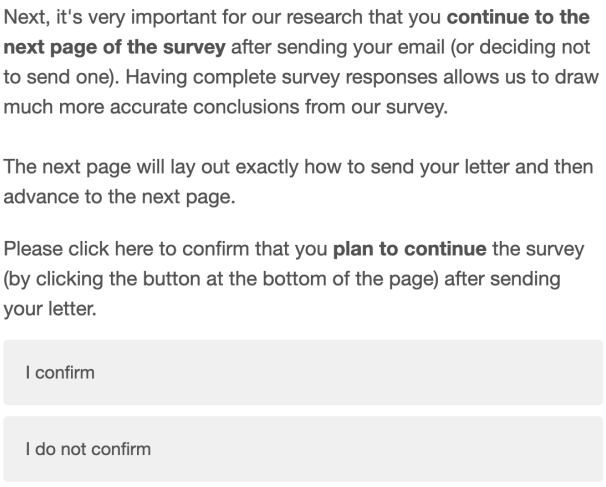
Note: After the Wave-1 and Wave-2 treatments, we ask participants if they want to start the email process. We reiterate to participants that they should feel free not to email Congress; we vary this disclaimer somewhat between Waves 1 and Waves 2 and between Wave-1 treatment groups. Throughout the project, we use a binary variable for whether participants opt into the email process at this step as a secondary measure of climate action.

Figure B9: Continuing the email process

(a) Second invitation to email Congress

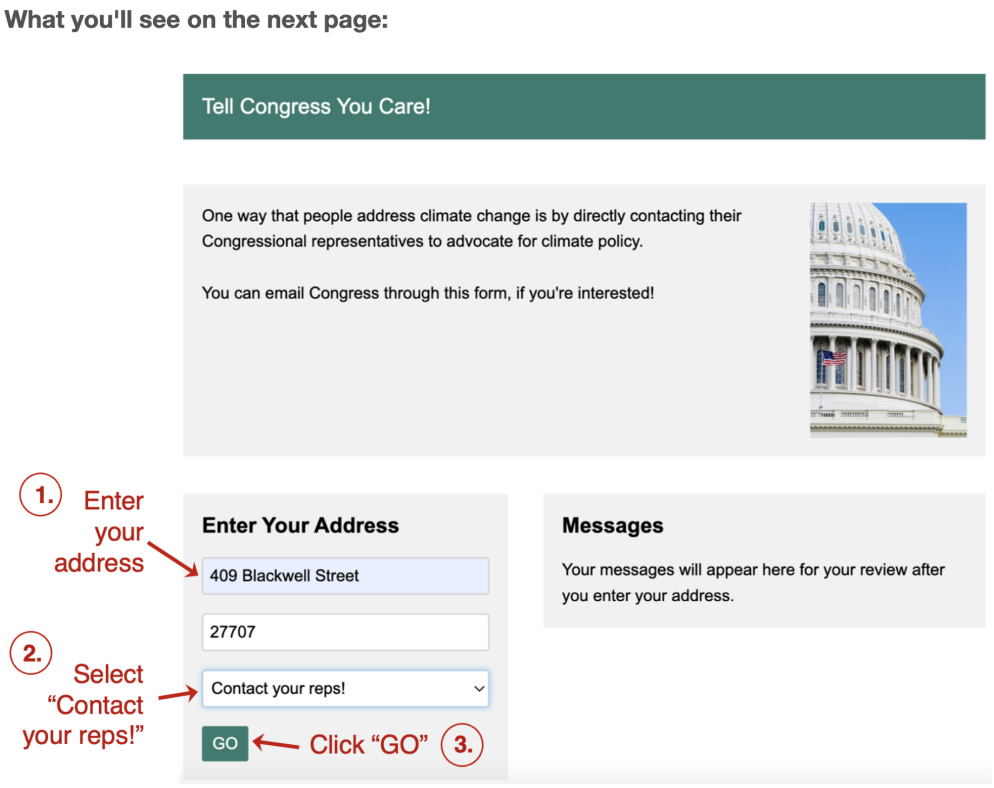


(b) Commit to continuing survey



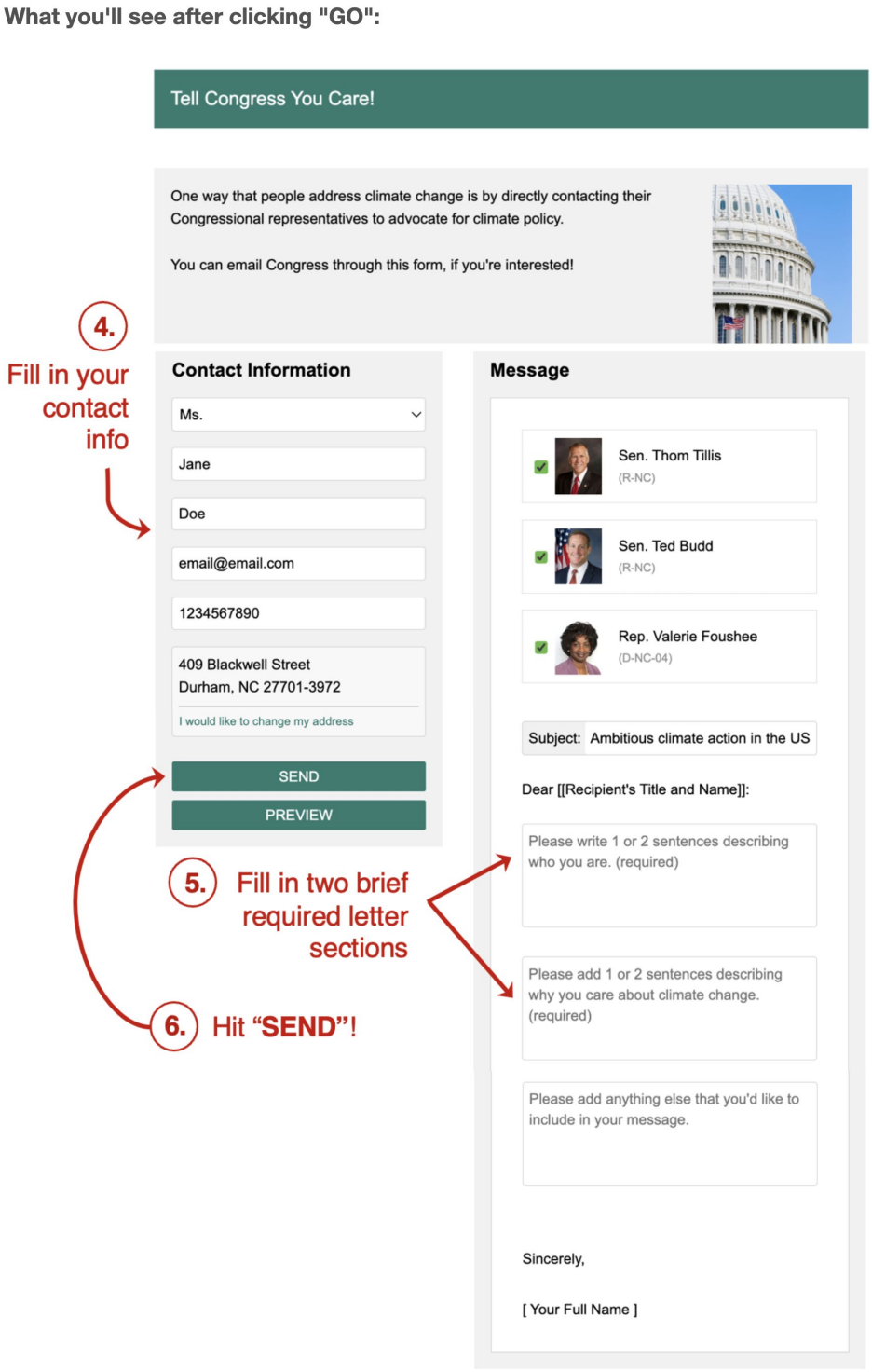
Note: If participants opt into the email process, we tell them that the form will ask for an address and ask if they are still interested. If they again say yes, we ask them to commit to continuing the survey after sending their email. We then walk participants through screenshots of the email process (Appendix Figures B10 - B12) and they begin the process shown in those screenshots. This process is the same in Waves 1 and 2.

Figure B10: SoftEdge email form, Page 1



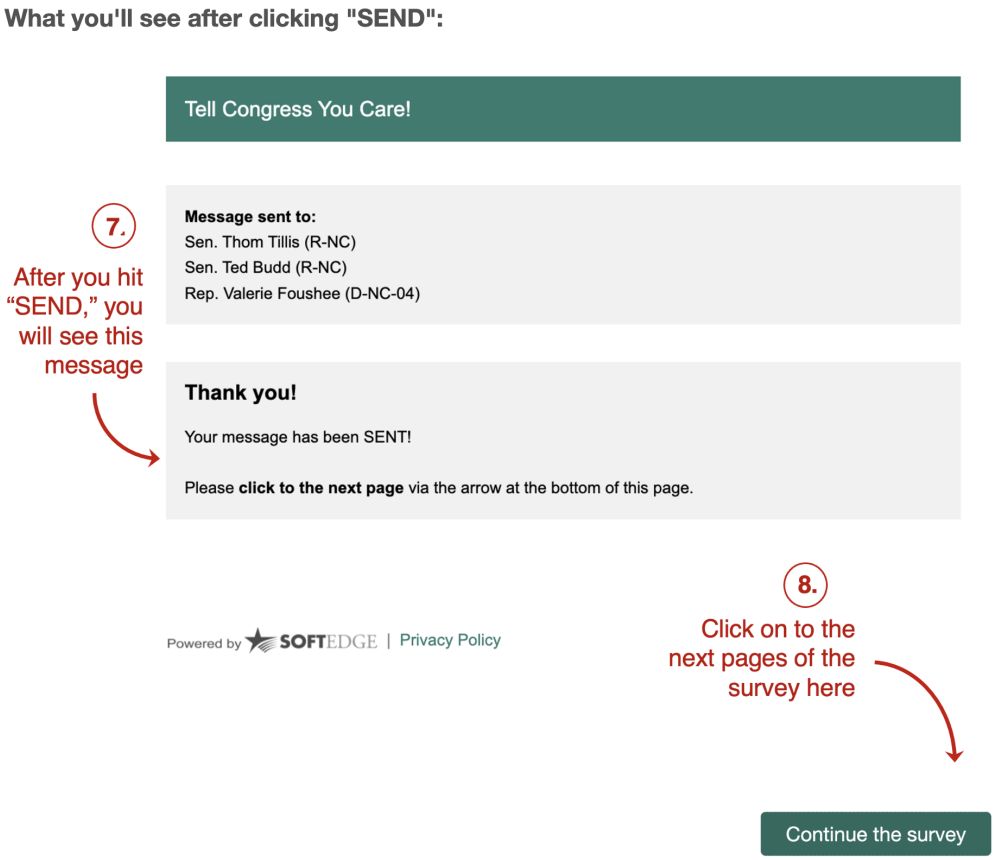
Note: This figure and Appendix Figures B11, and B12 show screenshots from a guide to the email form that all participants can download before emailing Congress. Participants start the email process by entering their residential address into the form landing page. They think click "Go."

Figure B11: SoftEdge email form, Page 2



Note: The form then autopopulates senders' national Senators and U.S. Representative. Participants fill in their name, contact information, and email text. The form requires that participants fill out two open response fields. We recommend to participants that they use these spaces to describe who they are and why they care about climate change, but they can fill in any text they wish. While the email body is fully customizable, the email subject line is fixed to support ambitious climate policy. We randomize the subject line from options such as "Ambitious climate action in the US" and "My strong support for US climate policy." Participants can send their email by clicking "SEND."

Figure B12: SoftEdge email form, Page 3



Note: When participants click send, the form shows a confirmation and thank you message. Participants can then click on to continue with their survey. While we cannot directly observe participants' clicks or activity in the SoftEdge form itself, we observe the full text of all emails sent via this form. Appendix B.2.1 describes the process by which we match participants to email records.

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[43] and 1,834 were sent by unique Wave-2 participants.

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

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

3. Finally, we merge participants with email records on first name, state, treatment status, and the date and time at which participants took their survey. We restrict matches to whose where the participant did not provide a last name in the survey consent, the participant attested in the survey that they emailed Congress, and the email was sent within 1 hour of the participant starting they survey. This step merges an additional 130 Wave-1 emails and 28 Wave-2 emails. In total, then, we merge 3,968 out of 4,124 total Wave-1 emails (96%) and 1,674 out of 1,834 total Wave-2 emails (91%), with no differences by treatment status.

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.

[43] 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).

- *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 and attrition

Appendix Figure B13 summarizes Wave-1 recruitment. We recruit Wave-1 participants using ads on Facebook, Instagram, and Twitter (Appendix Figure B14). 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.

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.

Figure B13: Wave-1 recruitment

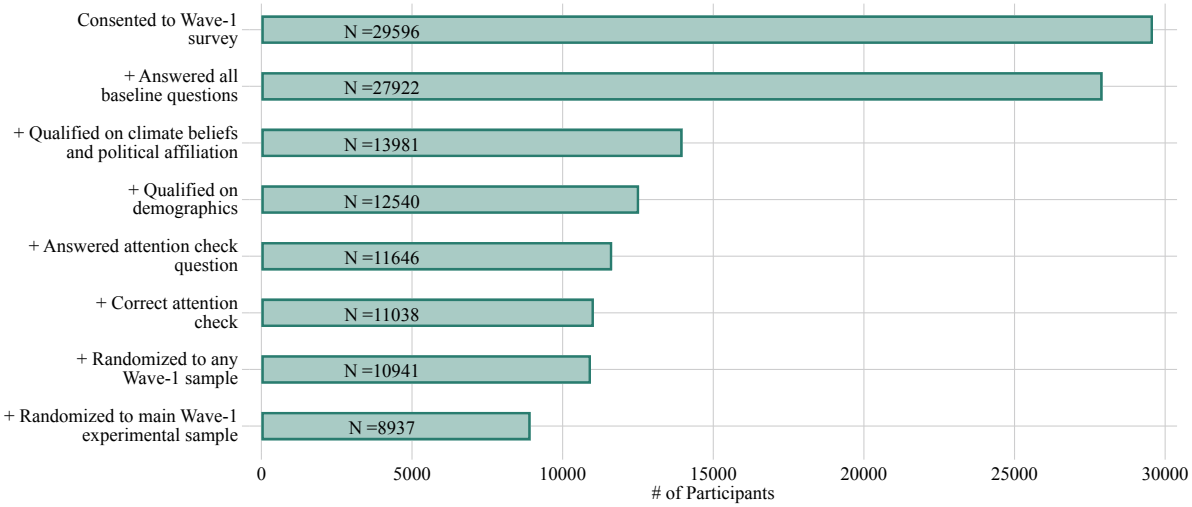
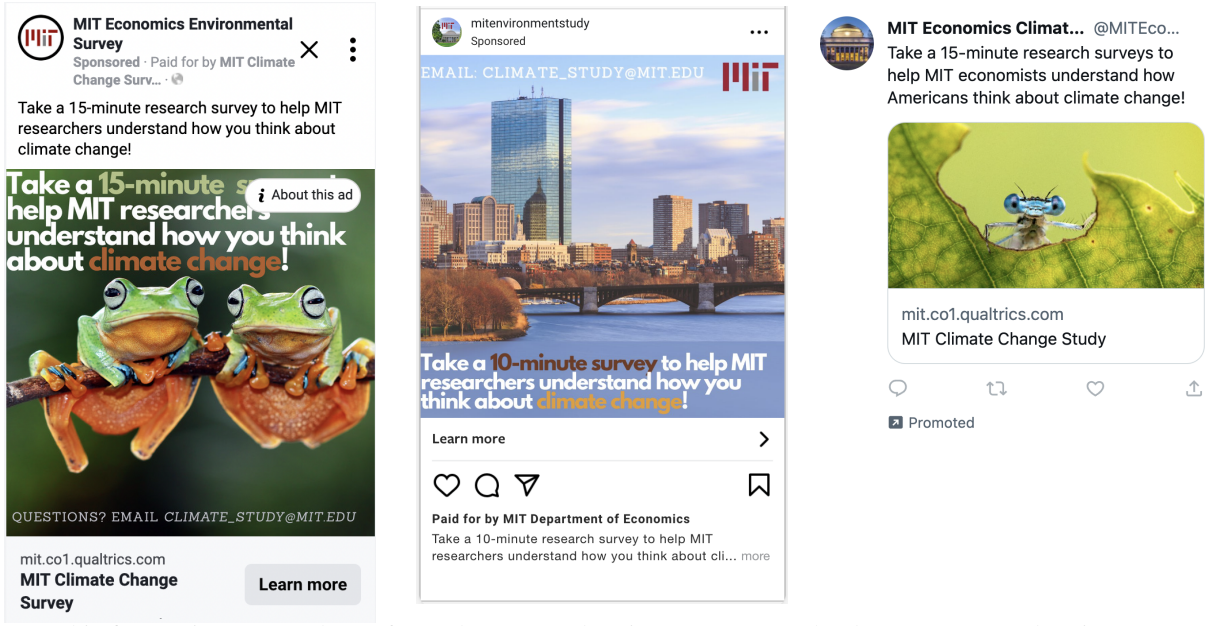


Figure B14: Sample social-media advertisements

Facebook: *Instagram:* *Twitter:*



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.

Table B1: Wave 1 attrition results

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

Panel B: Characteristics of those with and without email choices			
	Observe if emailed Congress?		(1) = (2)
	(1) Yes	(2) No	*p-value*
Woman	0.645	0.551	(0.000)
Hispanic	1.027	1.021	(0.317)
Has $\geq$ 4-year college degree	0.828	0.798	(0.061)
Age ranges:			
20-39	0.118	0.057	(0.000)
40-59	0.341	0.343	(0.916)
60-79	0.541	0.600	(0.003)
Income bins (USD):			
Less than 50,000	0.167	0.169	(0.894)
50,000-99,999	0.321	0.317	(0.875)
100,000-149,999	0.234	0.231	(0.814)
150,000-199,999	0.130	0.136	(0.668)
200,000 or more	0.148	0.147	(0.943)
Residence by state marginality:			
Red state	0.237	0.223	(0.410)
Blue state	0.444	0.410	(0.089)
Purple state	0.319	0.367	(0.012)
Climate beliefs:			
Climate worry (std)	-0.001	0.014	(0.721)
Desire for climate action (std)	0.001	-0.012	(0.739)
Perceived local impacts (std)	-0.005	0.057	(0.137)
Political engagement and beliefs:			
Political engage. index (std)	0.010	-0.118	(0.001)
Prev. contacted reps	0.733	0.704	(0.096)
Prev. donated	0.820	0.801	(0.235)
Prev. canvassed	0.082	0.060	(0.028)
Prev. signed petition	0.830	0.799	(0.053)
Prev. phonebanked	0.113	0.088	(0.037)
Political efficacy index (std)	-0.001	0.017	(0.659)
Degree prefer Dem friends (std)	-0.002	0.023	(0.549)
Sample size	8269	668	

Note: Panel A exams 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. 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. Panel B compares baseline traits between Wave-1 participants for whom we observe active email choices (column 1) and those who leave the survey before this choice (column 2). Column 3 gives p-values from a test of equal means between these groups.

Table B2: Baseline predictors of contacting Congress in the Wave-1 and Wave-2 control groups

	Whether email Congress			
	Wave 1		Wave 2	
	(1)	(2)	(3)	(4)
Woman	0.046***	(0.017)	-0.002	(0.012)
Hispanic	-0.007	(0.048)	-0.048**	(0.021)
Has $\geq$ 4-year college degree	0.035*	(0.021)	-0.005	(0.014)
Age ranges:				
40-59	0.022	(0.026)	-0.023	(0.015)
60-79	0.003	(0.025)	-0.026	(0.016)
Income bins (USD):				
50,000-99,999	-0.025	(0.023)	0.001	(0.015)
100,000-149,999	0.020	(0.026)	-0.021	(0.019)
150,000-199,999	-0.004	(0.029)	-0.003	(0.027)
200,000 or more	-0.002	(0.029)	-0.071***	(0.024)
Residence by state marginality:				
Blue state	0.010	(0.020)	0.001	(0.015)
Purple state	0.018	(0.021)	-0.015	(0.014)
Climate beliefs:				
Climate worry (std)	0.022**	(0.009)	0.025***	(0.008)
Desire for climate action (std)	0.014*	(0.008)	0.010	(0.007)
Perceived local impacts (std)	0.008	(0.008)	0.016**	(0.007)
Political affiliation:				
R-leaning Independent			-0.007	(0.017)
L-leaning Independent			0.000	(0.023)
Democrat			-0.002	(0.020)
Political engagement and beliefs:				
Prev. contacted reps	0.097***	(0.018)	0.058***	(0.016)
Prev. donated	0.051***	(0.019)	0.023	(0.015)
Prev. canvassed	-0.020	(0.032)	0.026	(0.033)
Prev. signed petition	0.037*	(0.020)	0.056***	(0.013)
Prev. phonebanked	0.025	(0.030)	-0.000	(0.029)
Political-efficacy index (std)	-0.025***	(0.008)	-0.033***	(0.012)
Control mean	0.312		0.210	
Sample size	3588		3985	

Note: This table regresses indicators for whether Wave-1 and Wave-2 participants emailed Congress on baseline traits, restricting to the pure control groups of each experiment. All baseline traits are as described in Appendix B.2.2. Columns 1 and 3 present estimates in Wave 1 and Wave 2, respectively, while columns 2 and 4 present the associated heteroskedasticity-robust standard errors in parentheses. We indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

B.4 Wave-1 comprehension

Just 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. We ask the following questions:

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

⇒ Answers: [A repeat of your demographic profile; Nothing; An additional profile that also says that you contacted Congress]

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

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

   ⇒ Answers: [Before they decide whether to contact Congress or not; After they decide whether to contact Congress or not]

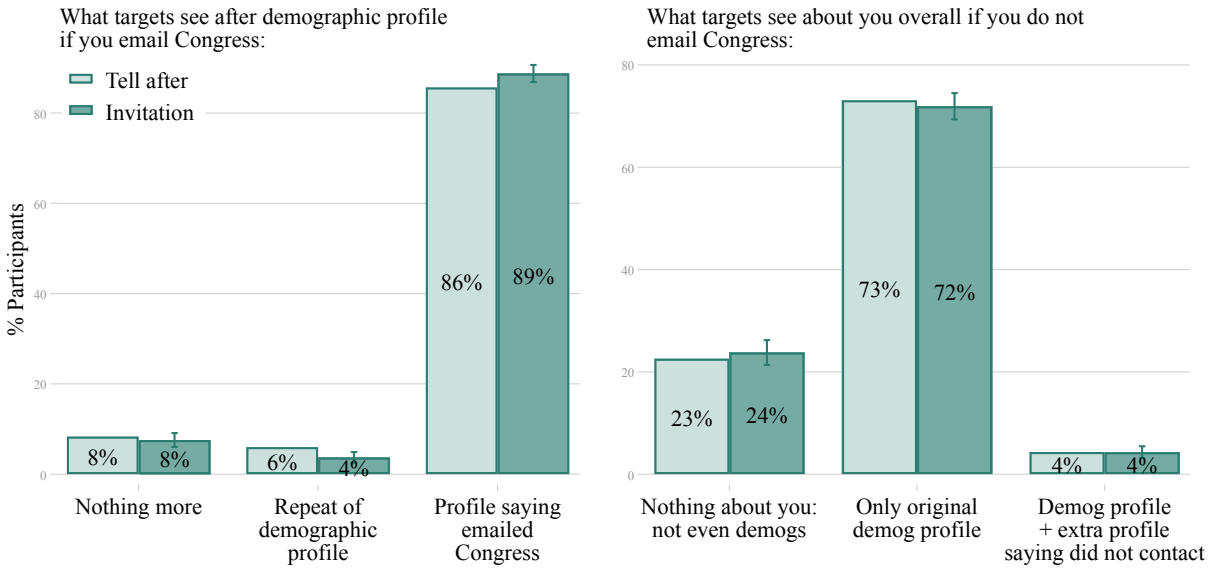
   We tell participants whether their answer is correct and reiterate whether future participants would see their profile before or after they decide whether to email Congress.

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

   ⇒ Answers: [Only your original demographic profile; Nothing about you–not even a basic demographic profile; Your demographic profile plus an extra profile saying you didn't contact Congress]

   We tell participants whether their answer is correct and reiterate that if they don't email Congress, the future participants they're paired with would still see their basic demographic profile but would not see an additional profile saying whether they emailed Congress or not. We reiterate that these matched participants would not see any information about whether anyone else emailed Congress.

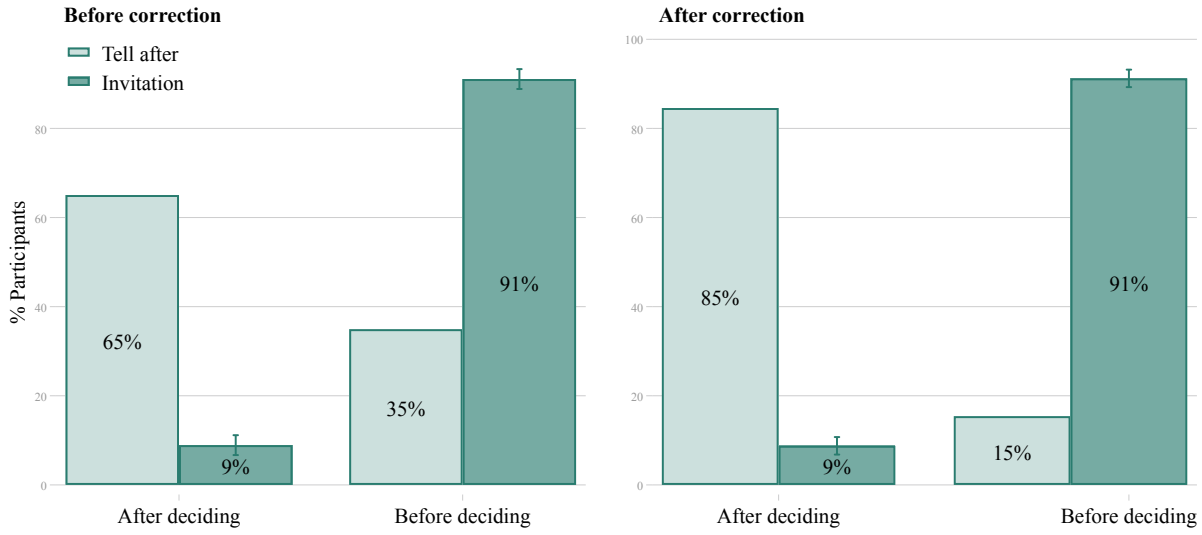
Figure B15: Wave-1 comprehension of what future participants will see about them



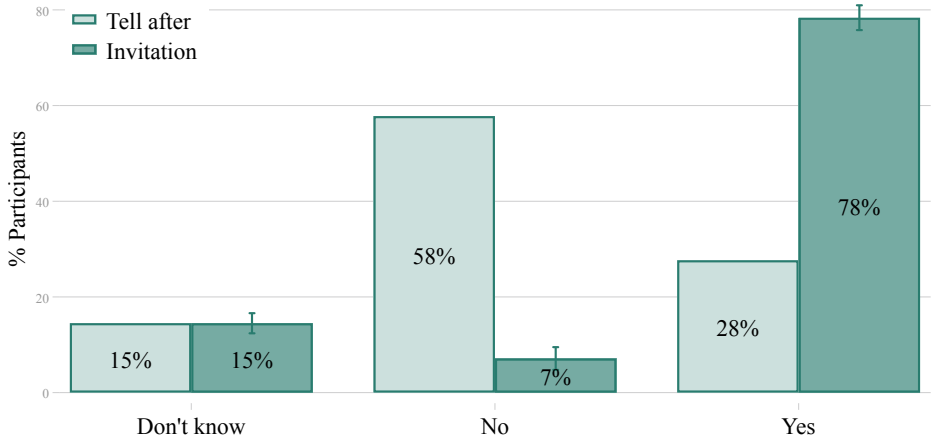
Note: This figure plots the distribution of Tell-after and Invitation participants' responses to comprehension questions 1 and 3, listed above. The error bars give the 95% confidence intervals for a regression of indicators for each response on an indicator for being in the Invitation group. N = 4,836 at left and N = 4,727 at right, or the 91% and 89% of the Invitation and Tell-after groups that had not yet left the survey.

Figure B16: Wave-1 comprehension of profile timing and opportunities to influence Wave 2

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



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



Note: Panel A plots the distribution of Tell-after and Invitation participants' responses to comprehension questions 2 and 4. Participants' responses were corrected after question 2. Panel B plots the distribution of Tell-After and Invitation participants' responses to comprehension question 5. 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.

At the end of the Wave-1 survey, we ask the Tell-after and Invitation groups several additional comprehension questions. We do not correct their answers. Again, we say that we will randomly choose 20 people to receive a gift card with $5 for each question that they answer correctly:

4. 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

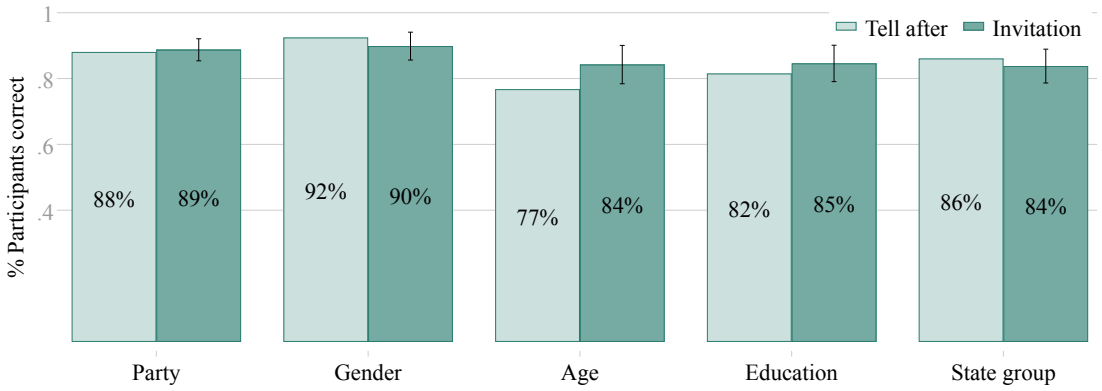
group have seen/Since you contacted Congress, will participants in your paired demographic group see] a profile saying that you did so **before** or **after** they choose whether to contact Congress or not?

⇒ Answers: [Before; After]

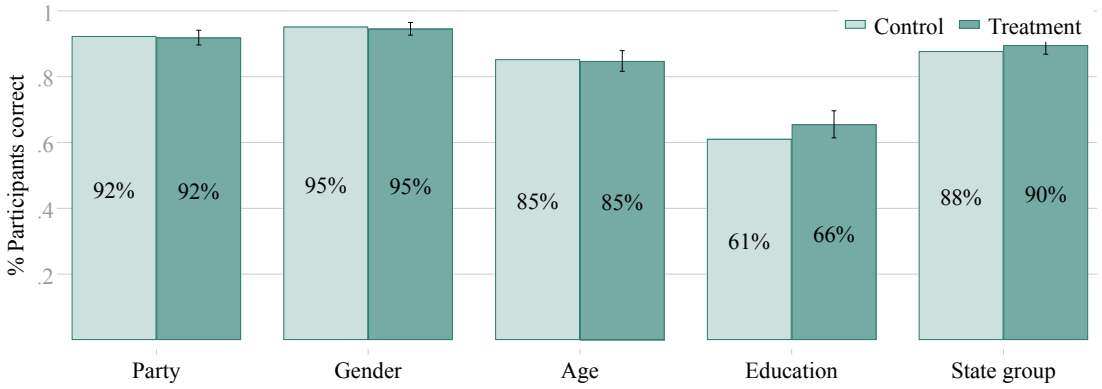
5. The future participants you're paired with will be choosing whether to contact Congress when they take this survey. In theory, could your decision to contact Congress or not influence whether they do so during the survey? ⇒Answers: [No; Yes; I don't know]
6. What about after the survey? In theory, could your decision to contact Congress or not influence whether your paired participants do environmentally-friendly things after finishing this survey? (This was asked of a random subset of participants.) ⇒Answers: [No; Yes; I don't know]

Figure B17: Whether participants correctly remember paired participants' traits

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



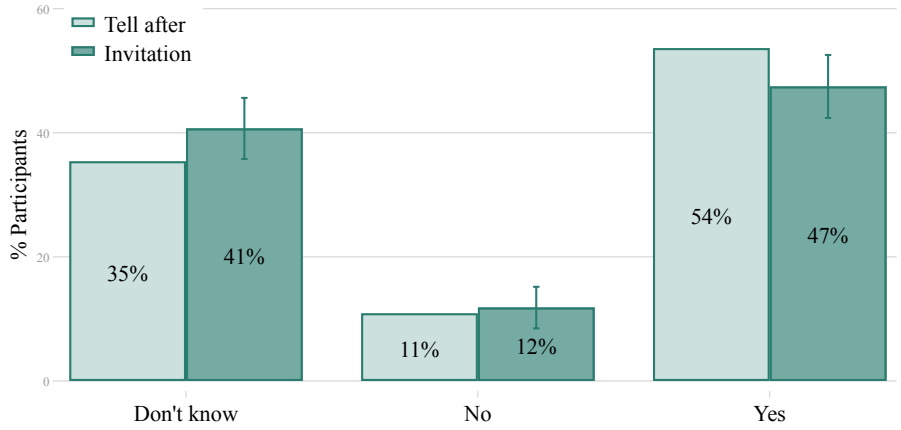
Panel B. Share Wave-2 participants who correctly remember Wave-1 matches' traits



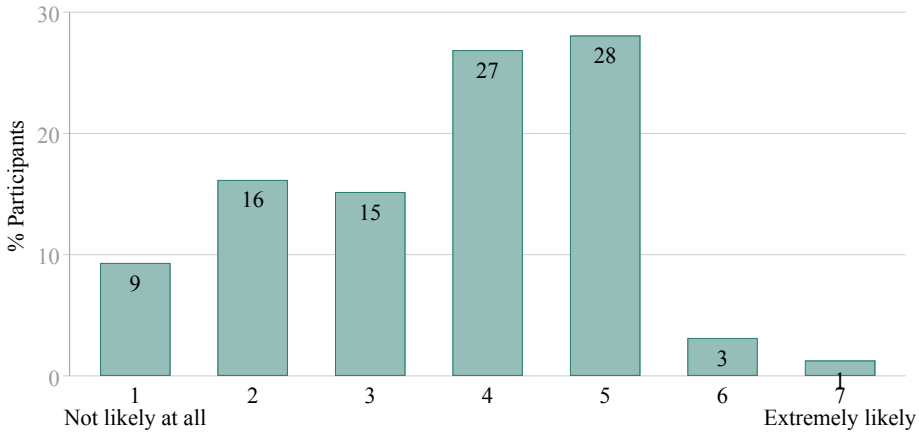
Note: This figure plots the share of Wave-1 and Wave-2 participants who correctly identified their matches' demographics. The error bars give the 95% confidence intervals for a regression of whether participants answered correctly on an indicator for being in the [Invitation/Treatment] group. In Wave 1 (Panel A), we randomized one third of the Tell-after and Invitation groups to answer multiple-choice questions about either the gender or age, the political party, and either the education or state group of their Wave-2 matches. In Wave 2 (Panel B), we randomized half of participants to answer multiple-choice questions about either the gender, age, and political leanings or the education and state group of their Wave-1 match. The sample sizes in Panels 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 B18: Wave-1 beliefs on action after the survey

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

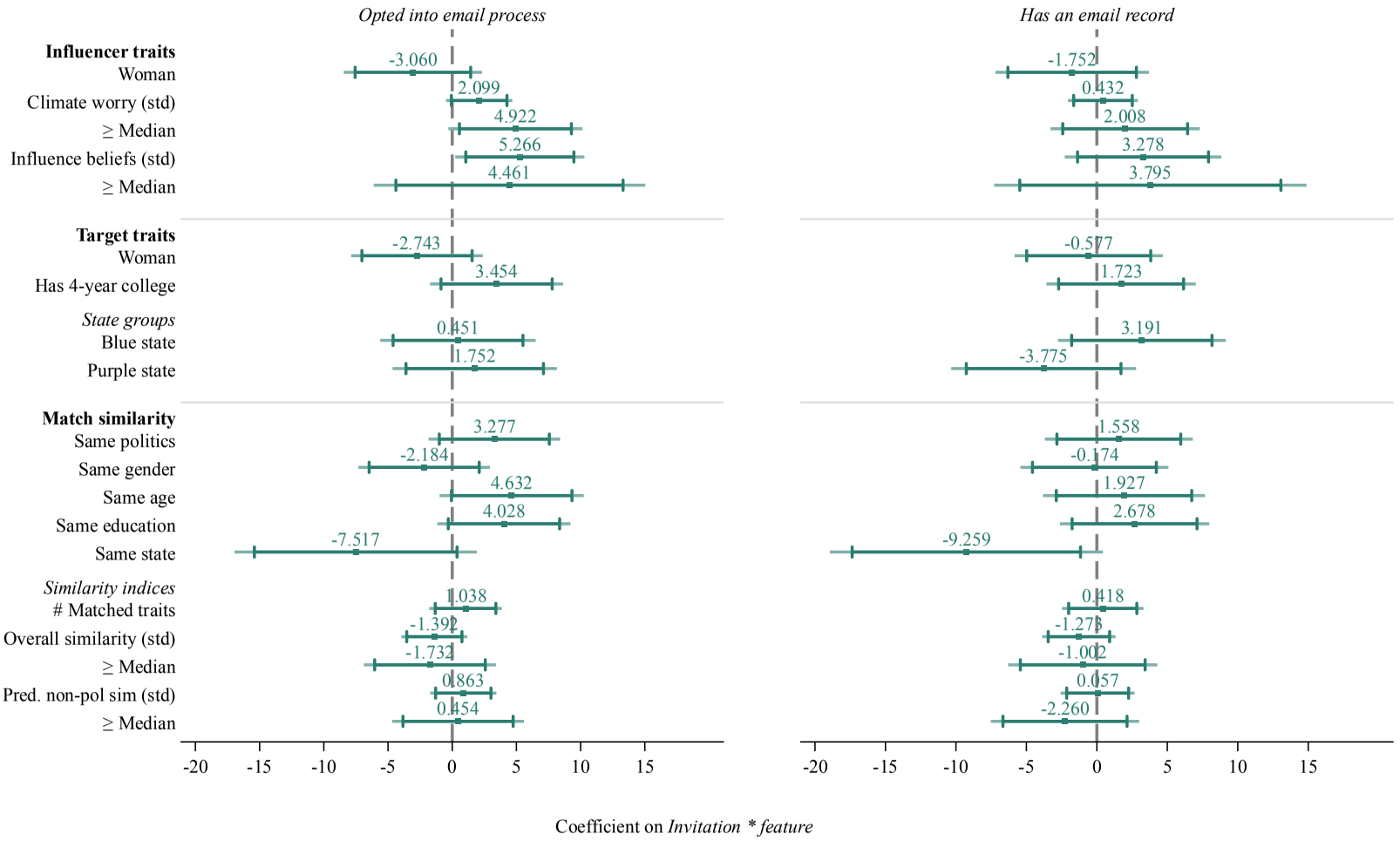


Panel B. How likely is your decision to affect what targets do after the survey?



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.

Figure B19: Heterogeneous influence motives (β_3) by inviter and match traits



Note: This figure plots differential estimates for β_3 in Equation 3 by Wave-1 influencer traits, Wave-2 target traits, and match similarity, estimated via separate regressions with interactions between each trait and both $Invitation_i$ and $Tell_i$. Appendix B.2.2 describes the baseline measure of climate worry and demographic traits. Appendices B.5.1 and B.5.2 describe how we measure influence beliefs and match similarity, respectively. We only observe influence beliefs in the Tell-after and Invitation groups, so we estimate a version of Equation 3 that excludes the $Tell_i$ coefficient and its interaction. Note that we standardize influence beliefs and identify participants as having above- or below-median influence beliefs separately among Wave-1 Democrats asked to predict influence on Democrats and Republicans. All regressions use the same specifications and controls as in Table 2. The darker and lighter capped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively.

B.5.1 Measuring Wave-1 influence beliefs ($\Delta P(Email)$)

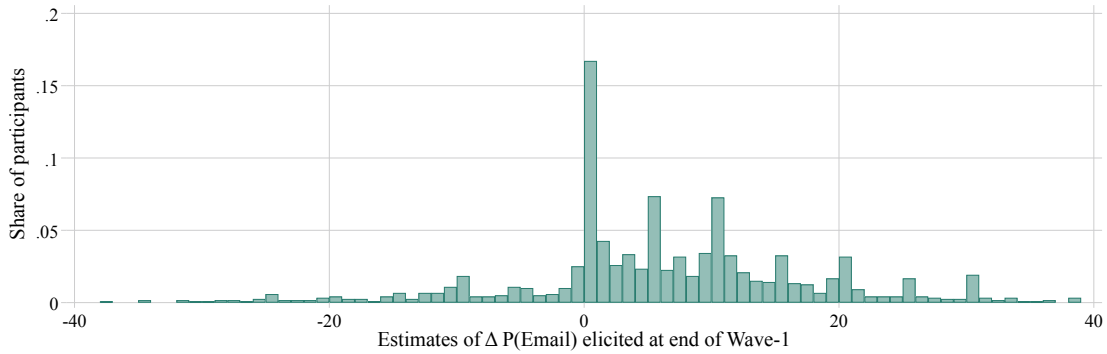
We test for heterogeneous Wave-1 effects by participants' influence beliefs, $\Delta P(Email)$. We elicit quantitative estimates for $\Delta P(Email)$ from a randomly-chosen 30% of Tell-After and Invitation participants at the end of the Wave-1 survey. We ask them, unincentivized, to predict the probability that a random future participant would email Congress if they did or didn't see an invitation to do so. We calculate influence beliefs as the change in probability that the future participant would email Congress if they saw the invitation. We plot their raw distribution in Appendix Figure B20. We find no evidence that treatment affects this end-of-survey measure (Appendix Table B3, column 1). Appendix F.1 describes how this measure of influence beliefs differs from our pre-registered plan.

Table B3: Wave-1 impacts on secondary outcomes

	(1) Influence beliefs	(2) Share Wave-1 participating	(3) Experimenter demand (std)	(4) Thermometer aff. polarization
Tell		10.782***	0.313***	
		(0.662)	(0.031)	
Invite	1.201	-0.639	0.059**	-0.012
	(0.795)	(0.644)	(0.028)	(0.030)
Control mean		29.766	0.000	
Tell-after mean	5.307			-0.000
N	1222	6071	6163	4321

Note: This table estimates treatment effects on secondary Wave-1 outcomes. Column 1 tests for impacts of the treatments on participants' influence beliefs, elicited among random subsets of the Tell-after and Invitation treatments at the end of the Wave-1 survey (see the note to Appendix Figure B19). Column 2 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 3 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. Column 4 estimates effects on thermometer-based affective polarization, elicited at the end of Wave 1 (Appendix B.5.3). All controls and specifications are as in Table 2. We present heteroskedasticity-robust standard errors in parentheses and indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

Figure B20: Distribution of Wave-1 estimated influence beliefs, $\Delta P(Email)$



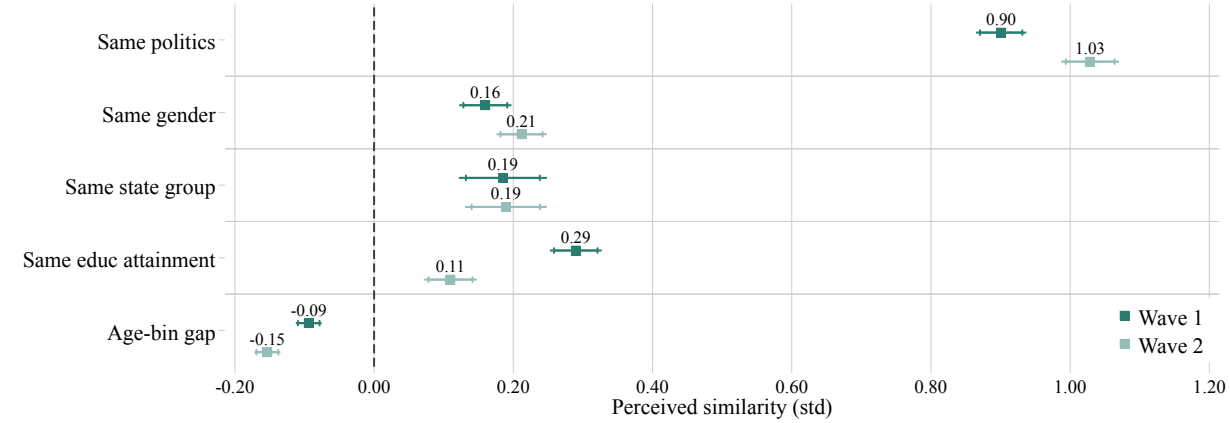
Note: This figure plots the distribution of Wave-1 participants' estimates for $\Delta P(Email)$, elicited at the end of the Wave-1 survey (Appendix B.5.1). N = 1,222. We drop 22 observations with values below -40pp or above 40pp.

B.5.2 Measuring Wave-1-Wave-2 match similarity

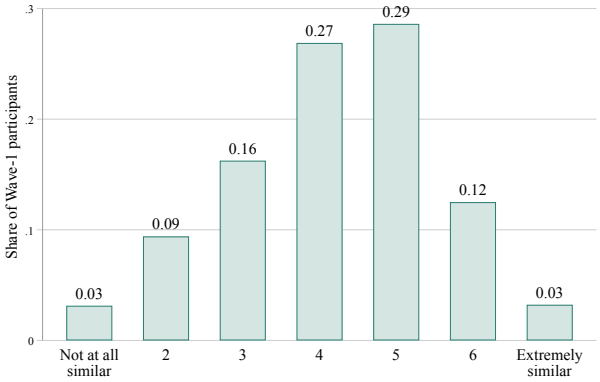
We also test for heterogeneous influence motives by match similarity. After showing Wave-1 participants their Wave-2 matches' demographic profile, we ask them to rate how similar they are on a scale from 1 (Not at all similar) to 7 (Extremely similar). In the Wave-2 experiment, we similarly ask Wave-2 participants to rate how similar they feel to Wave-1 matches after seeing their demographics. We then use these elicitations to predict perceived non-political similarity. First, we regress overall perceived similarity on indicators for matching on each demographic trait. We then predict perceived overall similarity from these estimates, subtracting the predicted contribution of shared party, and standardize this measure in our sample of interest. Appendix Figure B21 plots the determinants of perceived similarity and the distributions of elicited similarity and predicted non-political similarity in the Wave-1 sample.

Figure B21: Perceived similarity between Wave-1 and Wave-2 matches

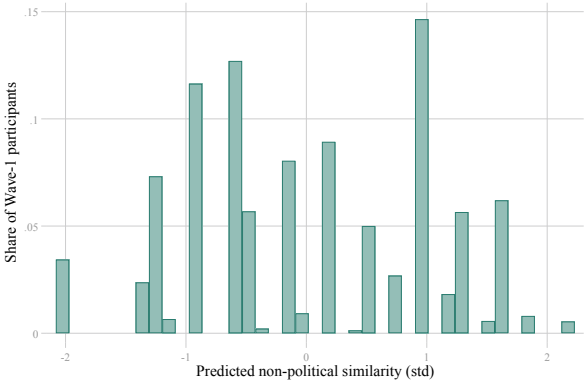
Panel A. Determinants of perceived similarity



Panel B. Distribution of raw similarity to W2 matches



Panel C. Predicted non-political similarity to W2



Note: Panel A presents results from regressing Wave-1 and Wave-2 participants' self-reports of how similar they feel to their Wave-2 or Wave-1 match, respectively, on measures of demographic similarity between them. We elicit similarity from 1 (Not at all similar) to 7 (Extremely similar) and standardize these reports to have mean zero and SD one in each sample. We regress these measures on whether participants match on politics, gender, state group, and educational attainment. We define a linear "age gap" measure by indexing the age bins (20-34; 35-49; 50-64; 65-79) and then calculating the absolute difference in these indices in Wave-1-Wave-2 pairs. We show heteroskedasticity-robust 90% and 95% confidence intervals. Panel B graphs the distribution of Wave 1's stated similarity to Wave-2 matches, and Panel C graphs the distribution of Wave 1's predicted non-political similarity to Wave-2 matches.

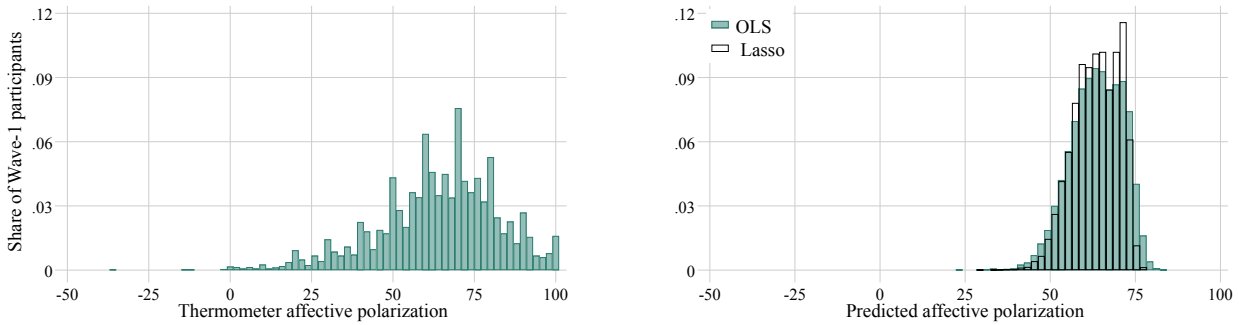
B.5.3 Measuring Wave-1 affective polarization

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 A4). 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 A4),[44] 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 B22).

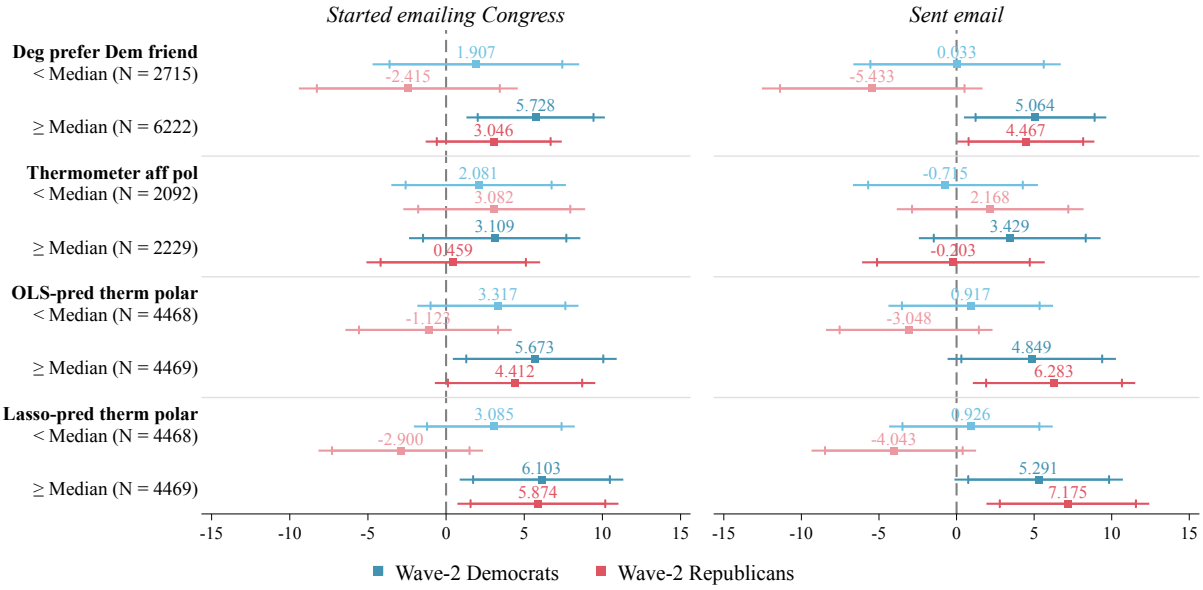
[44] We pre-registered that we would not test heterogeneity by this measure due to potential treatment effects, but we find no treatment effects on this polarization measure (column 4, Appendix Table B3). See Appendix F for more details.

Figure B22: Wave-1 heterogeneity by affective polarization

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



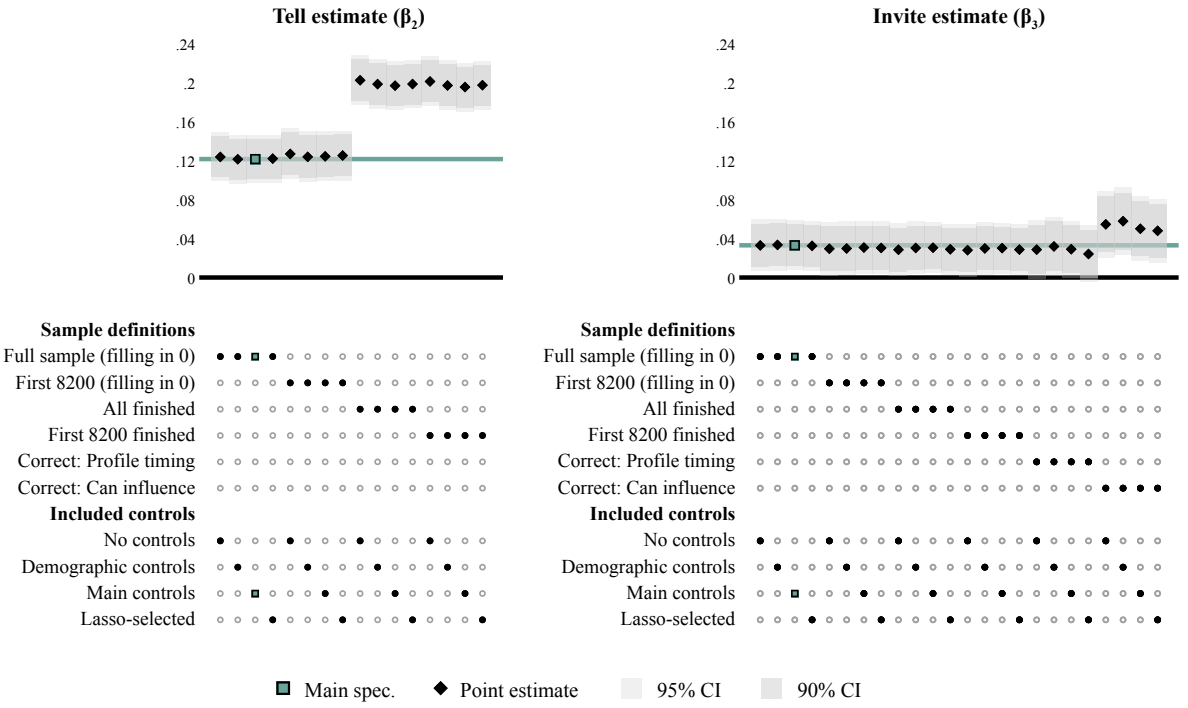
Panel B. Heterogeneous partisan gaps in β_3 by affective polarization



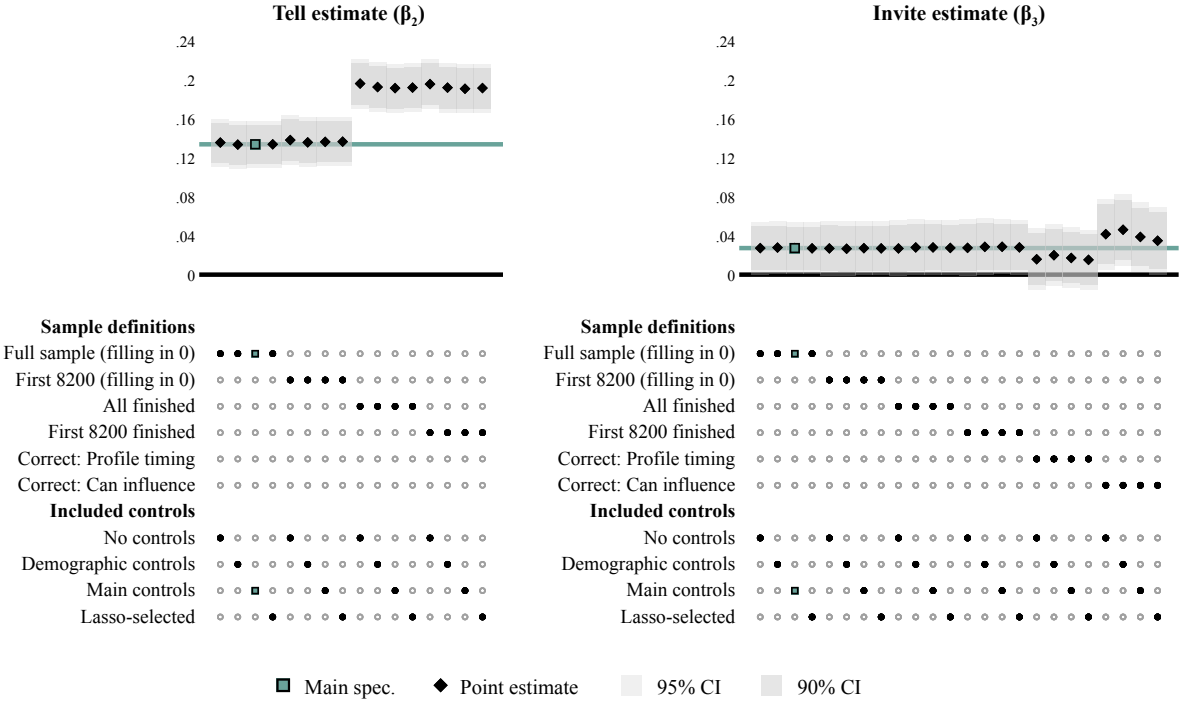
Note: Panel A plots the distributions of thermometer-based affective polarization among Invitation and Tell-after participants (N = 4,321) and predicted thermometer-based polarization in the full Wave-1 sample (N = 8,937), predicted from the demographics, climate beliefs, and political beliefs included as controls in the main Wave-1 regressions (Appendix B.2.2). Panel B estimates β_3 in Equation 3 separately for those with above- and below-median affective polarization and paired with Wave-2 Democrats and Republicans. These regressions are otherwise identical to those in Table 2. Sample sizes with above- and below-median polarization on each measure are in parentheses. Capped and uncapped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively.

Figure B23: Wave-1 specification charts

Panel A. Whether participants start the email process



Panel B. Whether participants match to an email record



Note: This figure plots estimates for β_2 and β_3 in Equation 5 under a series of specification tests. The outcomes in Panels A and B are whether participants start the email process and ultimately match to an email record, respectively. We estimate treatment effects under various sample restrictions and control specifications, as detailed in the text of Appendix B.5.4.

B.5.4 Wave-1 robustness

Sample definitions: The Wave-1 results are robust to changes in the experimental sample (Appendix Figure B23). 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.[45]

Our results for the Invitation versus Tell-after gap are also 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 choice to email Congress could affect whether Wave-2 matches do the same.[46]

Control variables: Our Wave-1 results are fully robust to the set of control variables we include (Appendix Figure B23): no controls, only demographic controls, 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. 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.

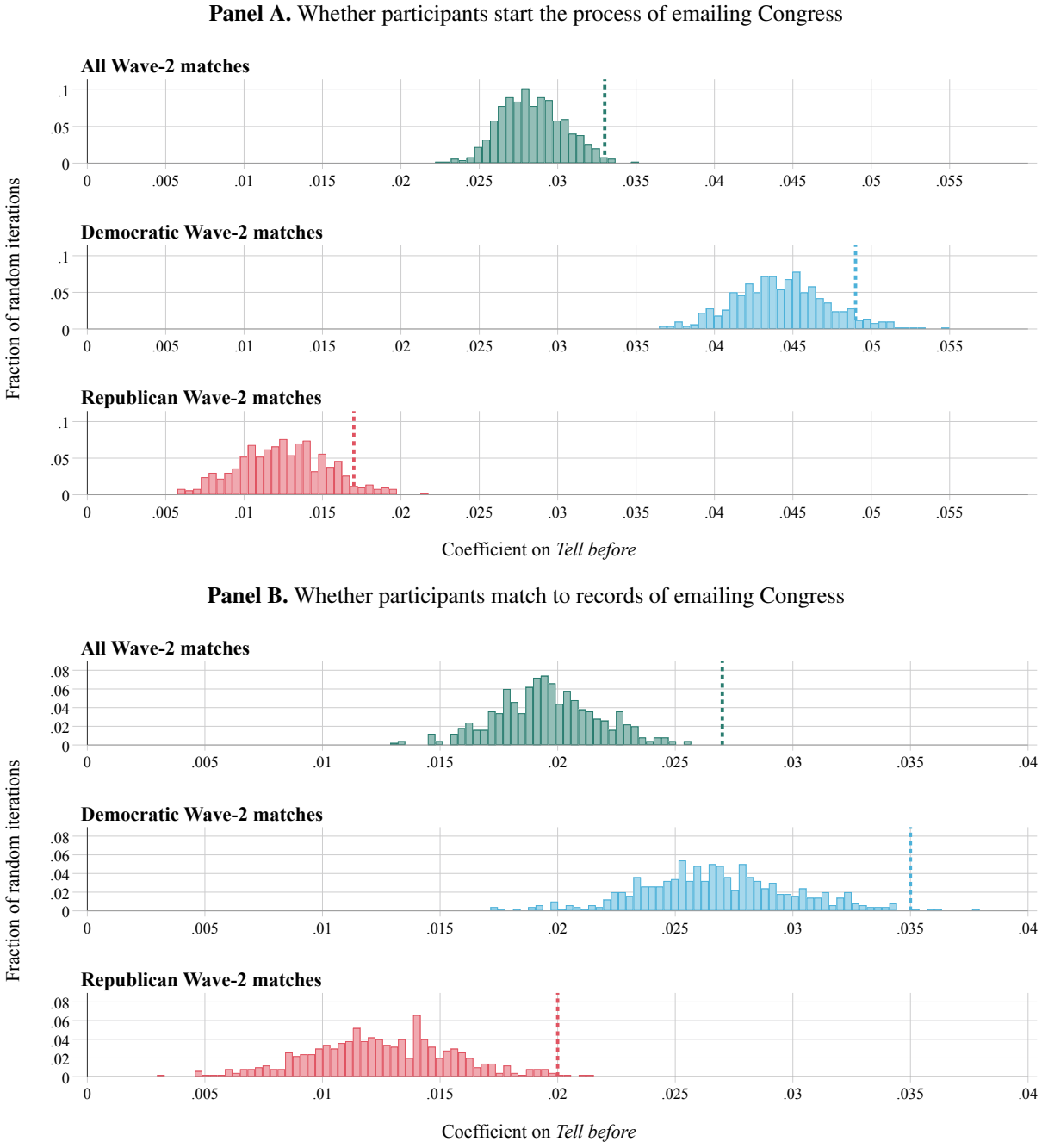
For completeness, we show that our estimates of influence motives are robust to eliminating any differential demand effects that remain. We elicit perceived experimenter demand by asking participants to rate how strongly they think we wanted them to email Congress; perceived demand is 0.06sd higher among Invitation participants than Tell-after participants (column 3, Appendix Table B3). 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

[45] 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. However, this shift simply means that our main estimates for the Tell-after effects are conservative.

[46] 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.

in each adjusted sample. Our point estimates fall on average by only a small amount when we correct for differential demand (Appendix Figure B24).

Figure B24: Estimated Invitation effect (β_3) with adjustment for differential demand effects



Note: This figure plots estimates for β_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 WTP set-up explanation

Figure C25: Explanation of the WTP survey

Slide 1.

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



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

Slide 2.

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

The two options are:

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

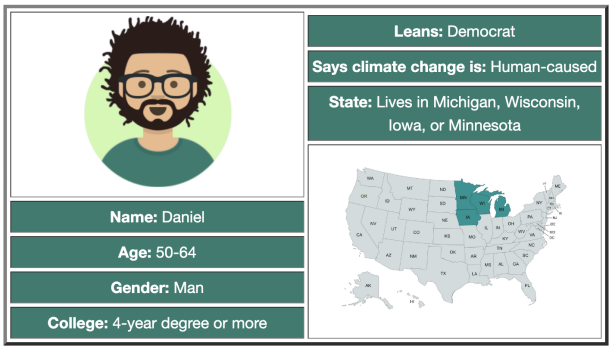
We'll provide more detail about each of these options in the next few slides.

Slide 3.

Option 1:

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

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



The next page will give more background on carbon offsets.

Slide 4.

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

What are **carbon offsets**?

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

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

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

Slide 5.

Option 2:

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

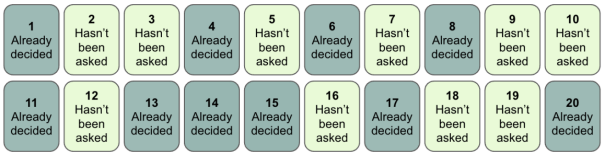
The appearance of this extended profile will depend on one key feature of the future participant you're paired with: whether **they've already decided whether to contact Congress or not**.

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

Slide 6.

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

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



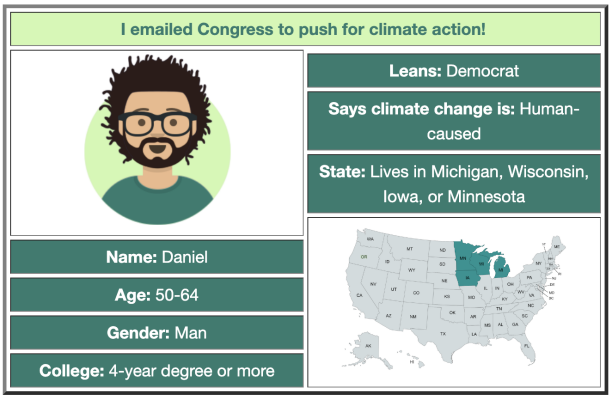
Here's what those mean:

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

Figure C26: Explanation of the WTP survey, cont.

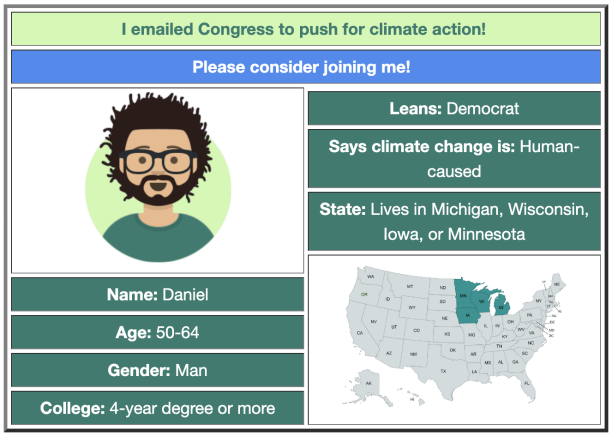
Slide 7.

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



Slide 8.

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



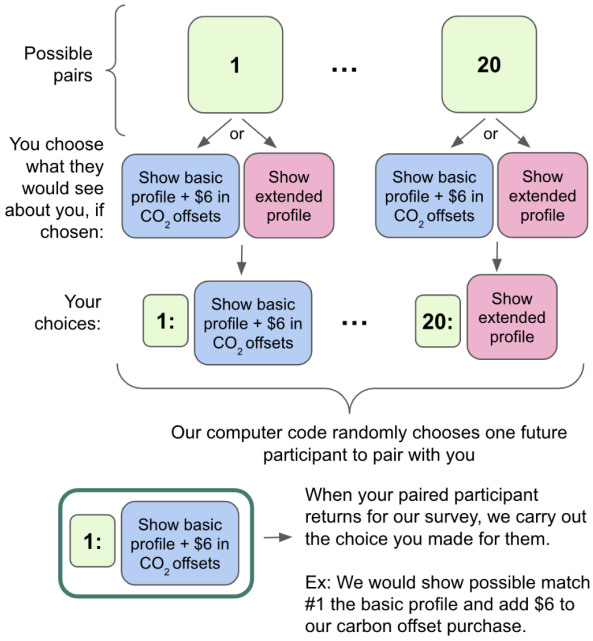
(Note that it includes an invitation to join you in acting, since they haven't yet decided whether to contact Congress or not.)

Slide 9.

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

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

Here's a diagram laying that out:



Slide 10.

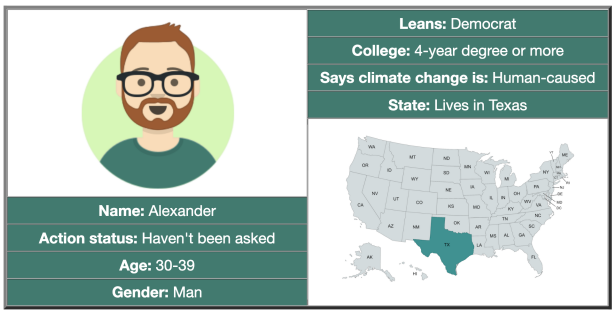
One last thing to note about the set-up:

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

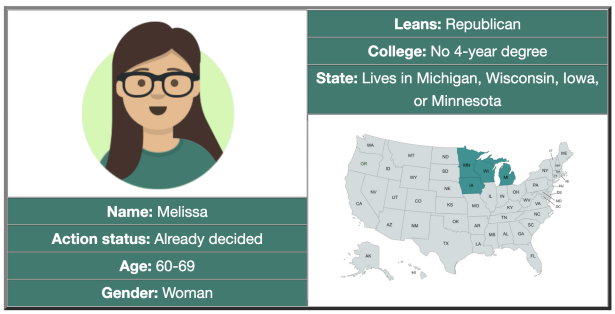
Note: This figure presents screenshots of the WTP experiment flow in which we explain the experimental set-up to participants. We randomize twhether we present the basic or extended profiles as option 1 or 2 in slides 2 through 8, whether we present the profiles shown to "Already decided" or "Haven't been asked" participants first in slides 7 and 8, and whether the example flowchart in slide 9 shows a basic or extended profile being chosen to be implemented. The survey flow then goes through a series of comprehension questions, shown in Appendix C.3, before participants make their profile choices.

Figure C27: Examples of profiles of possible WTP matches

(a) Before, with climate-belief info



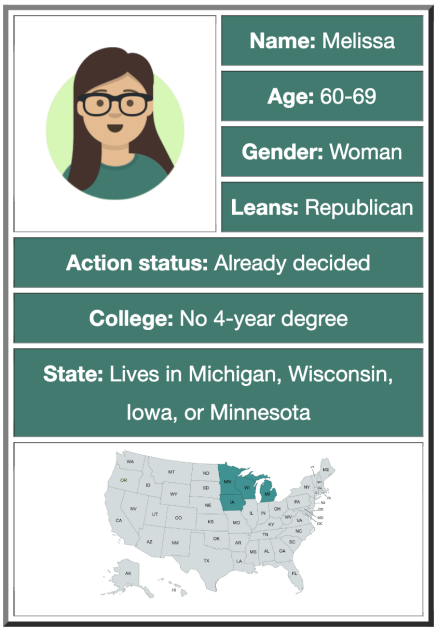
(b) After, without climate-belief info



(c) Before, with climate-belief info

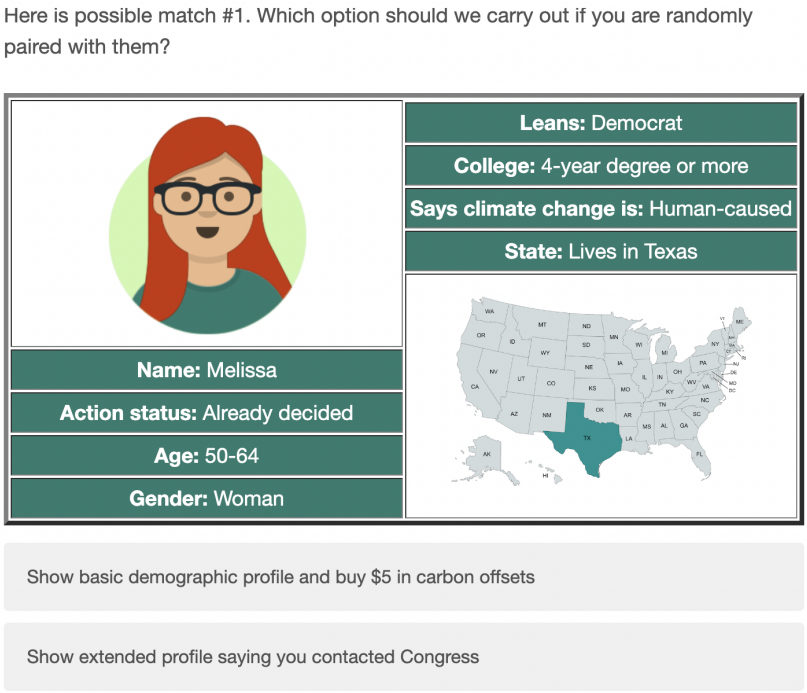


(d) After, without climate-belief info



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. Sub-figures (a) and (b) show profiles shown to WTP participants using desktop computers, while (c) and (d) show profiles shown to mobile users. 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 (subfigures (a) and (c)), while randomizing others to see profiles without this information (subfigures (b) and (d)).

Figure C28: Binary choices between profiles



Note: This figure shows a screenshot of the page on which participants choose between a basic and extended profile for each possible match. Participants make these choices for 20 possible matches. To encourage them to pay attention to each possible match, they must stay on each choice page for 8 seconds.

C.2 WTP recruitment and completion

Sample recruitment: We recruit WTP participants from "inframarginal climate activists," or 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 source samples, we invite participants who emailed Congress to take an extra 15- to 20-minute survey section in exchange for being entered into an additional gift-card lottery. We frame this survey section as an opportunity for them to choose how we should spend our time and research funding to have impact on climate change.

In total, 3,616 were assigned to the Wave-1 Control group and 1,205 were recruited in the top-up sample who completed an equivalent survey. Of these total 4,821 participants, 1,657 (34%) said that they emailed Congress during the survey. While we were unable to confirm in real-time that they'd done so, 85% of these match with email records. We invited these 1,657 participants to take the additional WTP survey, and 1,519 (92%) began the survey. 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: Of the 1,350 participants who begin the main WTP version, 1,239 go through the full

description of the WTP set-up and begin answering comprehension questions (Appendix C.3). 1,109 participants are randomized to either see profiles of matches that show or hide their beliefs about climate change, and 1,091 participants finish answering the final comprehension question after this randomization. 1,058 participants then start making binary profile choices, and 1,023 finish making all 20 choices. These participants compose our main analysis sample.

Table C4: WTP attrition

	(1)	(2)	(3)
	Finish 20 WTP choices		
	Round 1		
	Main WTP variant	Both WTP variants	*Round 2*
Has climate-belief info	-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 sample. Across columns, 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 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 from other participants in the second-round WTP experiment (Appendix E.7).We present heteroskedasticity-robust standard errors in parentheses and indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

Table C5: 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	Hide 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 1 gives more detail on baseline traits.

C.3 WTP comprehension questions

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

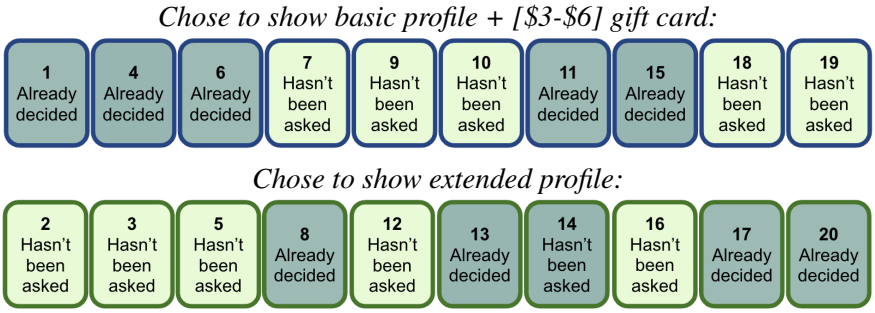
- **Step 1.** We remind participants that we will show them 20 other study participants that we will recontact for follow-up surveys, and to whom they may be randomly paired. These participants would see their profile either before or after choosing whether to email Congress.

- **Step 2.** For each of these 20 options, which of the following two options can they choose for us to implement, in case that match is randomly paired with them and returns for their survey?

  - We show the match their basic demographic profile and add [CORRECT AMOUNT] to our team's purchase of carbon offsets
  - We show the match their basic demographic profile and add [INCORRECT AMOUNT] to our team's purchase of carbon offsets
  - We show the match their extended profile saying that they contacted Congress
  - We show the match nothing about them

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

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



- **Step 3.** Next, we remind participants that a computer program will randomly choose one of the 20 study participants to pair with them when that match returns for a follow-up survey. We ask participants to imagine that the computer program randomly paired them with participant #10 and ask them three questions in that case.

  1. What information about you would participant #10 see about you when they return for their follow-up survey?

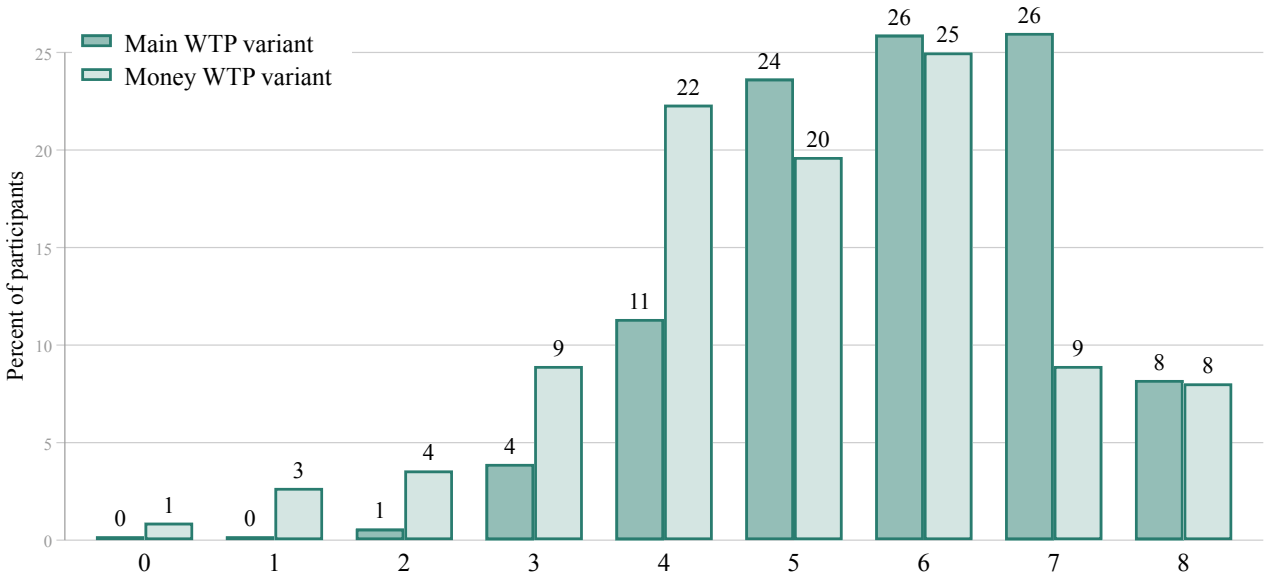
- A profile including just your demographics
- A profile that just says you contacted Congress, with no demographics
- A profile that includes your demographics and says you contacted Congress

2. And what amount of money would we donate to carbon offsets on your behalf, when participant #10 returns? [Multiple choice among $0, the correct amount, and 2 incorrect amounts]
3. Would participant #10 be choosing whether or not to contact Congress after they saw your profile? [Yes / No]

We then correct participants' answers and reiterate that participant #10 would see their basic demographic profile, that we would add the correct amount to our carbon offset purchase, and that participant #10 would choose whether to contact Congress after seeing their demographic profile. We then repeat this process for participant #8.

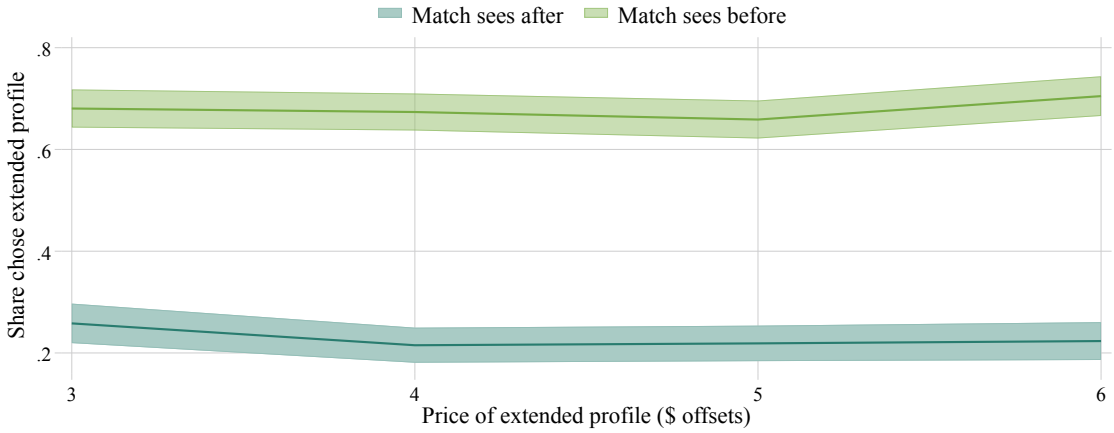
- Finally, we ask participants to imagine that the computer program paired them with participant #5 instead of participant #8. We ask whether there is any difference between the extended profile that would be shown to participant #8 and that shown to participant #5, if they were randomly chosen. We correct their answer and reiterate that the invitation shown to participant #5 would also include an invitation to join in action, since they would see it before deciding whether to email Congress.

Figure C29: 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, split by those in the main WTP variant and those in the WTP variant with a gift-card option, described in Appendix C.4. The figure restricts to those who finished all 20 binary choices, all of whom were asked 8 comprehension questions.

Figure C30: 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.

C.4 WTP design with gift-card option

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 show the returning match the WTP participant's basic demographic profile and send them a gift card valued at the same amount as the carbon-offset donation. 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 C4). 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.6 shows that all of our results are fully robust to including all participants.

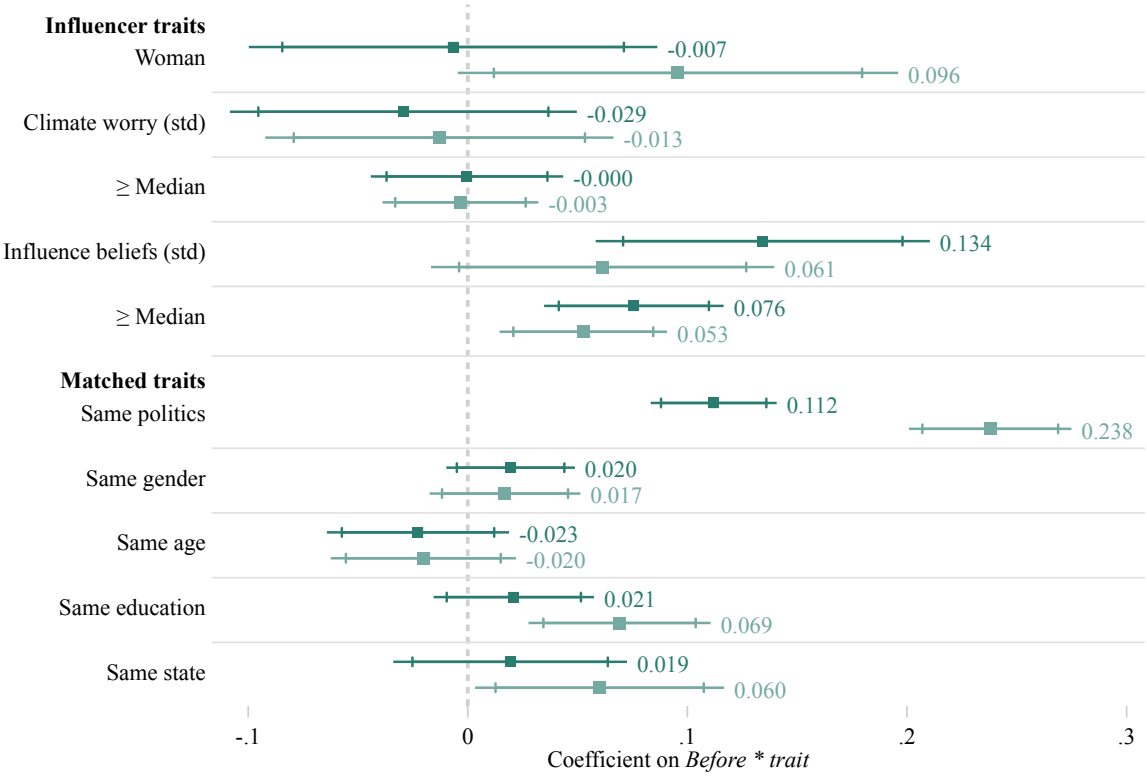
WTP participants value carbon offsets: While participants chose the basic profile and offset donation in an average of 10.5 choices, they chose the basic profile and gift card in an average of 1.2 choices; about 80% of participants never chose the take-home gift card. Thus, most participants value one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

There are two remaining concerns. First, we might worry that participants avoid choosing the gift cards due to the perceived social desirability of taking a pro-climate action. To mitigate this risk, we explicitly frame all of the options as ways to take action on climate change: in presenting the gift-card option, we suggest that participants could use it to buy an eco-friendly product or free up other money to donate to an environmental organization. Second, we might worry that participants 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.

C.5 Other WTP heterogeneity

Figure C31: Heterogeneous influence motives by matching traits and influencer characteristics



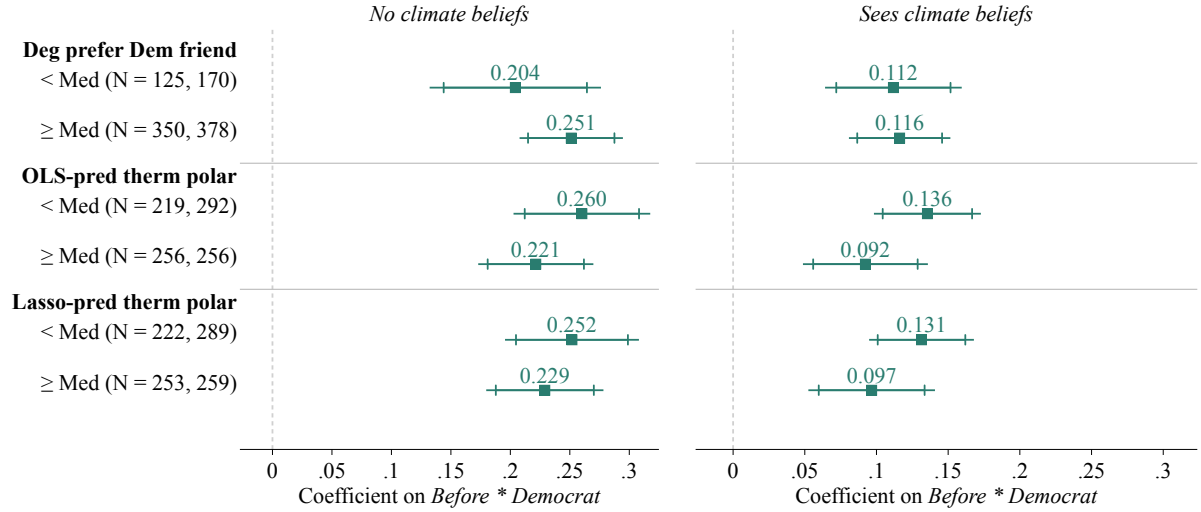
Note: This figure tests additional WTP heterogeneity. First, we test for differential influence motives among WTP participants who are women, have stronger climate worry, and have stronger influence beliefs by separately estimating variants of the following specification: $Extended_{ij} = \alpha_i + \beta_1 Before_{ij} + \beta_2 Before_{ij} * Trait_i + \varepsilon_{ij}$. Note that the main effect for each trait is collinear with participant fixed effects. Appendix C.5 describes how we measure influence beliefs here; we standardize and calculate medians in the WTP sample. Next, we test whether WTP participants differentially seek to mobilize possible matches that match them on particular non-political traits, estimating a variant of equation 6 that jointly includes indicators for matching on each trait and their interactions with $Before_{ij}$. Note that the indicator for having the same political leanings is co-linear with matches being Democrats. 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% confidence intervals, respectively.

Measuring influence beliefs: Like in Wave 1 (Appendix B.5.1), we elicit WTP participants' unincentivized influence beliefs at the end of the survey. We ask them to predict the probability that two randomly chosen hypothetical participants would email Congress if they did or did not see an invitation from the WTP participant to join in doing so; we then calculate the impact of the invitation on each hypothetical recipient. Two factors strongly predict these elicited influence beliefs: whether hypothetical targets are Republicans and Democrats and whether WTP participants see matches' climate beliefs (including of these hypothetical targets). We standardize predicted influence within the four categories from the interaction of these traits, average the two values per participant, and standardize these averages in the WTP analysis sample.

Measuring affective polarization: Unlike in Wave-1, we don't directly observe thermometer-based affective polarization for WTP participants. Thus, we measure affective polarization using preferences for Democratic friends and predicted thermometer-based polarization (via OLS and lasso; Appendix B.5.3).

Measuring match similarity: We don't elicit WTP participants' perceived similarity to each of their possible matches, so we predict each pair's non-political similarity using the prediction model derived from Wave-1 participants' stated similarity to their Wave-2 matches (Appendix B.5.2).

Figure C32: Heterogeneous WTP partisan influence gaps by affective polarization



Note: This figure plots estimates for β_2 in Equation 6 separately among WTP participants with above- and below-median affective polarization on three measures (Appendix C.5). These regressions are otherwise identical to those in Table 3. Sample sizes with above- and below-median polarization and without and with climate-belief information (at left and right, respectively) are given in parentheses. We calculate medians among the WTP sample. Capped and uncapped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively.

Table C6: Heterogeneity in partisan recruitment gaps by 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 A.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.

C.6 WTP robustness

Sample definitions and demand effects: Our WTP results are robust to using a range of adjusted samples (Appendix Figures C33 through C34). First, they 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), and to excluding the 166 participants in our main analysis sample who answered fewer than 5 comprehension questions correctly. Next, our results are robust to excluding the 7 participants in our main sample who explicitly mentioned being concerned that Republicans would email Congress *opposing* climate policy. We have tried 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.

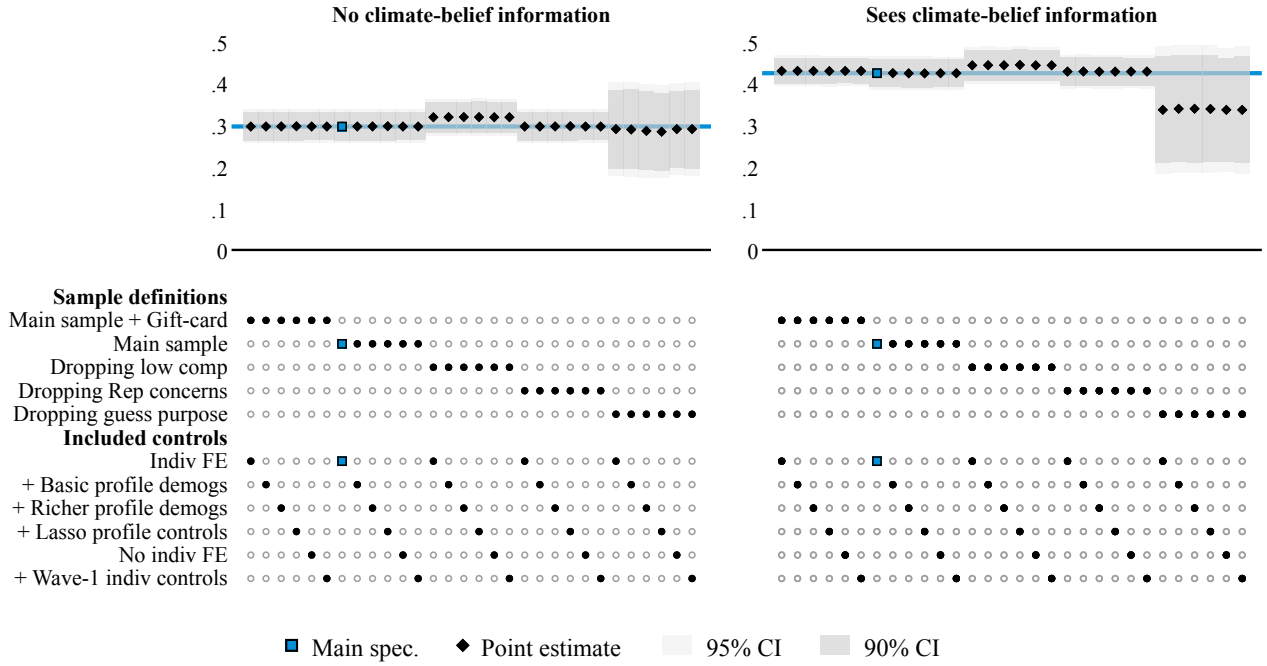
Finally, we might be concerned that our results are driven by experimenter demand effects, if participants guess that we are studying differential outreach by political party. Our main results are robust to excluding participants who guess this study purpose in a free-text response at the end of the WTP survey, though the coefficient on $Democrat_{ij}$ in Equation 6 falls. We manually identify 83 such participants among 125 participants whose answers for study purpose include keywords such as "politics," "party," and "Republican."

Control variables: Next, our WTP results are fully robust to the set of control variables we include (Appendix Figures C33 through C34). While our main specifications include only WTP participant fixed effects and choice-number fixed effects, our estimates are quite stable to adding controls for basic match demo-

graphics (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).

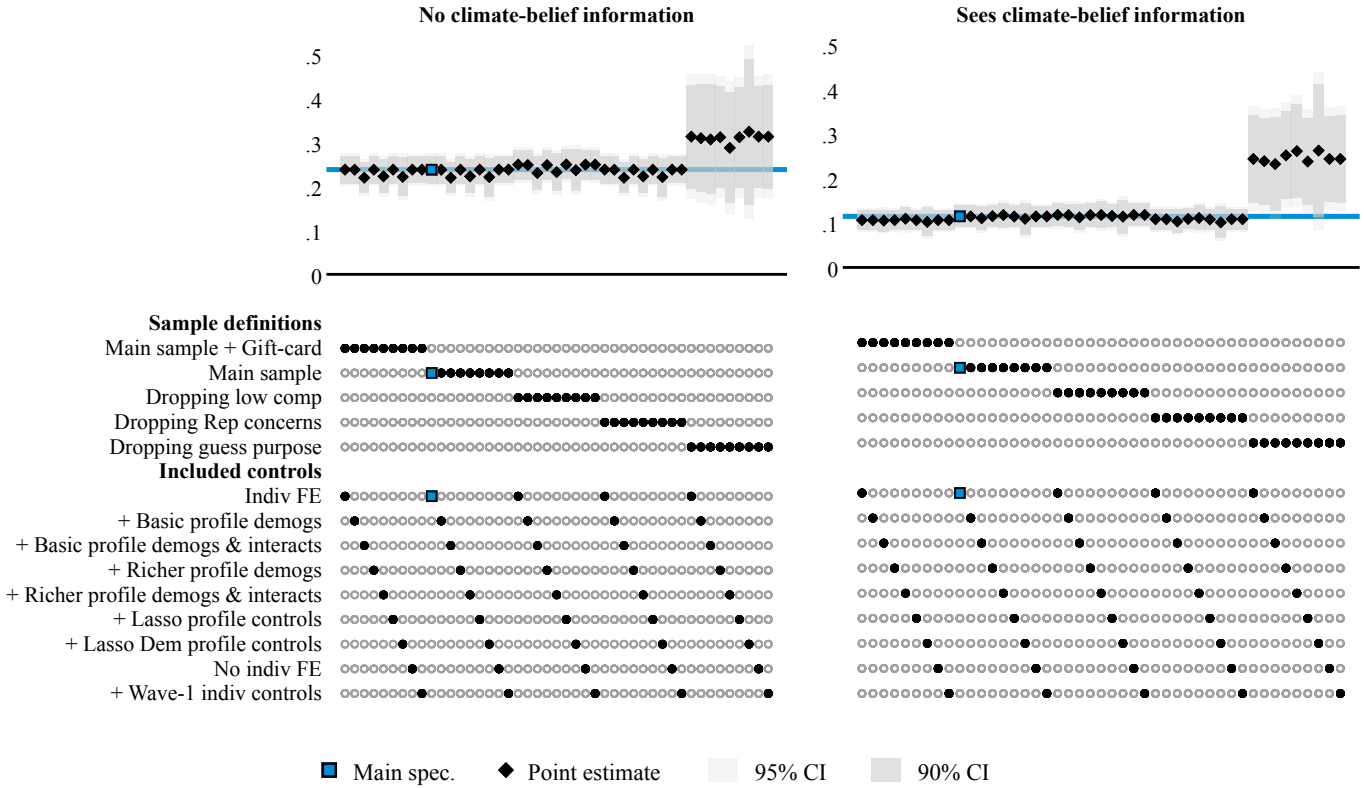
Moreover, we also show that the coefficients on Dem_{ij} and $Before_{ij} * Dem_{ij}$ in Equation 6 are robust to controlling for the interactions of all match demographics with $Before_{ij}$ and for the main effects and interactions with $Before_{ij}$ of all traits selected by Lasso to predict Dem_{ij}. Thus, we are confident that our estimates for partisan gaps in influence motives are not capturing correlations between matches' politics and other traits; this is theoretically possible, as political party is not randomly assigned here. (Note that we do not test that the coefficient on $Before_{ij}$ (β_1) is robust to controlling for the interactions of $Before_{ij}$ with match demographics, as doing so changes the object that β_1 measures.)

Figure C33: Specification chart: WTP experiment, β_1 coefficient on $Before_{ij}$



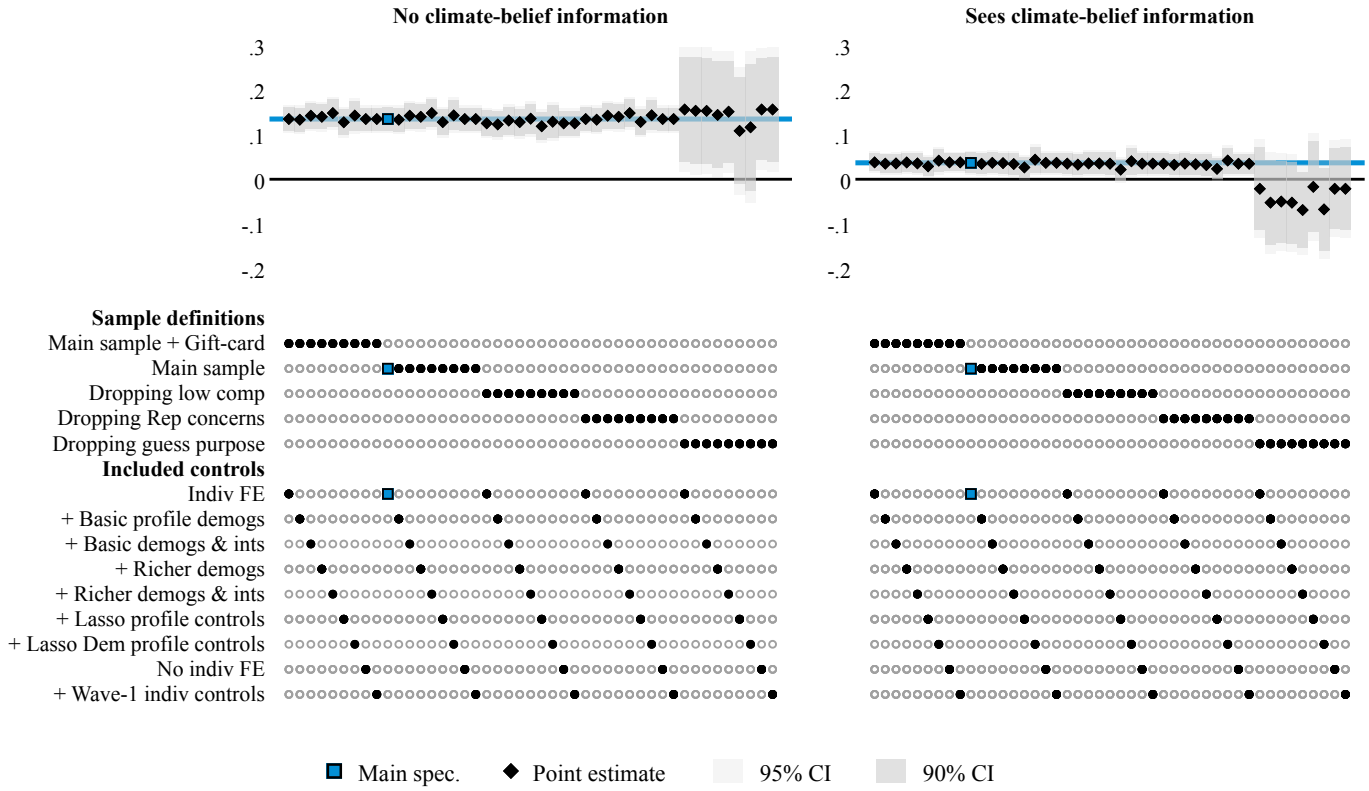
Note: This figure plots our estimates for β_1 in Equation 6 under a range of sample and control specifications (described in Appendix C.6). The panels at left and right plot coefficients in the WTP subsamples who saw profiles without and with information on matches' climate beliefs, respectively. The outcome variable is whether participants pass on an extended profile showing that they emailed Congress.

Figure C34: Specification chart: WTP experiment, β_2 coefficient on $Before_{ij} * Dem_{ij}$



Note: This figure is analogous to Appendix Figure C33, but with estimates for β_2 in Equation 6.

Figure C35: Specification chart: WTP experiment, β_3 coefficient on Dem_{ij}



Note: This figure is analogous to Appendix Figure C33, but with estimates for β_3 in Equation 6.

D Appendix on mechanism experiments

D.1 Eliciting $\Delta P(Email)$ influence beliefs

Figure D36: Explanation of the belief elicitation

Slide 1.

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

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

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

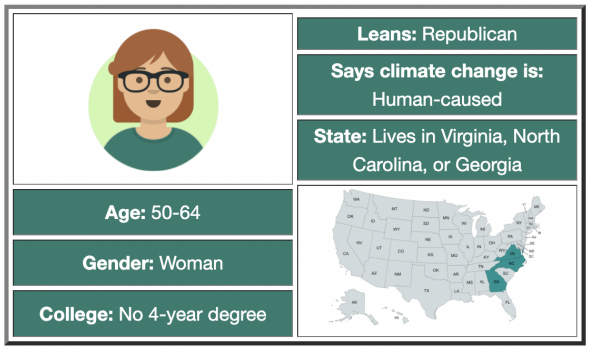


Throughout these questions, please **do not click back** in your browser. Doing so will send you back to the beginning of the survey.

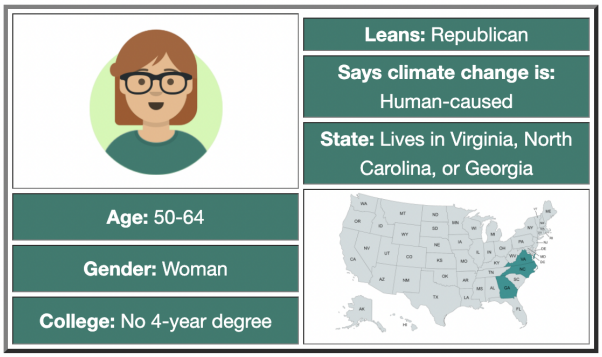
Slide 2.

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

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

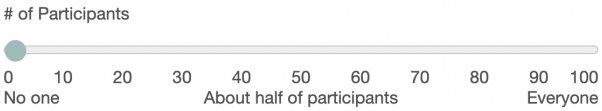


Slide 3.



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

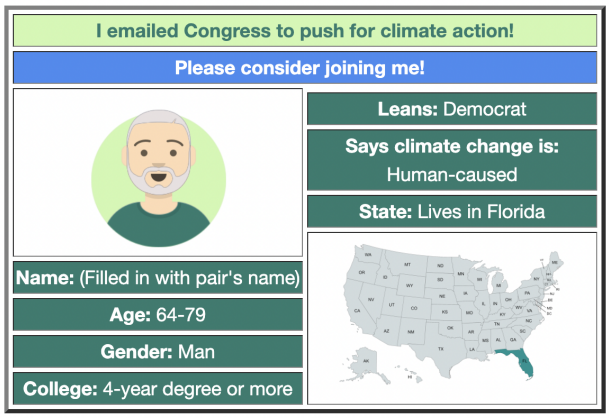
How many of those 100 participants do you think would choose to **email Congress about climate policy** during our survey?



Slide 4.

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

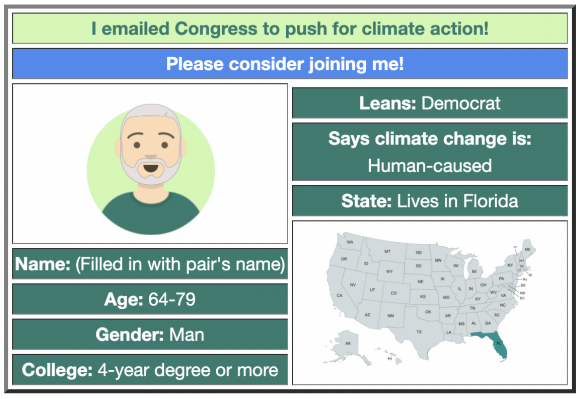
If their paired earlier participant **emailed Congress** when they took our survey, we'll **show the future participant** an extra profile of their earlier match that looks like this:



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

Figure D37: Explanation of the belief elicitation, cont.

Slide 5.

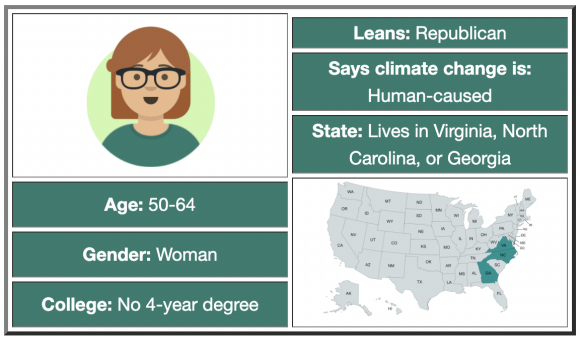


If 100 people from your paired group of future participants (see profile below) **saw one of these profiles before** they decided to email Congress or not, **how many** do you think would choose to **email Congress** during our survey?

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

# of Participants

0 10 20 30 40 50 60 70 80 90 100
No one About half of participants Everyone

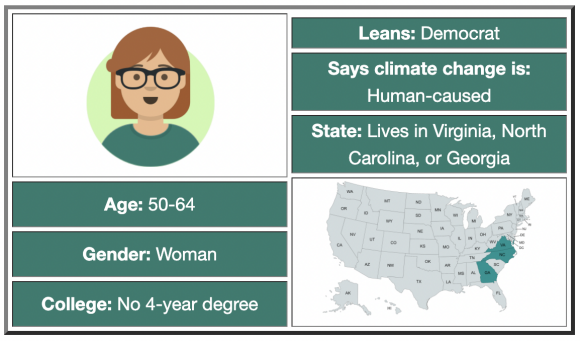


Slide 6.

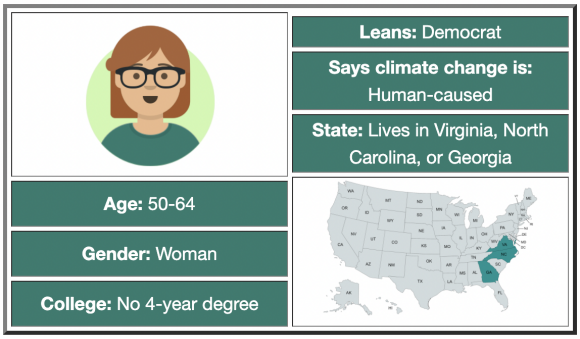
Thank you for doing that! We'll now ask you to make the same kinds of predictions for **a different demographic group** of future participants who are **quite similar** to your paired group.

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

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



Slide 7.



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

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

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

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

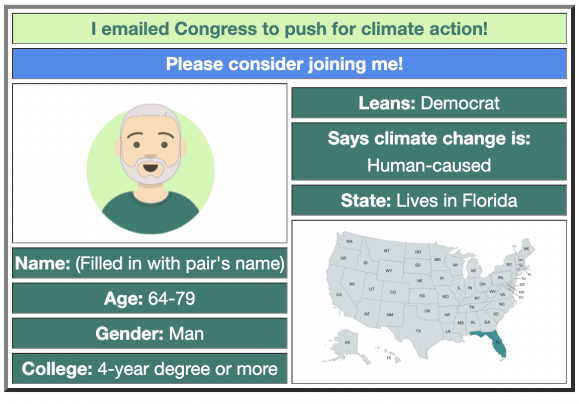
# of Participants

0 10 20 30 40 50 60 70 80 90 100
No one About half of participants Everyone

Slide 8.

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

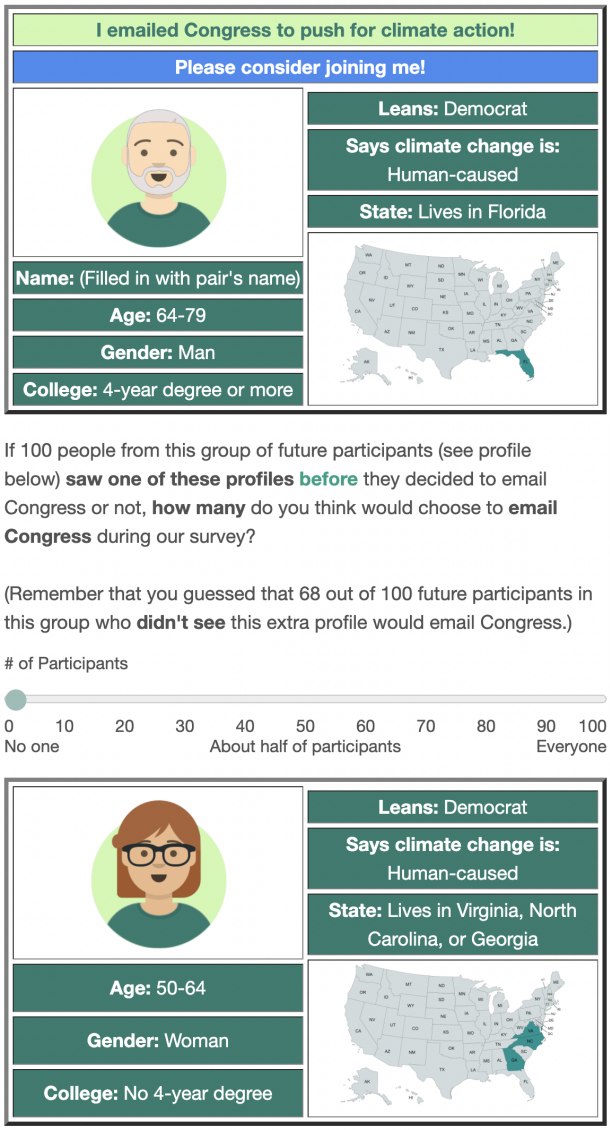
If their paired earlier participant **emailed Congress** when they took our survey, we'll **show the future participant** the following profile of this earlier match:



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

Figure D38: Explanation of the belief elicitation, cont.

Slide 9.



Incentives description:

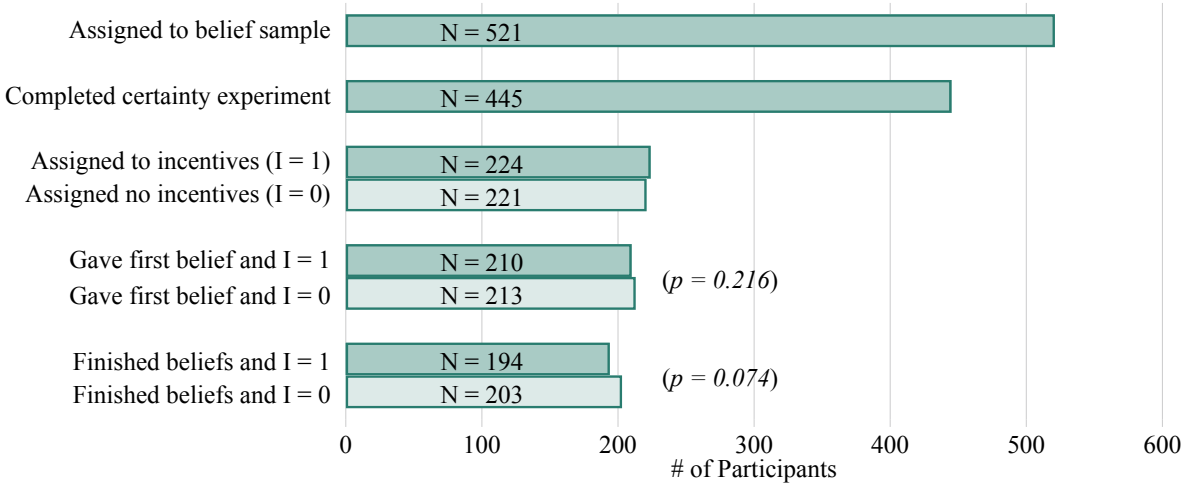
You can **win money** by making **accurate guesses** for how many participants will contact Congress.

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

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

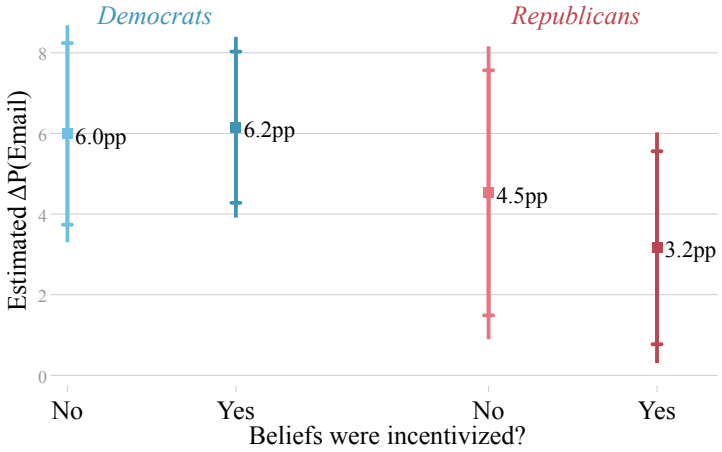
Note: Figures D36 through D38 show screenshots of the survey flow in which we elicit participants' beliefs about the impacts of invitations on whether Wave-2 participants email Congress. Participants are randomized into this survey sequence after seeing the upcoming opportunity to email Congress; the survey to this point is identical to the main Wave-1 survey. They then complete the certainty experiment described in Section 6.3 and Appendix E.6; this experiment includes the 10 choices referenced in slide 1 here. After completing the belief elicitation, participants choose whether to email Congress or not and then conclude the survey. Half of participants are randomly assigned to make incentivized choices, with the description of incentives shown after slide 1.

Figure D39: 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 E.6) 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.

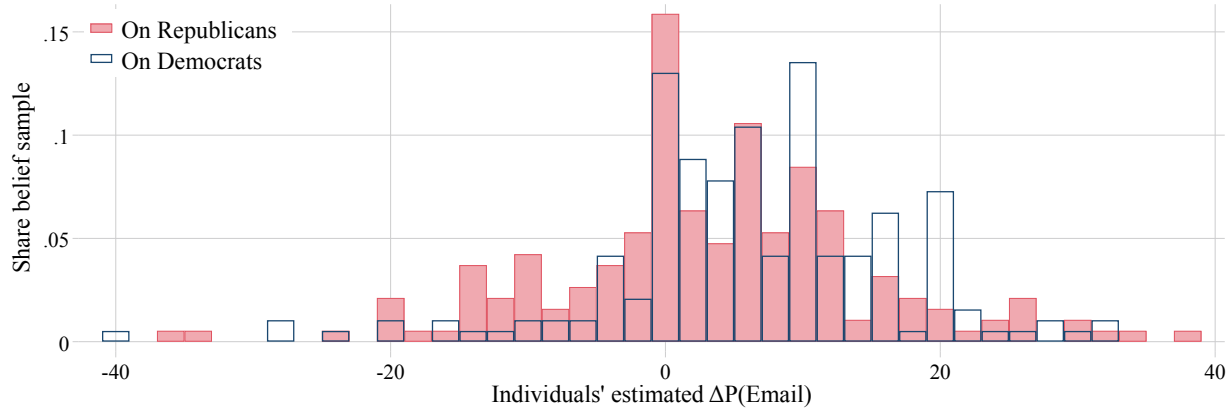
Figure D40: Comparing estimated $\Delta P(Email)$ with and without accuracy incentives



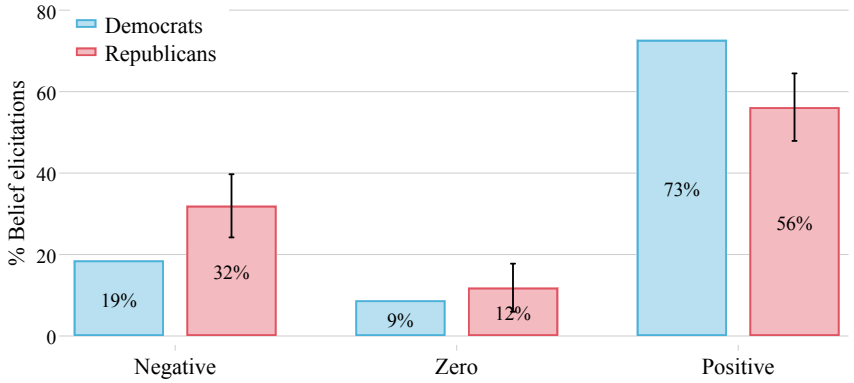
Note: This figure compares our estimates for participants' $\Delta P(Email)$ beliefs with and without accuracy incentives. We estimate Equation 7 separately among beliefs about Wave-2 Democrats and Republicans and by whether belief participants had accuracy incentives or not (see footnote 26). 194 participants 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 D41: Other views on $\Delta P(Email)$ beliefs by target politics

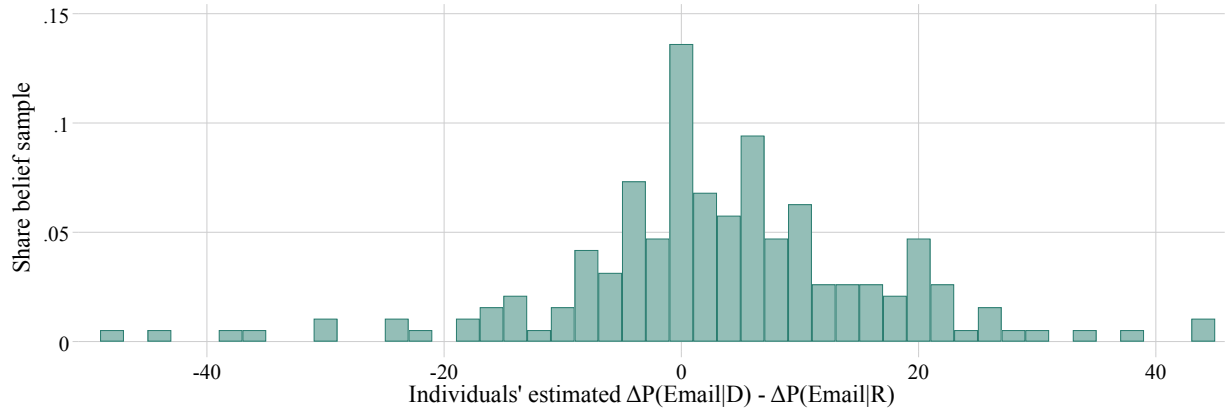
Panel A. Distribution of $\Delta P(Email)$ influence beliefs by political party



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



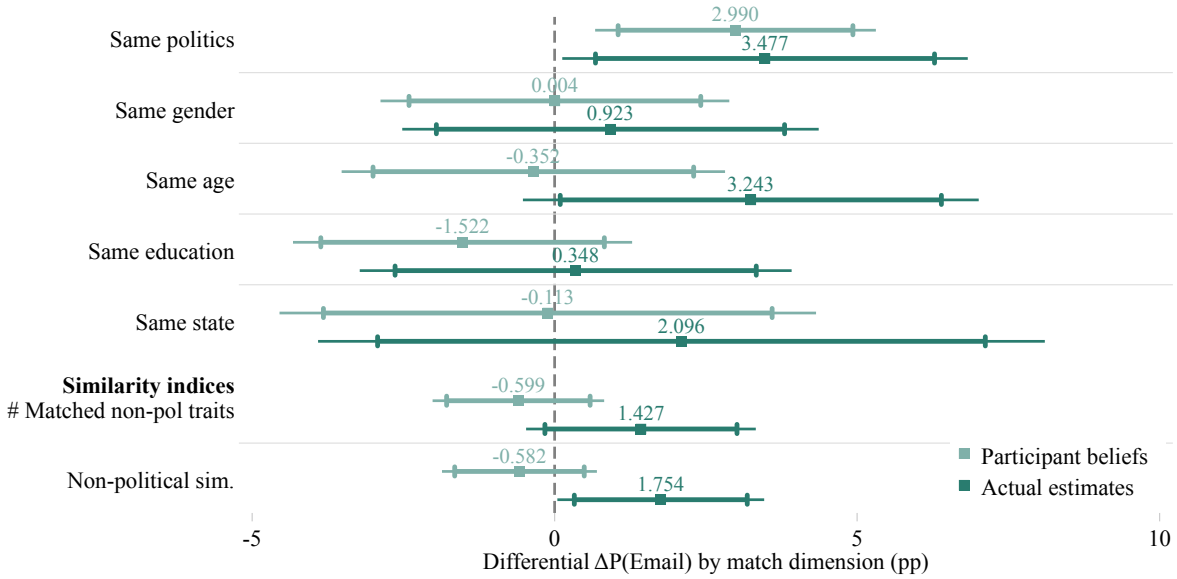
Panel C. Distribution of individuals' beliefs for $\Delta P(Email|D) - \Delta P(Email|R)$



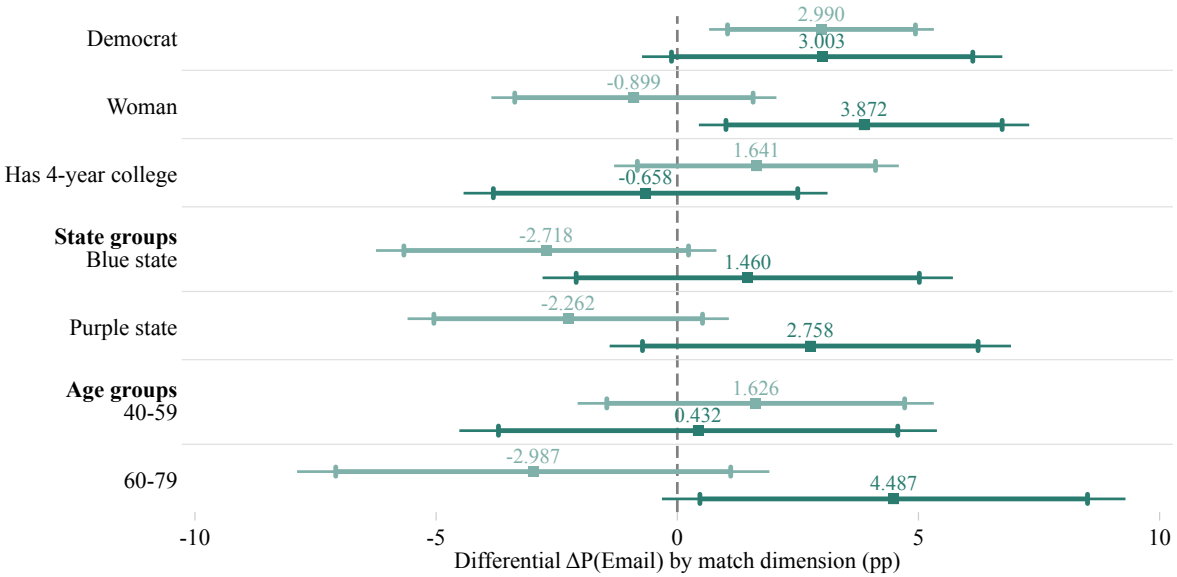
Note: Panel A plots the distribution of participants' individual estimates of $\Delta P(Email)$, calculated as how many more of 100 members of a particular Wave-2 demographic group they think would email Congress if they saw an invitation from a member of the belief participant's own demographic group 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 D42: Differential perceptions of and actual $\Delta P(Email)$ by match similarity and recipient traits

Panel A. By senders' and recipients' political and non-political similarity

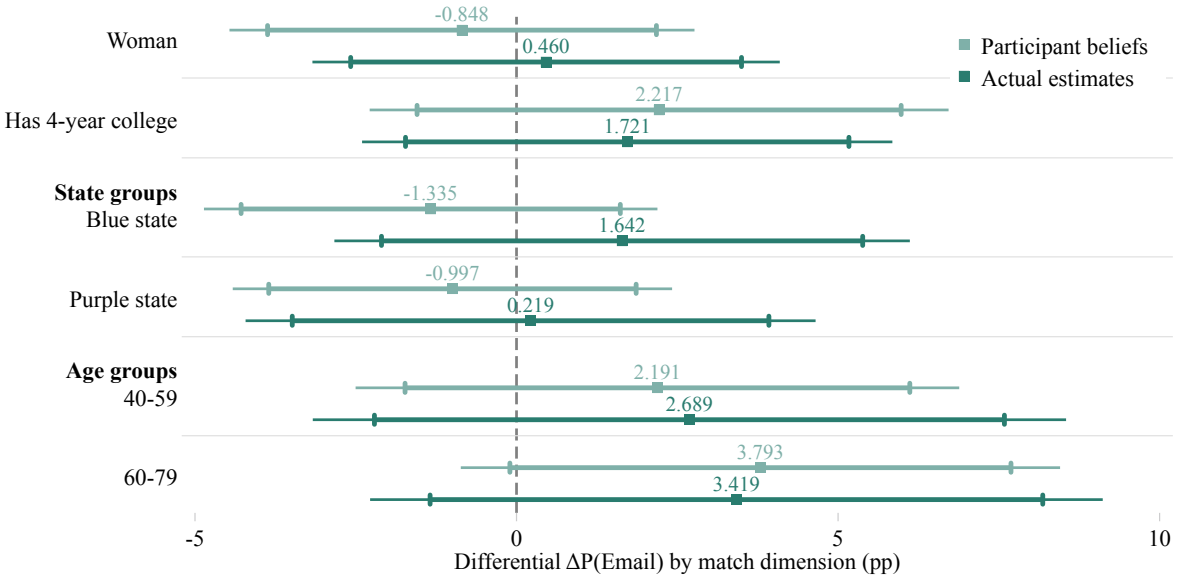


Panel A. By Wave-2 recipient traits



Note: The panels of this figure compare the belief sample's estimates for differential $\Delta P(Email)$ by sender-recipient similarity (Panel A) and Wave-2 recipient traits (Panel B). Measures of non-political similarity in Panel A are defined as in Figure 3. Note that we calibrate non-political similarity in models of the belief sample's predictions from Wave 1's reported similarity to their Wave-2 matches, while estimating this measure in models of the true $\Delta P(Email)$ effect from Wave-2 participants' reported similarity to their Wave-1 matches. We estimate differential influence beliefs in versions of Equation 7 in which we simultaneously interact $Profile_i$ with each of the similarity traits and Wave-2 recipient traits. Likewise, we estimate actual differential $\Delta P(Email)$ by match traits in versions of Equation 8 in which we simultaneously interact $Treatment_i$ with each of these traits. We estimate separate regressions with interactions for matching on each of the particular traits in Panel A, for the indices of non-political similarity in Panel A, and for the recipient traits in Panel B. When interacting the invitation treatment with indices of non-political similarity, we also include an interaction of treatment with matching on politics. Standard errors...

Figure D43: Differential perceptions of and actual $\Delta P(Email)$ by Wave-1 senders' traits



Note: This figure is fully parallel to Panels A and B of Appendix Figure D42, but it tests for differential perceived and actual $\Delta P(Email)$ by Wave-1 inviter traits.

Table D7: Comparing the belief and certainty samples to the Wave 1 and WTP samples

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

Note: This figure compares the baseline traits of the main Wave-1 experimental and WTP samples with the belief sample (Appendix D.1), the certainty-experiment sample (Appendix E.6), and the round-2 WTP sample (Appendix E.7). All traits are as in Table 1. 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.

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

We estimate the role of non-political similarity in participants' influence beliefs in a variant of equation 7, where we interact $Profile_{ij}$ with indicators that senders and recipients match on gender, age group, educational attainment, and state group.

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

E Wave-2 Appendix

E.1 Randomization to Wave-1 profiles and treatments

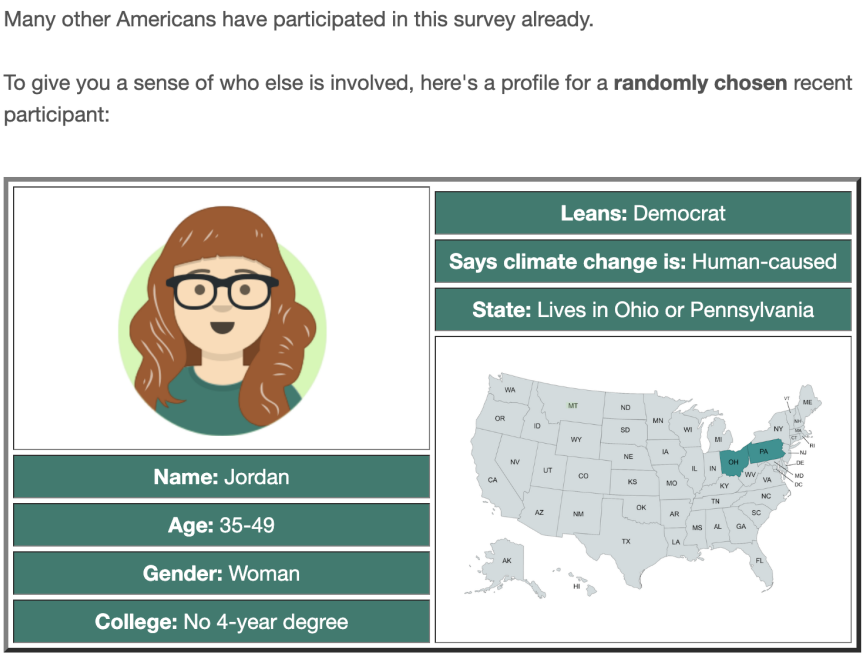
Wave-2 participants begin their survey by answering a series of questions on their demographics, political engagement, and climate beliefs, which we then combine in a basic demographic profile (Appendix Figure 1a). From participants' perspective, the next step is to show them the basic demographic profile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

Before we show Wave-2 participants the basic demographics of their randomly-paired Wave-1 match, we randomly assign each participant to a treatment status: whether they ultimately see an invitation to join an earlier participant in emailing Congress or not. We perform this randomization at this point because participants' treatment status determines the earlier Wave-1 participant to which they can be randomly paired. In particular, we must truthfully carry out what we told each Wave-1 participant about whether and to whom their demographic profile, and in some cases extra climate-action profiles, will be shown. Thus, any Wave-2 participant assigned to see a profile inviting them to join in emailing Congress must be paired with a Wave-1 participant from the A2 (Invitation) group who chose to email Congress.[47] (See Section 4 for details on the Wave-1 treatment groups.) Wave-2 participants assigned not to see an invitation to join in action can be paired with any other Wave-1 participants.

Wherever possible, we randomize Wave-2 participants to be matched with a Wave-1 participant from their paired demographic group (Section 2.2) for whom an action invitation can or cannot be shown, depending on the Wave-2 participant's treatment status. In some cases, Wave-2 participants' matched Wave-1 demographic groups are small enough that there are no Wave-1 participants to whom they can be paired; recall that these demographic groups are defined by the interactions of gender, 15-year age bins, whether participants have a 4-year college degree, and 13 groups of geographically close and politically similar

[47] Note that we cannot show a profile of an A0 participant who emailed Congress because these participants are invited to complete a separate sub-experiment, described in Section 5, in which we tell participants that we will show profiles saying that they emailed Congress only to specific returning participants.

Figure E1: Showing all Wave-2 participants a Wave-1 participant's demographics



states. In these cases, we randomly pair Wave-2 participants with an earlier participant from a small sample of Democrats recruited alongside Wave 1 that we refer to as "Wave-1 blanks." We told all of these participants that we would show future participants invitations to join them in emailing Congress, if they did so, without specifying the demographics of these future participants. (We do not include these Wave-1 blanks in our Wave-1 experimental analysis.) 49% of Wave-2 participants are paired with a Wave-1 participant from their matched demographic group, while the remaining participants are paired with a Wave-1 blank.

E.2 Wave-2 recruitment, attrition, and comprehension

E.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 E3). 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 E2: Treatment: Action invitation from Wave-1 match

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

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

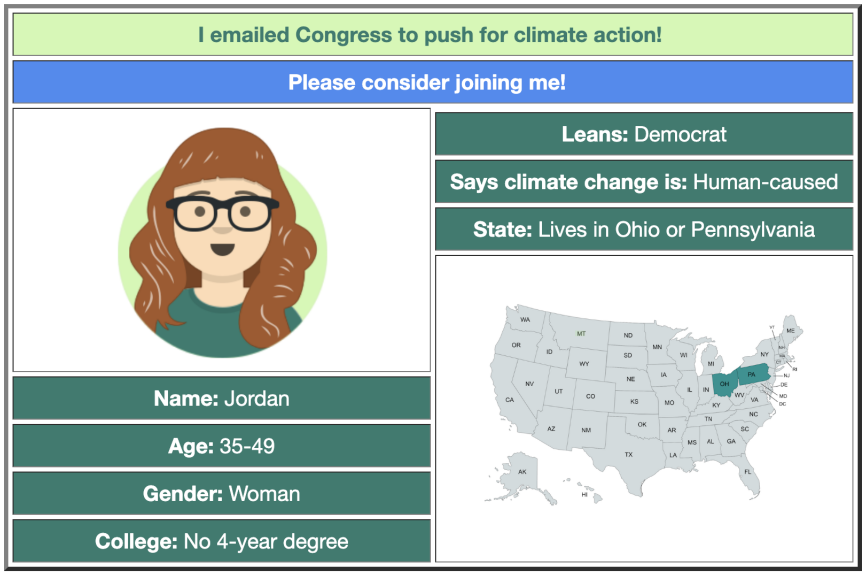
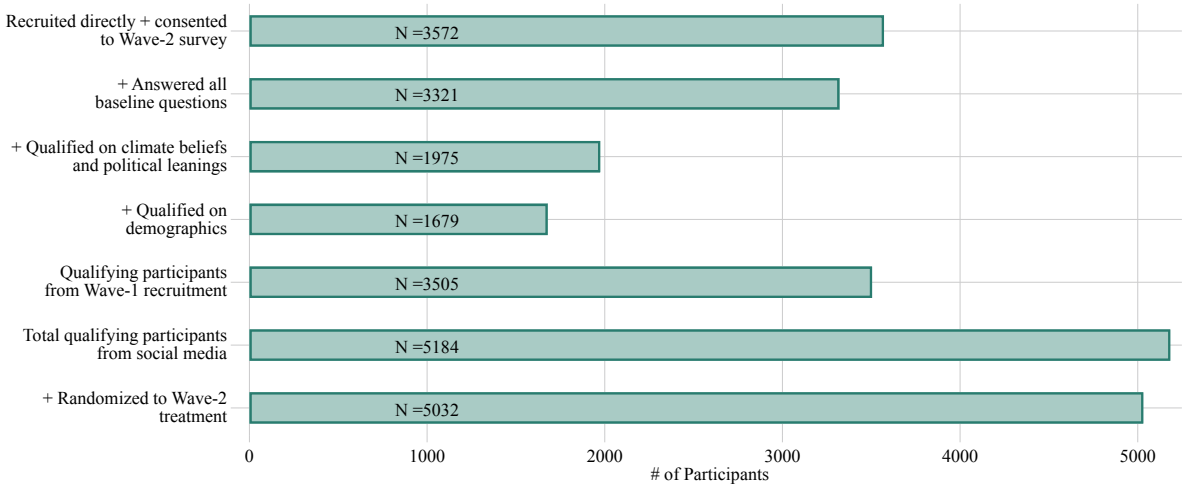
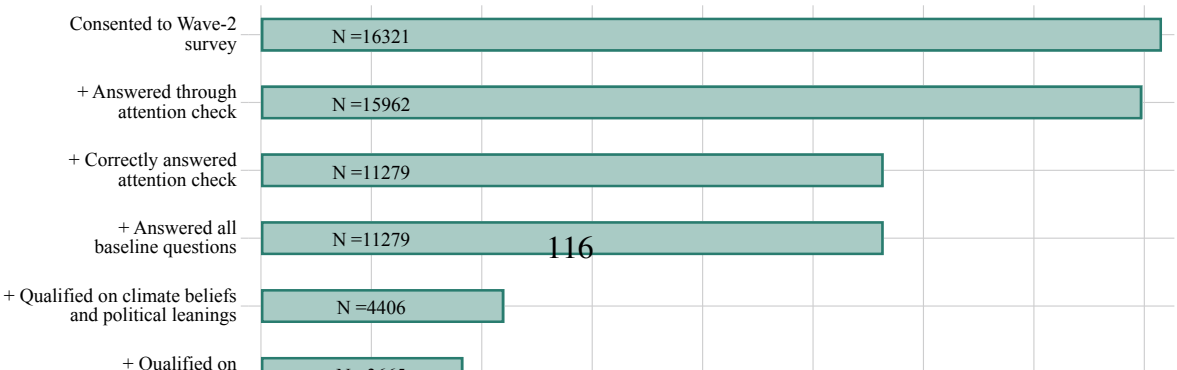


Figure E3: Wave-2 recruitment

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 2.2), restricting to participants who live within the contiguous United States, are between age 20 and age 79, identify as a man or a woman, and identify as White. These restrictions leave 1,679 qualifying participants recruited directly to the Wave-2 survey, or 85% of those who qualify for Wave-2 on politics and climate beliefs and 51% of those who completed the baseline screening questions.

The full sample of N = 5,184 qualifying participants recruited from social media (either by way of the Wave-1 experiment or directly to Wave 2) then build an avatar, and the remaining 5,032 participants recruited from social media are randomized to a Wave-2 treatment group. This sample is highly skewed towards liberals: only 211 participants recruited via social media and randomized to a Wave-2 treatment are conservative

Qualtrics recruitment: While we recruited most liberals in the Wave-2 sample from social media, we recruited nearly all conservatives in the Wave-2 sample directly from Qualtrics, which aggregates respondents from over 20 partnering market-research panels and other online samples. Participants recruited via Qualtrics were subject to identical screening criteria on demographics, political affiliation, and climate beliefs as Wave-2 participants recruited from social media (Panel B of Appendix Figure E3).

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

E.2.2 Wave-2 attrition

Appendix Table E1 tests for differential attrition by treatment status in the Wave-2 sample. There is no differential attrition in whether we observe if participants' choose to email Congress or not (column 1). Columns 1 through 7 then test for differential attrition in our secondary outcomes: participants' reports for

how strongly the researchers wanted them to email Congress (column 2), and their perceptions of others' actions and social norms around climate advocacy (columns 3 through 7). There is no differential attrition in any of these measures, which we use to test the mechanisms by which Wave-1 invitations affect action (Appendix E.3). Note that we randomize participants to answer one of two sets of secondary-outcome questions in the Wave-2 survey, so a control mean of 50% in columns 3 through 7 would denote full completion.

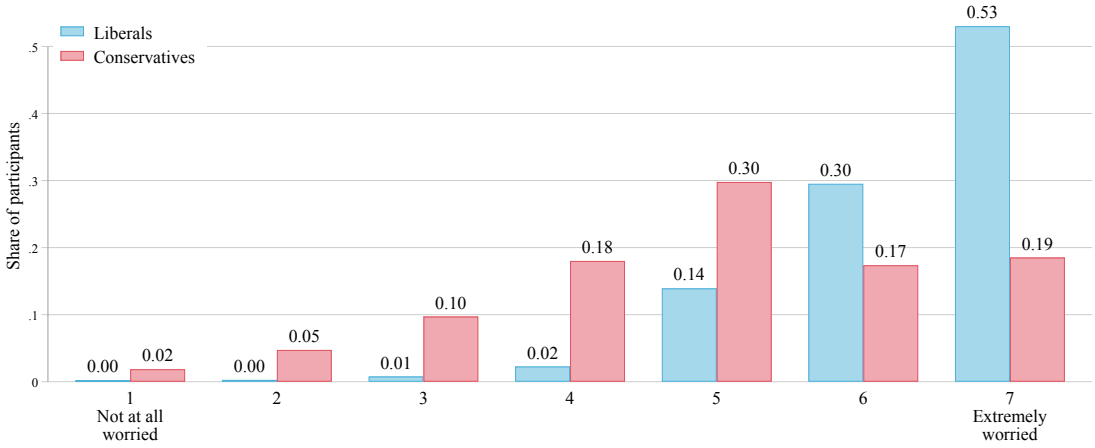
Table E1: Wave-2 attrition

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Secondary set 1			*Secondary set 2*	
	Observe if email	Demand effect	Others' action	How worth.	Therm. questions	Policy beliefs	Social norms
Treatment	0.005	0.003	-0.001	-0.001	-0.002	0.004	0.004
	(0.005)	(0.006)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Control means	0.915	0.896	0.446	0.445	0.441	0.437	0.437
Sample size	8685	8685	8685	8685	8685	8685	8685

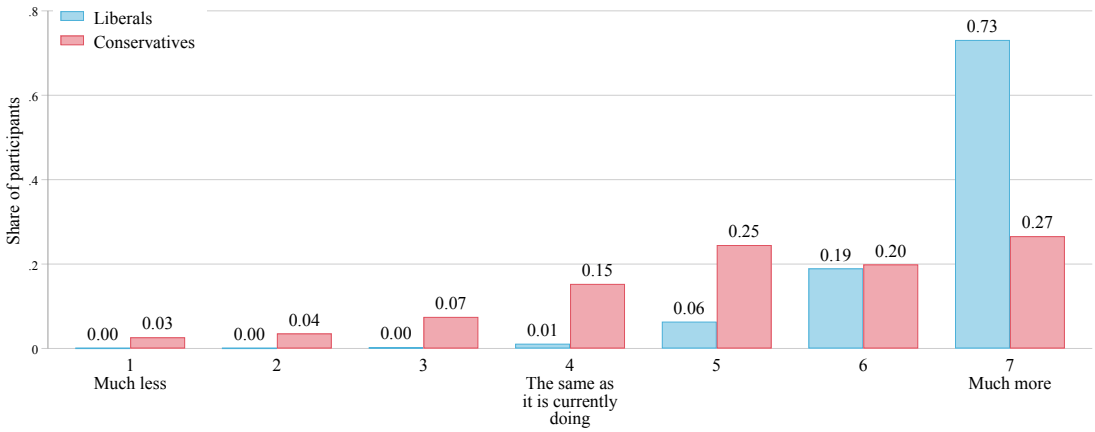
Note: This table tests for differential attrition by treatment status in Wave 2. Column 1 tests for differential attrition in whether we observe if participants choose to email Congress, our primary outcome variable. Column 2 tests for differential attrition in participants' reports of how strongly we, the researchers, wanted them to email Congress. Columns 3 through 5 test for differential attrition in whether we observe participants' incentivized guesses for the share of other participants who will email Congress, beliefs on how worthwhile it is to email Congress, and their thermometer ratings of how warmly they feel towards the Democratic and Republican parties, respectively. Columns 6 and 7 test for differential attrition in whether we observe participants' beliefs for the probability that a climate bill would pass Congress and their perception of their peers' and other Americans' beliefs about climate action, respectively. We randomly assign participants either to the first or second set of secondary questions, so full completion on columns 3 through 7 would correspond to a control mean of 50%. We present heteroskedasticity-robust standard errors in parentheses and indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively. The last row of the table presents p-values for heteroskedasticity-robust tests of the equality between the treatment effects on liberals and conservatives.

Figure E4: Baseline climate beliefs: Wave 2, split between conservatives and liberals

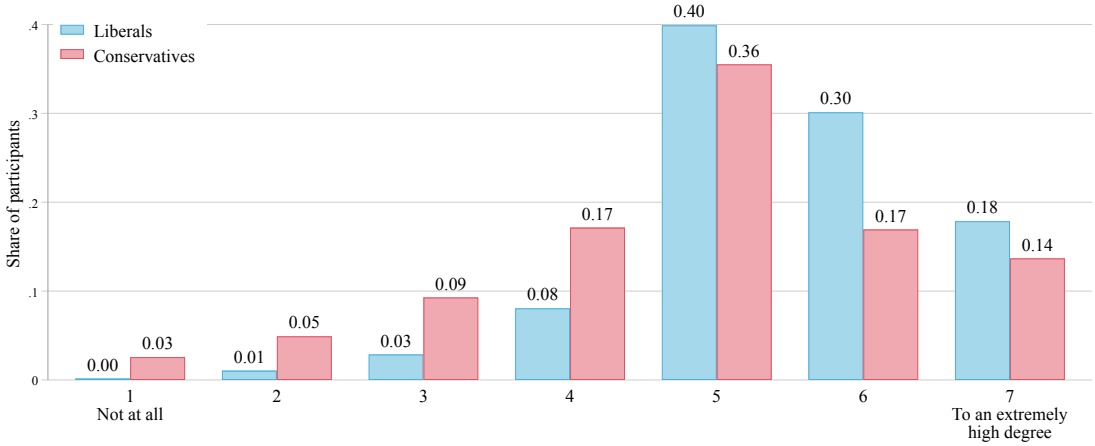
Panel A. How worried are you about climate change?



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



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



Note: These figures plot the distributions of baseline climate beliefs across liberals (N = 5,027) and conservatives (N = 2,954) in the Wave-2 experimental sample. These beliefs were collected before treatment or the opportunity to email Congress.

Table E2: Comparing Wave-2 participants recruited via social media vs. Qualtrics Panels

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

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 4. Columns 3 and 6 give p-values for tests of equality in means between columns 1 and 2 and columns 4 and 5, respectively.

E.2.3 Wave-2 comprehension questions

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

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

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

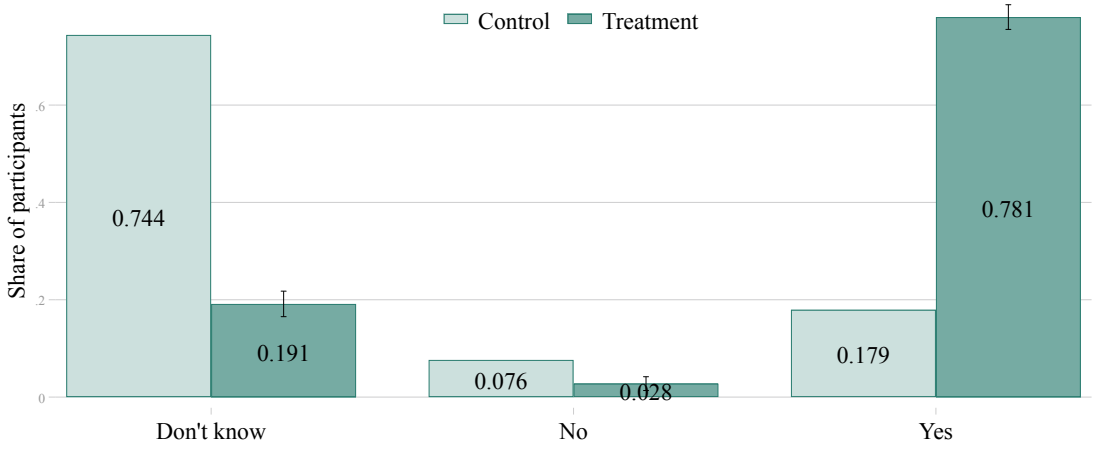
1. "Earlier in the survey, you might remember that we showed you a profile for someone who took a similar survey earlier. Do you know whether that past participant emailed Congress while taking our survey or not?" [Yes, they did; No, they didn't; I don't know what they did]

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

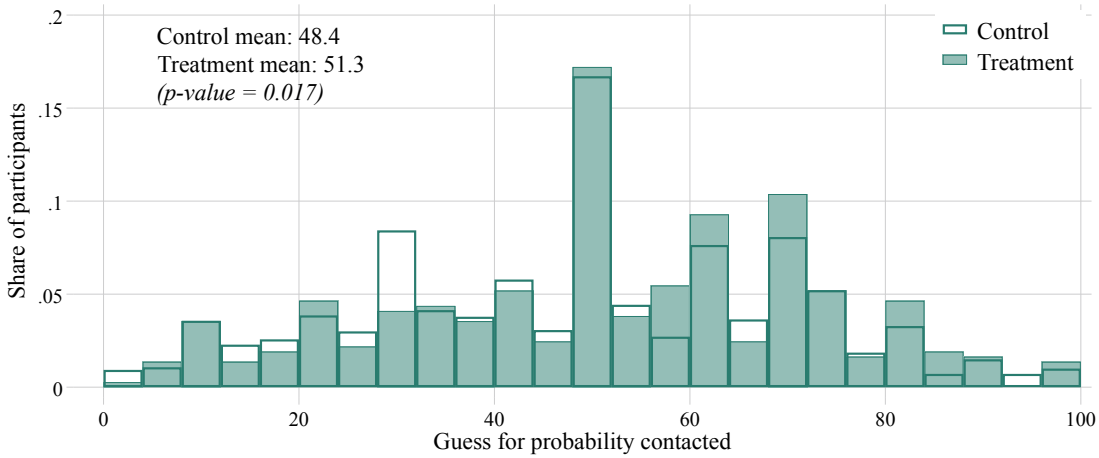
3. We then ask all participants: "Will any future participants be told whether or not you contacted Congress?" [Yes; No; I don't know]

Figure E5: Comprehension among Wave-2 participants

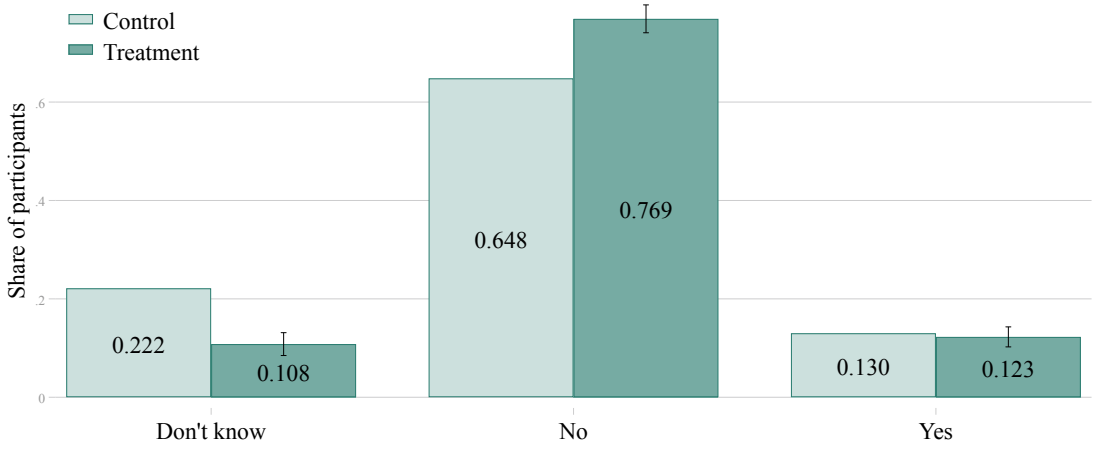
Panel A. Do you know whether your paired earlier participant emailed Congress while taking our survey or not?



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



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



E.3 Why do invitations affect action?

To understand the mechanisms by which Wave-1 invitations increase action, we test the impacts of treatment on a range of secondary outcomes that we collected after participants choose whether to email Congress or not; to reduce the survey length, we randomize participants between two sets of secondary outcomes. Recall that there is no differential attrition by treatment on these outcomes (Appendix Table E1).

E.3.1 Beliefs about the impact of emailing Congress

First, seeing that an earlier participant emailed Congress and passed on an invitation to join may shift Wave-2 participants' action by sending a signal that emails to Congress are worthwhile (Table E3, column 1). We ask participants to report on a scale from 1 (A total waste of time) to 7 (Extremely impactful) how impactful it would be for them to email their Senators, assuming that 30 other survey-takers from their state were also doing so; conditioning these beliefs on a fixed amount of participation by others ensures that effects here do not arise from treatment effects on participants' beliefs about others' political participation coupled with beliefs about strategic complementarity or substitutability. Seeing that an earlier participant emailed Congress and invited them to join increases participants' perceptions of emails' impact by 0.08sd. While our point estimate for this impact is larger among liberals, we cannot reject that the effects on liberals and conservatives are equal (columns 1 and 2, Appendix Table E5).

Table E3: Impacts of invitations on beliefs and perceived social norms

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

Note: This table reports impacts of seeing an invitation from a Wave-1 participant on Wave-2 participants beliefs about worthwhile it is to email Congress (column 1), perceived descriptive norms (columns 2 through 5), and perceived injunctive norms (columns 6 and 7). In particular, the outcomes for perceiveddescriptive norms are participants' beliefs about the share of other study participants who emailed Congress (column 2), the share of Americans who think government should address climate change (column 3) and would contact Congress to support a climate bill if it were introduced in the next few months (column 4), and the probability that such a climate bill would pass Congress (column 5). The outcomes for perceived inunctive norms are the number out of 20 friends and out of 100 Americans who would agree that advocating for climate policy is the right thing to do. The sample and control variables match those in Table 5. We present heteroskedasticity-robust standard errors in parentheses and indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.

Table E4: Impacts of invitations on perceived injunctive norms and the value of emails: Split by party

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

Note: This table estimates the regressions in columns 1, 6, and 7 of Table E3 separately among liberal and conservative Wave-2 participants. We present heteroskedasticity-robust standard errors in parentheses and indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively. The last row of the table presents p-values for heteroskedasticity-robust tests of equality between the treatment effects on liberals and conservatives.

Table E5: Impacts of invitations on perceived descriptive norms: Split by party

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

Note: This table estimates the regressions in columns 2 through 5 of Table E3 separately among liberal and conservative Wave-2 participants. We present heteroskedasticity-robust standard errors in parentheses and indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively. The last row of the table presents p-values for heteroskedasticity-robust tests of equality between the treatment effects on liberals and conservatives.

E.3.2 Perceptions of others' action and social norms

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

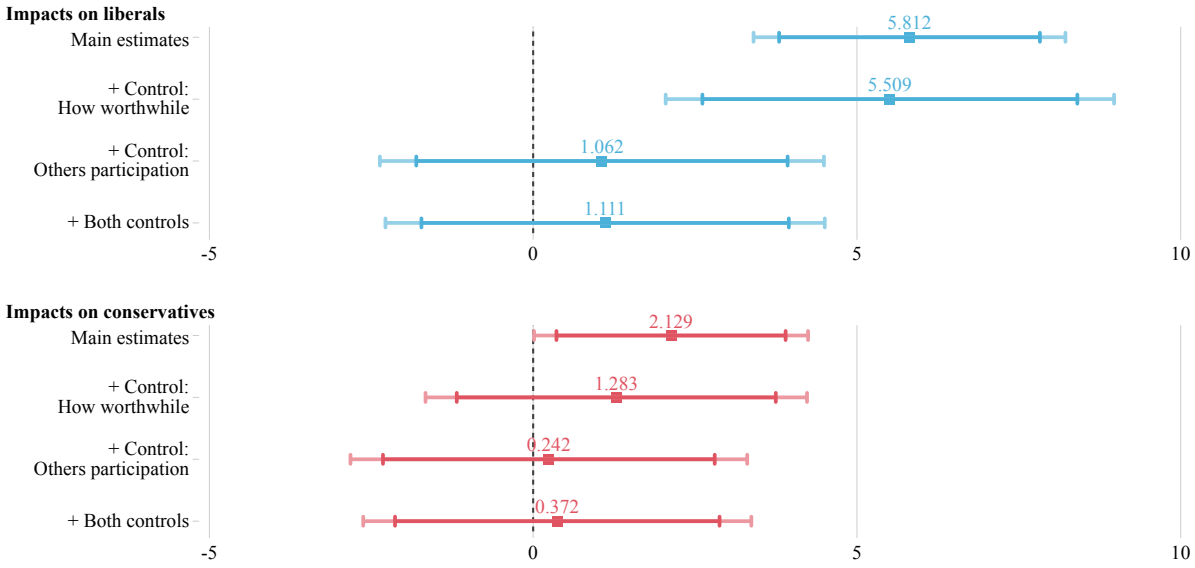
Columns 2 through 5 of Table E3 present the impacts of seeing a Wave-1 invitation on Wave-2 participants' perceived descriptive norms of political climate advocacy. Seeing an invitation increases participants' estimates for the share of other survey participants who emailed Congress via our form by 7.5 pp, or about 21% of the control mean; liberals and conservatives show similar patterns of updating (columns 1 and 2, Appendix Table E5) On the other hand, participants do not seem to extrapolate this signal to beliefs about Americans' political activity at large: seeing an invitation has no effect on participants' beliefs about the share of Americans who would say they think the US government should address climate change (column 3), the share of Americans who would contact Congress to support a climate bill if it were introduced in September 2023 (column 4), or the probability that such a climate bill would pass (column 5). We also find no impact on the injunctive norms about the ethics of climate advocacy that participants perceive among their friends or Americans at large (columns 6 and 7). There are also no effects on perceptions of these broader norms when we estimate them separately among liberals and conservatives (columns 2-6 of Appendix Table E5; Appendix Table E4).

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

E.3.3 Combining mechanisms

Appendix Figure E6 provides suggestive evidence on the contribution of each of these possible mechanisms to the Wave-2 treatment effects among liberals and conservatives. We estimate the treatment effects of invitations when we control for participants' beliefs about how worthwhile it is to email Congress, their beliefs about how many others are also emailing Congress during our survey, and both of these intermediate outcomes. While controlling for participants' beliefs about the value of emailing Congress reduces the impact of invitations somewhat among conservatives, it has very little impact on our estimated treatment effect among liberals. In contrast, controlling for participants' beliefs about others' participation substantially reduces the treatment effects we estimate both among liberals and conservatives. While this correlational decomposition is only suggestive, it is consistent with invitations primarily working by changing participants' perceived norms of political action during the survey.

Figure E6: Correlational role of Wave-2 mechanisms



Note: This figure plots the coefficient on Wave-2 treatment assignment in our main specifications (as in Table 5) and when we add controls for participants' standardized beliefs about how worthwhile it is to email Congress, for their beliefs about the share of other participants who emailed Congress, and for both of these possible mediators. The top panel estimates these regressions among Wave-2 liberals (N = 5,027), while the bottom panel estimates them among Wave-2 conservatives (N = 2,954). The coefficients give treatment effects on the probability that Wave-2 participants match to an email record, with 90% and 95% confidence intervals shown in the capped ranges.

E.4 Heterogeneous effects of invitations on Wave-2 action

This section tests for heterogeneous Wave-2 treatment effects by sample recruitment source, by political and non-similarity between Wave-1 senders and Wave-2 recipients, by other recipient and sender demographic traits, and by Wave-2 recipients' affective polarization.

E.4.1 By sample recruitment source

We first test for heterogeneous treatment effects among Wave-2 liberals and conservatives recruited from social media or from Qualtrics (Appendix Table E6). Only 163 conservatives in the Wave-2 sample are recruited from social media, so we do not interpret treatment effects in that small sample. We cannot reject that Wave-1 invitations have equal treatment effects among Wave-2 liberals recruited from Qualtrics and social media (columns 2 and 5).

Table E6: 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						

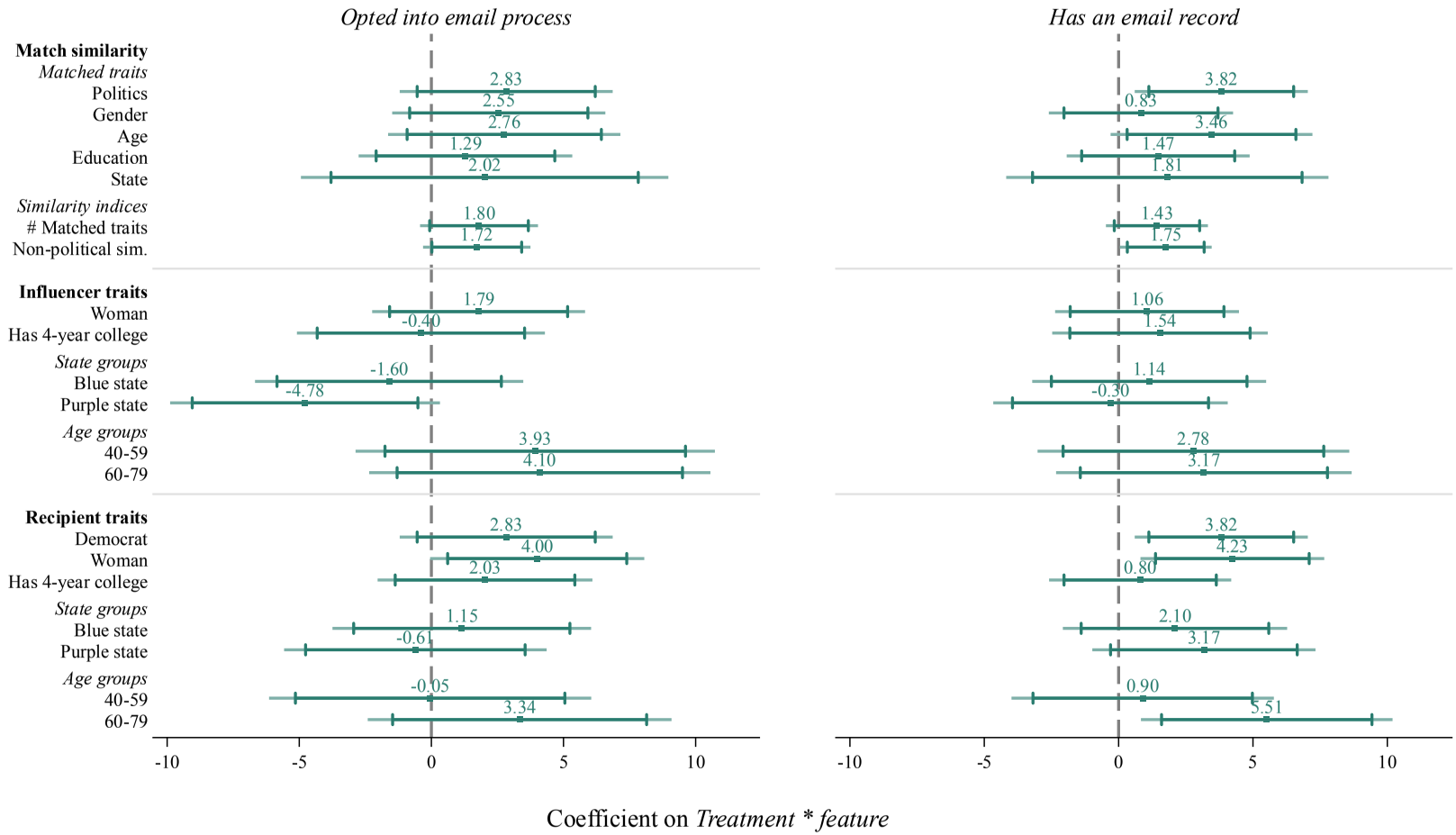
Note: This table tests for differential treatment effects of Wave-1 invitations among participants recruited from social media (columns 1-3) or from Qualtrics (columns 4-6). The regressions use the same control variables as in Table 5. We present heteroskedasticity-robust standard errors in parentheses and indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively. The last rows in Panels A and B give p-values for tests of equality in treatment affects among those recruited from each source, separately for liberals and conservatives.

E.4.2 By match similarity

E.4.3 By other recipient and sender traits

The second and third panels of Appendix Figure E7 test for heterogeneous impacts of invitations by Wave-1 sender traits and Wave-2 recipients traits, respectively. We find suggestive evidence that invitations from Wave-1 participants in red states, rather than blue or purple, may have larger effects overall, consistent with the idea that invitations from non-stereotypical climate activists could provide stronger signals to recipients. Turning to Wave-2 recipients' traits, we find that invitations may have larger impacts on woman and on older targets.

Figure E7: Heterogeneous influence effects by match similarity, inviter traits, and recipient traits



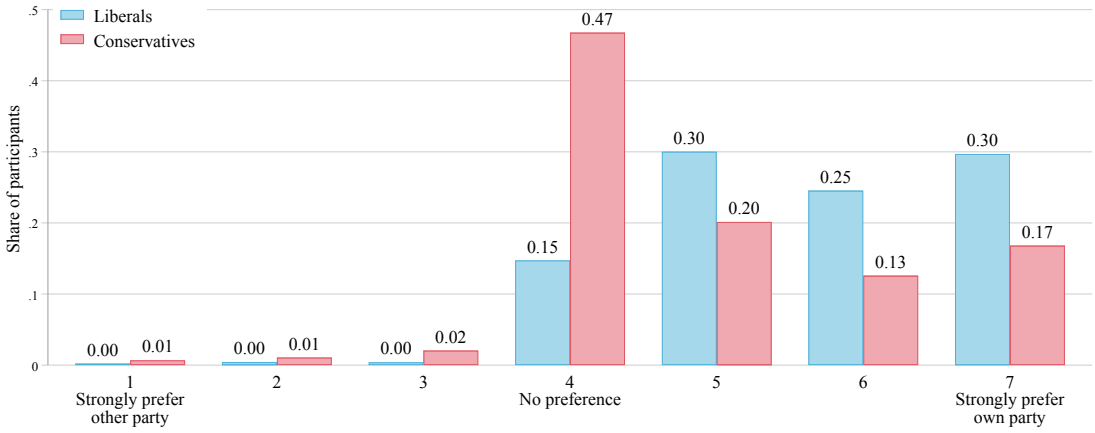
Note: This figure plots the differential impacts of Wave-1 invitations on whether Wave-2 participants match to email records when senders and recipients match on a range of demographic traits, when Wave-1 senders fall in certain demographic categories, or when Wave-2 recipients fall in certain demographic categories. We estimate this heterogeneity in versions of equation 8 where we include indicators for each of these traits, as well as interactions of each trait with $Treatment_i$. This figure plots the estimated coefficients on these interaction terms when we estimate them separately in a series of regressions, but our results change very little when we simultaneously test for heterogeity across the traits in each category (i.e. match similarity, inviter traits, or recipient traits). These results are available upon request. These regressions use the same sample restrictions and control variables as in Table 5. The darker and lighter capped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively.

E.4.4 By affective polarization

Finally, we test whether the Wave-2 treatment effects differ by recipients' affective polarization, separately among Wave-2 liberals and Wave-2 conservatives. We find suggestive evidence that more polarized conservatives respond less to invitations from Wave-1 Democrats. We define several measures of affective polarization.

Preferences for friends of own party: For our simplest measure of affective polarization, we ask all participants to report in a baseline question how strongly they would prefer being friends with someone from one of the major political parties versus the other. We elicit responses on a 7-point scale, where 4 denotes "No preference" and the endpoints are randomized between "Strongly prefer Republican" and "Strongly prefer Democrat." To obscure our focus on partisan polarization, we ask this question in a series where we also ask participants to rate their preference to be friends with someone who is either very religious or not and who has varying degrees of education. Figure E8 plots the distribution of these preferences among liberal and conservative Wave-2 participants. Liberals in our sample show substantially higher affective polarization than conservatives on this measure. One key limitation of this measure is that conservatives' responses are highly lumped at the middle of the scale; only 111 conservatives offer a below-median response, limiting our ability to test for heterogeneous treatment effects by this measure. To isolate more granular variation in these preferences separate from other participants' other traits, we also estimate the residuals of this measure when regressed on demographics, climate beliefs, and political engagement, separately by political party.

Figure E8: How much would you prefer being friends with someone from one of the major political parties versus the other?



Note: This figure plots the distribution of liberal and conservative Wave-2 participants' responses to how much they would prefer being friends with someone from one of the major political parties versus the other. We phrase the question as asking whether participants would rather prefer to be friends with someone from the Democratic or Republican parties, rather than one's one party versus the other, randomizing which party is given at 1 versus at 7 on the 7-point scale.

Polarization feelings thermometer: Towards the end of the Wave-2 experimental survey, we measure af-

Table E7: Impacts on Wave-2 thermometer values

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

Note: This table estimates the impacts of the Wave-2 invitation treatment on Wave-2 participants' stated warmth towards the Democratic and Republican parties on the "feelings thermometer" used in the American National Election Study (Iyengar et al., 2019). Participants report how warmly they feel toward the Democratic party (columns 1 and 2) and the Republican party (columns 3 and 4), and we then calculate affective polarization as the gap in warmth of feeling towards their own party versus the opposing party. We estimate regressions separately by whether Wave-2 participants are liberal (columns 1, 3, and 5) or conservative (columns 2, 4, and 6). Our sample size is smaller than in the main Wave-2 regressions because we randomize only half of Wave-2 participants to answer the feelings-thermometer questions. These regression specifications are identical to those presented in Table 5. We present heteroskedasticity-robust standard errors in parentheses and indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively. The last row of the table presents p-values for heteroskedasticity-robust tests of the equality between the treatment effects on liberals and conservatives.

fective polarization using the "feelings thermometer" measured over time in the American National Election Study (Iyengar et al., 2019). In this classic measure of affective polarization, participants rate how warmly they feel towards each political party on a scale from 0 to 100 degrees; we then calculate affective polarization as the gap in warmth they feel towards their own versus the out party. We elicit this measure at the end of the survey, rather than before our experimental treatments, in order to minimize the salience of partisanship. While doing so opens the risk that the treatments might actually affect participants' responses to this measure, we find no evidence of treatment effects on these measures (Appendix Table E7).

We only observe this thermometer-based polarization measure for a random half of the Wave-2 survey. We thus also use OLS and lasso to predict values for this measure in the full sample. Both OLS and lasso predict our observed thermometer polarization measures reasonably well: the lasso- and OLS-predicted measures have correlation coefficients of 0.52 and 0.53 with the observed measure, respectively.

Testing for heterogeneity by affective polarization: Appendix Figure E9 plots the results of a series of regressions in which we estimate heterogeneous Wave-2 impacts among liberals and conservatives with above- versus below-median affective polarization by each measure. In particular, this figure plots the coefficients on the interaction between the Wave-2 treatment indicator and an indicator for having above-median polarization by each measure. Among Wave-2 liberals, there is no consistent heterogeneity in invitations' impacts by polarization. On the other hand, our point estimates suggest that invitations have substantially lower impacts among more polarized conservatives (columns 6-10), though we are under-powered to sta-

tistically reject homogeneous effects in most cases. These coefficients suggest that that invitations are at most half as effective among Wave-2 conservatives with above-median versus below-median affective polarization. (Results available upon request.) We only estimate a positive interaction term when we measure affective polarization using participants' simple Likert-scale reports of how much they would prefer to be friends with a member of their own party; due to heavy bunching on the scale, only 112 of 2,960 conservative Wave-2 participants report a value below the median.

Figure E9: Heterogeneous Wave-2 impacts by affective polarization

Note: This figure plots the differential impacts of Wave-1 invitations on whether Wave-2 participants opt into the email process and match to email records when Wave-2 recipients show above-median affective polarization. We estimate this heterogeneity in versions of equation 8 where we include indicators for having above-median affective polarization, as well as interactions of each trait with $Treatment_i$. We use several measures of affective polarization: self-reports of how much they would prefer being friends with someone from their own party, the residual of these self-reports when regressed on demographics and climate beliefs, thermometer-based polarization for the subsample where we observe it, OLS-predicted thermometer polarization, and Lasso-predicted thermometer polarization. These regressions use the same sample restrictions and control variables as in Table 5. N = 2,360 and N = 1,467 for regressions among Wave-2 liberals and conservatives, respectively, where we measure affective polarization with the raw feelings thermometer metric; sample sizes are N = 5,027 and N = 2,360, respectively, for regressions using the other polarization measures. The darker and lighter capped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively.

E.4.5 By similarity

Matching on politics increases invitations' impacts by 3.8pp in these regressions.[48] However, non-political similarity matters as well. While each of these interactions is imprecisely estimated, invitations increase Wave-2 participants' likelihood of matching to email records by an additional 1.5pp when senders and recipients share educational attainment, by 1.8pp when they live in the same state group, and by 3.5pp when they share an age group. Pooling across these dimensions, invitations' impact on email records rises by about 1.4pp for each shared non-political trait, though this coefficient is statistically indistinguishable from zero. Increasing a continuous measure of Wave-2 participants' perceived non-political similarity to their Wave-1 match by one standard deviation increases the invitations' impacts by a statistically-significant 1.8pp. Altogether, these results suggest that while Wave-2 participants respond more to invitations from senders who are like them in non-political ways, the impact of sharing these other traits is smaller than that of shared politics. Consistent with these patterns, shared politics is by far the largest contributor to how similar Wave-2 participants feel to their Wave-1 matches, even as matching on non-political traits matters as well (Appendix Figure **??**).

E.5 Wave-2 robustness

Sample definitions: Our main Wave-2 results are highly robust to changes in the sample definition (Appendix Figure E10). First, they are robust to expanding the Wave-2 experimental sample by assuming that

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

all participants who leave the experimental 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).[49] Next, they are robust to restricting precisely to our pre-registered sample sizes,[50] and to restricting to participants who correctly answer whether they know if their paired Wave-1 match emailed Congress when they took our survey.

Control variables: Our Wave-2 estimates are also fully robust the following control specifications (Appendix Figure E10): no controls, only demographic controls, the full set of controls for demographics, baseline beliefs, and baseline political engagement that we include in our main regressions, and a set of controls selected by double-post Lasso regressions following (Chernozhukov et al., 2018).[51]

Experimenter demand effects: Next, our Wave-2 estimates are robust to accounting for experimenter demand effects. Strong demand effects are organically entwined in the Wave-2 intervention, which is a direct invitation to join in emailing Congress. Indeed, participants assigned to see a Wave-1 invitation report 0.2sd higher demand on a 6-point Likert-style measure of how strongly they think we (the researchers) wanted them to email Congress during the survey. 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 7% of the Wave-2 experimental sample guesses that our research question relates to peer effects or the invitations' impacts, comprising about 11% of the treatment group and 4% of the control group. Our results are fully robust to excluding these participants (Appendix Figure E10). Next, our results are robust to assuming that recognizing the study purpose either increases or decreases our main outcomes by 0.2sd (Appendix Figure E11), 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 6.2.2). Our goal here is not to identify the differential impact of invitations on liberals or conservatives who are other-

[49] As in the Wave-1 sample, the assumption that those who leave the experimental survey after the email preview would not have emailed Congress appears to be reasonable in Wave 2 (Appendix E.5.1)

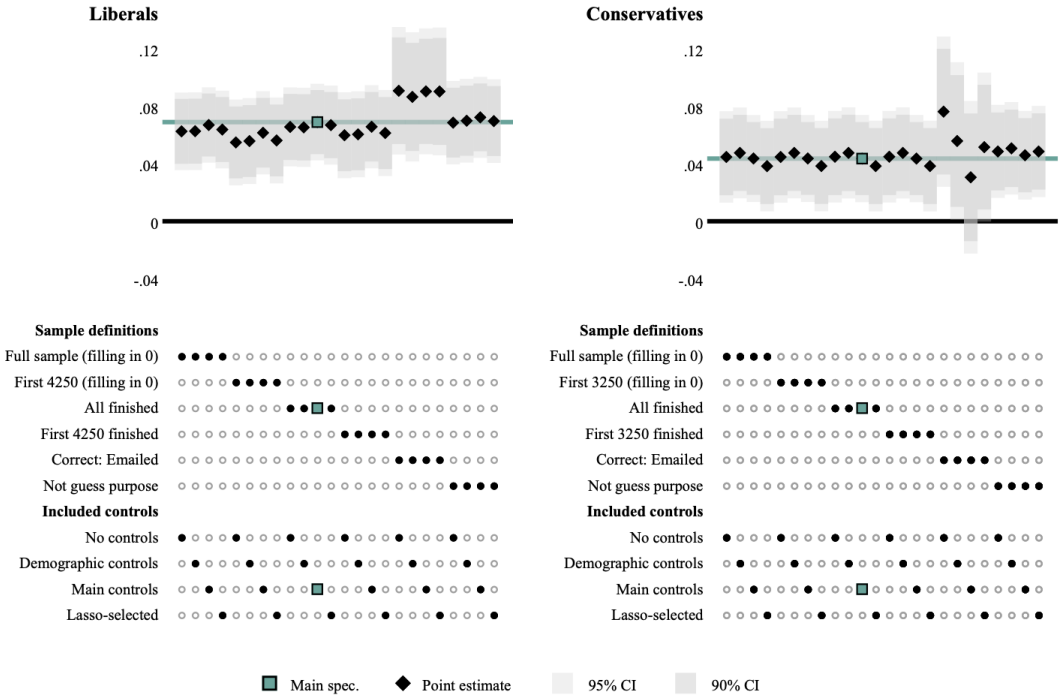
[50] While we pre-registered a total Wave-2 sample size of about 4,250 liberals, our recruitment on social media yielded a total of 5,200 liberals who saw the email preview, and thus for whom we can construct a filled-in email outcome, and 5,027 for whom we observe explicit choices to email Congress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

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

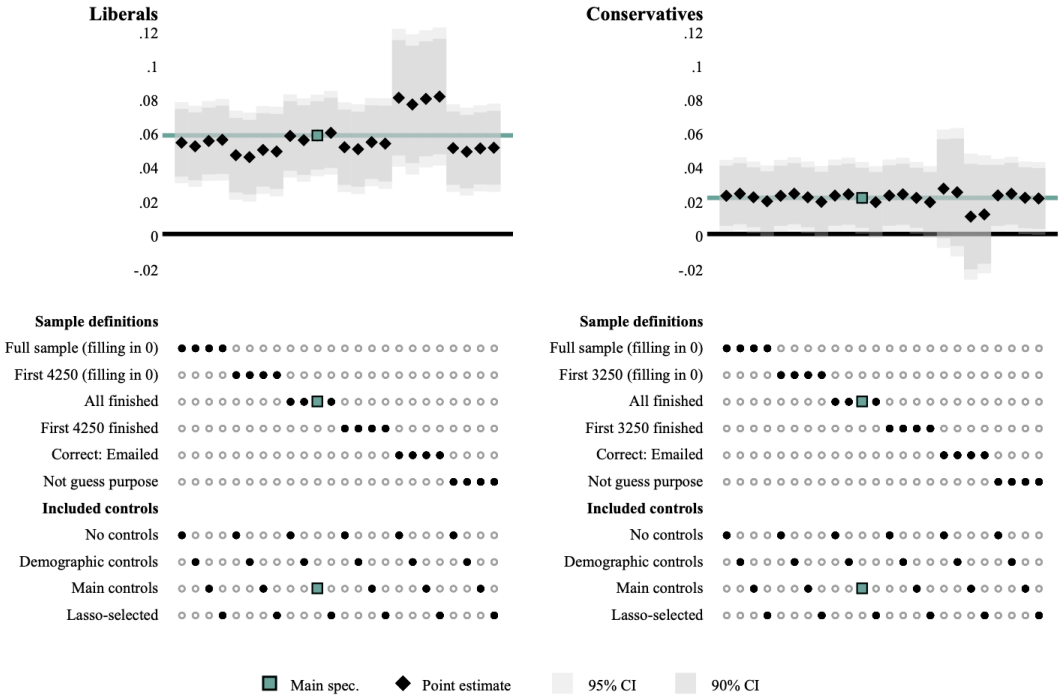
wise identical; indeed, gaps in concern about climate change and education would be at play in real-world attempts to mobilize liberals versus conservatives for climate action. Rather, Appendix Figure E12 shows that the partisan gap we estimate is largely robust to controlling for the interaction of treatment status with key traits associated with our differential recruitment strategies for liberals and conservatives: whether a participant is recruited from Qualtrics, political engagement, and income.

Figure E10: Wave-2 specification charts

Panel A. Whether participants start the email process



Panel B. Whether participants match to an email record



E.5.1 Robustness to the Wave-1 attrition assumption

One of the Wave-2 robustness checks that we conduct in Section E.5 is to show that our main results hold when we assume that those who leave the experimental survey after the email preview but before choosing whether to email Congress or not would not have done so. (We impose this assumption in our main specifications to deal with differential attrition in Wave 1.) As in Wave 1, this assumption appears to be justified in Wave 2. The small sample of participants in the main Wave-2 sample for whom we do not observe explicit email choices are more likely to be under 40, less likely to be over 70, express less concern about climate change, and are less politically engaged, though some of these gaps are not statistically significant (Appendix Table E10). In the Wave-2 control group, many of these patterns correlate with lower likelihood of explicitly choosing not to email Congress (column 2 of Appendix Table B2).

Table E8: Wave-2 attrition detail

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Attrition through observing emails						
					Social-media sample	
	Observe	Saw letter preview			Answered	Correct
	if email	All	Qual.	Social	attent. check	attent. check
Show profile	0.005	0.009**	0.000	0.016**	-0.001	0.017**
	(0.005)	(0.004)	(0.000)	(0.007)	(0.002)	(0.007)
Control means	0.915	0.933	1.000	0.885	0.955	0.927
Sample size	8685	8685	3653	5032	5032	4816
Panel B: Attrition in secondary outcomes						
		Have non-missing answer for:				
		Secondary set 1			*Secondary set 2*	
	Demand	Others'	How	Therm.	Policy	Social
	effect	action	worth.	questions	beliefs	norms
Show profile	0.003	-0.001	-0.001	-0.002	0.004	0.004
	(0.006)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Control means	0.896	0.446	0.445	0.441	0.437	0.437
Sample size	8685	8685	8685	8685	8685	8685

However, we do not make this assumption in our main Wave-2 results because it actually induces differential attrition by treatment arm. In Wave 1, all participants randomized to a treatment group had already seen the email-form preview, so this assumption allows us to retain all randomized participants in the main

Wave-1 experimental sample. In contrast, our Wave-2 approach opens up the possibility of differential selection into the main Wave-2 experimental sample. This attrition could arise simply from participants leaving the survey between randomization and the email preview, but it also arises from an attention check question that was asked of participants recruited via social media after randomization and just before the email preview. In the social-media sample, treatment participants are 1.9 pp more likely to answer this attention check correctly (column 6, Panel A, Appendix Table E1), despite the fact the two treatment groups' surveys have not diverged by this point. Overall, treatment participants are 1.4 pp more likely to see the email preview (column 3, Panel A, Appendix Table E1), driven entirely by participants recruited from social media (column 4, Panel A, Appendix Table E1).

However, we find no evidence for differential selection on observable traits in attrition across treatment arms. Appendix Table E9 compares participants who see the email preview separately among the treatment and control groups. While those who attrit differ substantially from those who do not in both treatment groups, we cannot reject that the attriters are identical across treatment groups on nearly every trait. Moreover, as we note above, there are no substantive differences in the treatment versus control surveys to this point, so there is no obvious mechanism that could drive differential selection. Nevertheless, for simplicity we restrict the main Wave-2 analysis to those who remained in the survey through choosing whether to email Congress or not.

Table E9: Comparing Wave-2 participants who do and do not see email preview

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Control group:			**Treatment group:**				
	See email preview	Δ Attrit	*(p-val)*	See email preview	Δ Attrit	*(p-val)*	**Among attrited** Δ Treat	*(p-val)*
Woman	0.545	0.166***	(0.000)	0.551	0.149***	(0.000)	0.029	(0.500)
Hispanic	1.073	0.014	(0.203)	1.071	0.009	(0.489)	0.003	(0.851)
Has ≥ 4-year college degree	0.614	0.040	(0.139)	0.614	0.042	(0.162)	-0.013	(0.732)
Age ranges:								
20-39	0.291	0.056**	(0.011)	0.290	0.090***	(0.000)	-0.036	(0.199)
40-59	0.331	0.081***	(0.004)	0.316	0.006	(0.851)	0.057	(0.155)
60-79	0.378	-0.137***	(0.000)	0.394	-0.096***	(0.005)	-0.021	(0.625)
Income bins (USD):								
Less than 50,000	0.289	-0.031	(0.233)	0.295	0.005	(0.858)	-0.029	(0.421)
50,000-99,999	0.349	0.059**	(0.035)	0.336	0.002	(0.950)	0.057	(0.155)
100,000-149,999	0.187	-0.021	(0.437)	0.193	0.020	(0.475)	-0.039	(0.292)
150,000-199,999	0.085	-0.005	(0.803)	0.092	0.002	(0.924)	-0.013	(0.643)
200,000 or more	0.089	-0.002	(0.924)	0.084	-0.029	(0.246)	0.024	(0.439)
Residence by state marginality:								
Red state	0.323	-0.010	(0.721)	0.320	0.009	(0.764)	-0.020	(0.608)
Blue state	0.359	0.038	(0.205)	0.361	0.004	(0.906)	0.031	(0.471)
Purple state	0.318	-0.028	(0.351)	0.318	-0.014	(0.662)	-0.010	(0.812)
Climate beliefs:								
Climate worry (1-7)	5.819	0.154**	(0.033)	5.810	0.245***	(0.005)	-0.124	(0.256)
Desire for climate action (1-7)	6.100	0.301***	(0.000)	6.136	0.292***	(0.000)	0.016	(0.878)
Perceived local impacts (1-7)	5.250	-0.030	(0.775)	5.247	0.250**	(0.028)	-0.240	(0.115)
Political affiliation:								
Republican	0.265	-0.030**	(0.021)	0.272	-0.013	(0.237)	-0.018	(0.290)
R-leaning Independent	0.096	-0.021*	(0.080)	0.091	-0.018	(0.166)	-0.003	(0.860)
L-leaning Independent	0.436	0.039	(0.164)	0.436	-0.001	(0.973)	0.029	(0.457)
Democrat	0.202	0.012	(0.631)	0.200	0.032	(0.236)	-0.008	(0.819)
Other political beliefs:								
Political engage. index (std)	0.037	0.774***	(0.001)	0.039	0.039	(0.883)	0.636*	(0.057)
Prev. contacted reps	0.246	0.033	(0.187)	0.254	0.058**	(0.032)	-0.018	(0.607)
Prev. donated	0.525	0.106***	(0.000)	0.526	0.029	(0.350)	0.068*	(0.098)
Prev. canvassed	0.054	0.017	(0.191)	0.057	-0.035*	(0.080)	0.057**	(0.014)
Prev. signed petition	0.619	0.103***	(0.000)	0.618	0.049	(0.102)	0.041	(0.306)
Prev. phonebanked	0.069	0.005	(0.769)	0.065	-0.033	(0.116)	0.031	(0.234)
Political efficacy index (std)	-0.006	-0.027	(0.427)	0.002	-0.061*	(0.090)	0.052	(0.269)
Prefer friend of own party (1-7)	5.392	0.211***	(0.005)	5.378	0.195**	(0.011)	-0.001	(0.992)
		138						
Sample size	4065	291		4095	234			

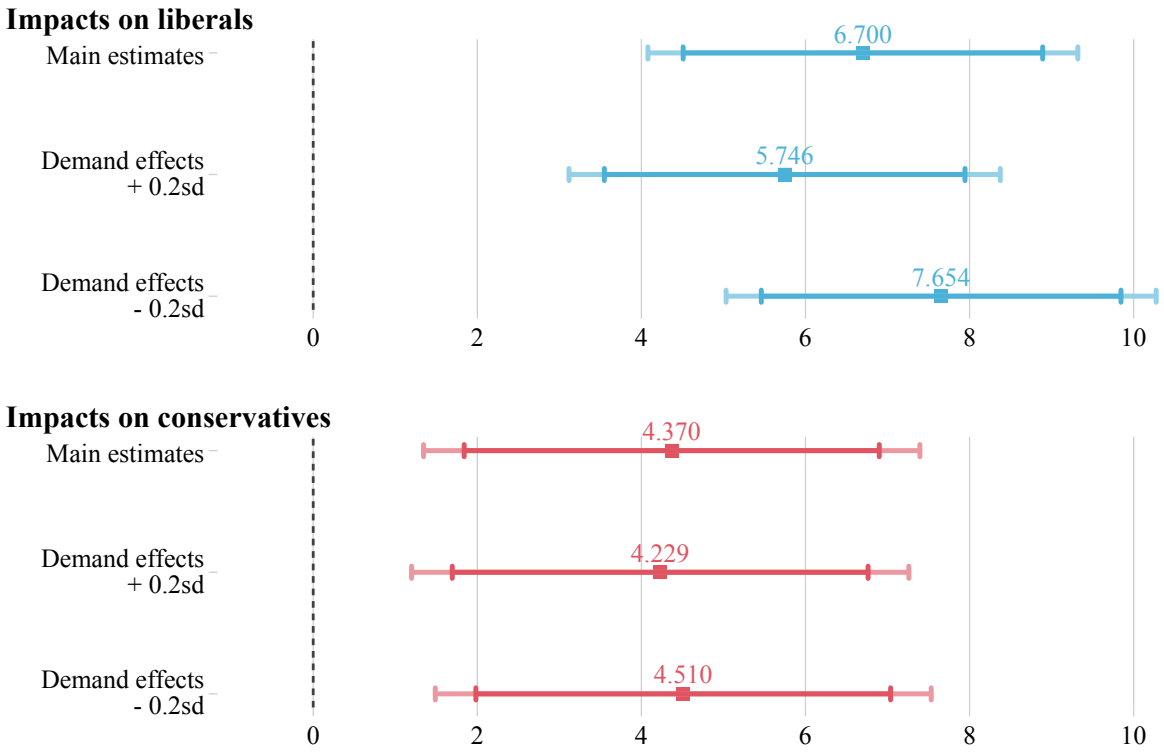
Add note here that we're missing perceptions of local climate impacts for some people.

Table E10: Comparing Wave-2 participants for whom we observe emails or not, among those who saw email preview

	(1) Mean: Observe if email	(2) Δ Not observe if email	(3) *(p-value)*
Woman	0.549	0.076**	(0.046)
Hispanic	1.072	-0.029	(0.147)
Has $\geq$ 4-year college degree	0.611	0.045	(0.173)
Age ranges:			
20-39	0.295	0.098***	(0.000)
40-59	0.323	-0.001	(0.978)
60-79	0.382	-0.097**	(0.011)
Income bins (USD):			
Less than 50,000	0.293	-0.029	(0.365)
50,000-99,999	0.344	0.044	(0.196)
100,000-149,999	0.190	0.024	(0.439)
150,000-199,999	0.087	-0.035	(0.195)
200,000 or more	0.086	-0.004	(0.878)
Residence by state marginality:			
Red state	0.321	-0.074**	(0.040)
Blue state	0.359	0.011	(0.766)
Purple state	0.320	0.063*	(0.056)
Climate beliefs:			
Climate worry (1-7)	5.802	-0.130**	(0.046)
Desire for climate action (1-7)	6.107	-0.042	(0.453)
Perceived local impacts (1-7)	5.244	-0.083	(0.293)
Political affiliation:			
Republican	0.275	0.001	(0.920)
R-leaning Independent	0.096	0.004	(0.689)
L-leaning Independent	0.429	-0.000	(1.000)
Democrat	0.201	-0.004	(0.897)
Other political beliefs:			
Political engage. index (std)	-0.000	-0.324	(0.316)
Prev. contacted reps	0.250	-0.026	(0.444)
Prev. donated	0.522	0.022	(0.530)
Prev. canvassed	0.056	-0.000	(1.000)
Prev. signed petition	0.614	-0.014	(0.641)
Prev. phonebanked	0.066	-0.048*	(0.055)
Political efficacy index (std)	-0.000	-0.012	(0.770)
Prefer friend of own party (1-7)	5.379	-0.003	(0.970)
Sample size	7981	179	

Figure E11: Robustness of Wave-2 estimates to demand effects

Panel A. Impacts on opting into the email process



Panel B. Impacts on email records

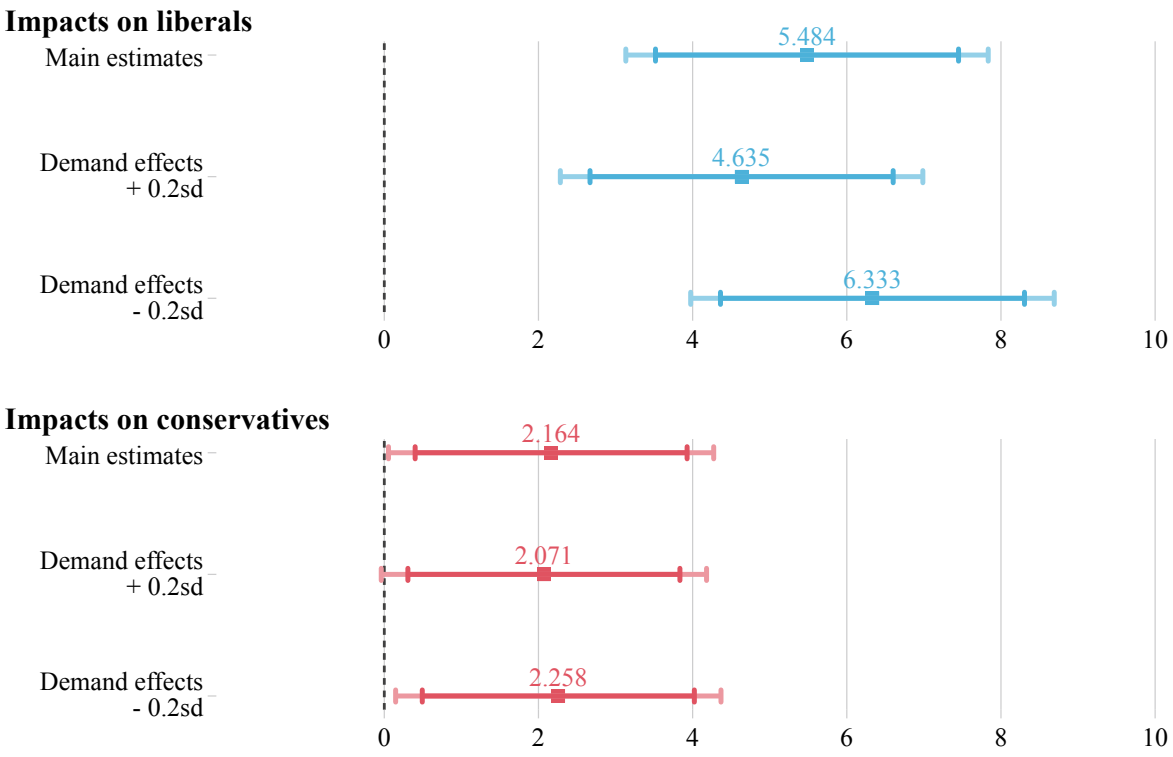
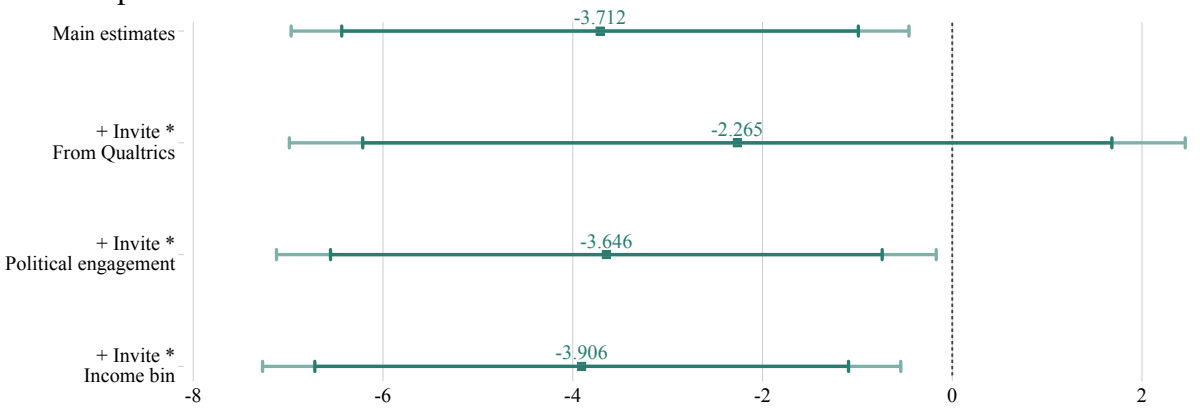


Figure E12: Robustness of Wave-2 partisan gaps

Panel A. Impacts on opting into the email process



Panel B. Impacts on email records



E.6 The certainty experiments

E.6.1 Text of the explanation to certainty participants

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

Figure E13: Explanation of the certainty experiment

Slide 1.

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

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

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

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

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

Slide 2.

First, what are **carbon offsets**?

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

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

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

Slide 3. (Only in Round 2)

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

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

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

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

(Show email preview, Figure B3c)

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.

E.6.2 Other differences between rounds 1 and 2

In addition to slightly revising our description of the choice set-up in the second round, we made several changes to the experimental set-up. First, participants in the second round of the

certainty experiment made choices over 14 demographic groups, while those in the first round only made 10 choices. Second, all participants in the second round made choices between letters and $10 in carbon offsets, while those in the first round were randomized to $8, $10, $12, $14, or $16 in carbon offsets.

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

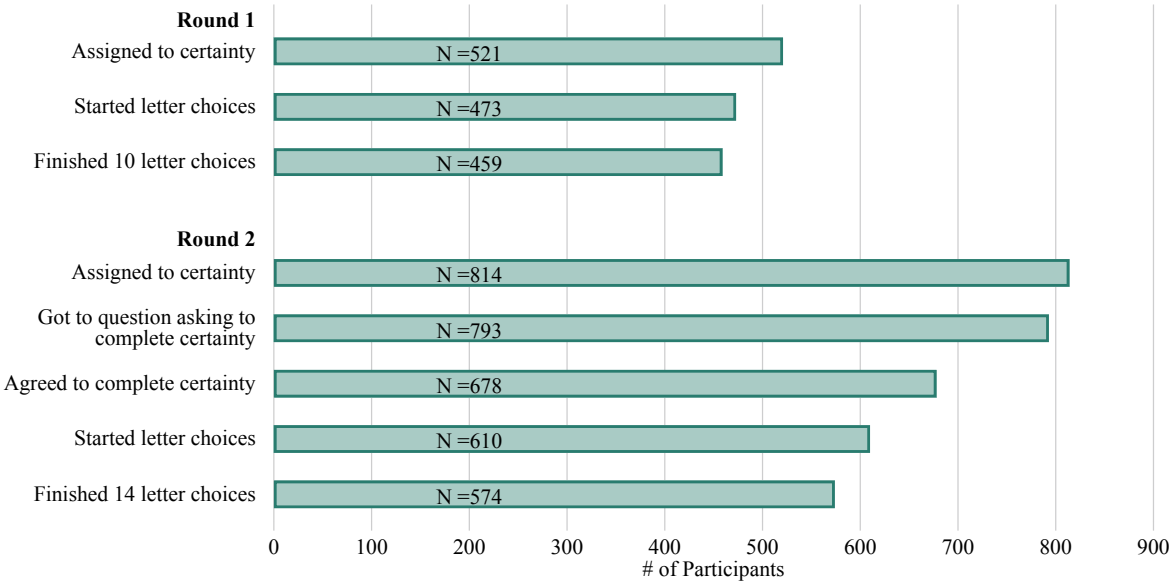
In the second round, in contrast, we fully randomize political party with respect to other demographic-group traits. Among the set of past study participants who had started but were ineligible for the Wave-1 survey, and were not redirected to the Wave-2 survey, we identify groups by gender, age group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

E.6.3 Certainty recruitment

We recruit participants for two rounds of the certainty experiment. We implement the first round among participants recruited alongside the Wave-1 action-experiment sample and assigned to the belief-elicitation sample described in Section 6.1 above; participants complete the certainty-

valuation choices before completing the belief-elicitation exercise. In total, 521 participants are randomized to begin the first-round certainty experiment, 473 make at least one choice, and 459 complete all 10 binary choices.

Figure E14: Recruitment to the letter-certainty experiment



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

Table E11: Demographics across certainty rounds

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

E.6.4 Certainty results

Table E12: Certainty results table

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Chose email over offsets				Impact on climate policy			
		State groups				*State groups*		
	All	Red	Blue	Purple	All	Red	Blue	Purple
Republican	-0.003	-0.002	0.004	-0.012	0.080	0.199*	-0.045	0.121
	(0.013)	(0.017)	(0.019)	(0.025)	(0.063)	(0.119)	(0.101)	(0.123)
Woman	0.004	0.010	0.003	0.003	-0.007	-0.085	0.073	-0.049
	(0.008)	(0.014)	(0.017)	(0.031)	(0.069)	(0.127)	(0.119)	(0.245)
Has 4-year college	0.011	0.001	-0.002	0.028	-0.004	0.032	-0.014	-0.150
	(0.011)	(0.019)	(0.020)	(0.040)	(0.082)	(0.162)	(0.133)	(0.201)
Age ranges:								
35-49	0.011	-0.014	0.001	-0.054	-0.080	-0.280	0.021	0.049
	(0.013)	(0.019)	(0.032)	(0.042)	(0.108)	(0.175)	(0.195)	(0.237)
50-64	0.011	0.012	0.013	-0.023	-0.139	-0.163	-0.288	-0.015
	(0.012)	(0.019)	(0.032)	(0.041)	(0.105)	(0.187)	(0.181)	(0.253)
65-79	0.006	-0.034*	0.012	-0.008	-0.201**	-0.359	-0.264*	0.021
	(0.013)	(0.020)	(0.033)	(0.045)	(0.100)	(0.238)	(0.141)	(0.195)
State policy groups:								
Blue state	-0.104***				0.245***			
	(0.015)				(0.080)			
Purple state	0.046***				0.200**			
	(0.011)				(0.088)			
Sample mean	0.601	0.630	0.525	0.670	-0.025	-0.160	0.054	0.017
N	12167	4975	4279	2913	1002	326	400	276

E.7 Follow-up WTP experiment

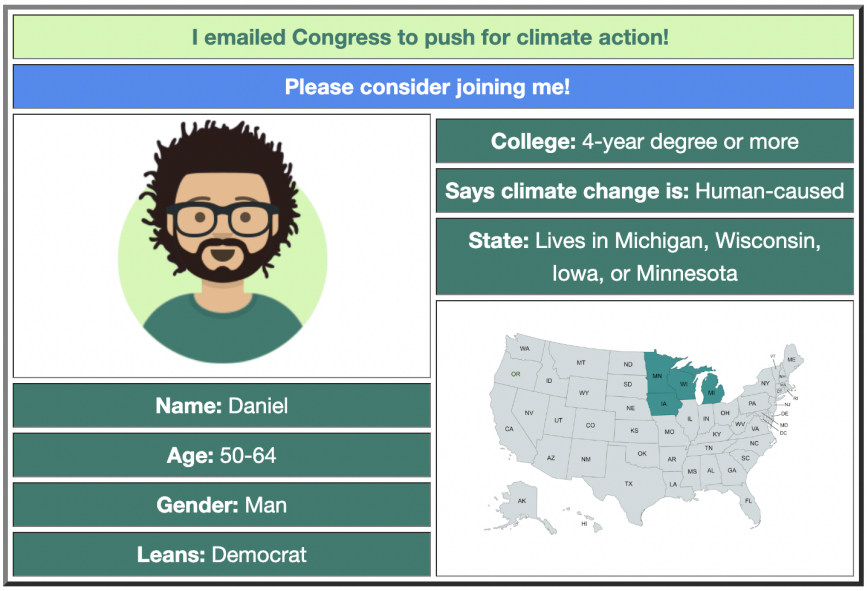
E.7.1 Explanation to participants

The vast majority of the round-two WTP experiment exactly matches the main WTP set-up. Participants wh oare randomly assigned to have the option to hide their own political leanings also see the following explanation slide, in between slides 9 and 10 of the main WTP explanation (Appendix Figure C25).

Figure E15: Explanation of the option to hide your WTP participants' leanings

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

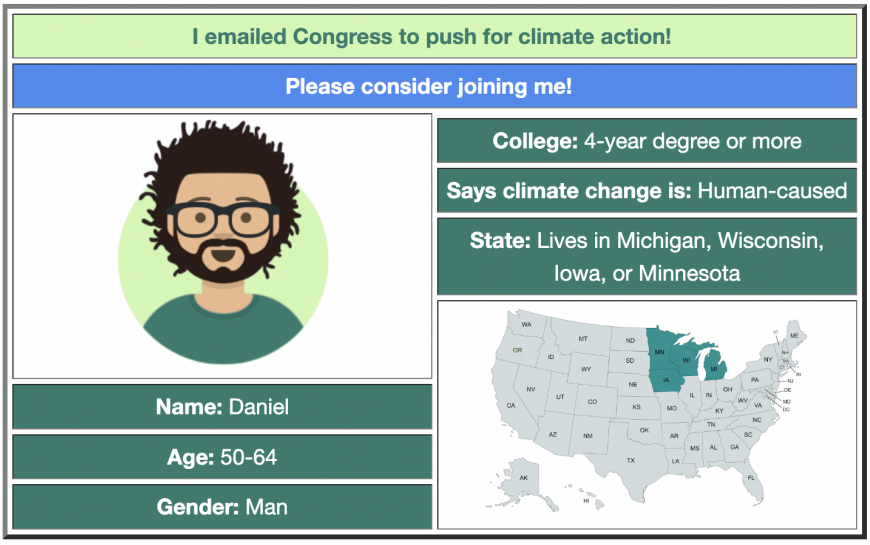
First, all of the sample profiles we've shown you so far have included your own political leanings, like this one:



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

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

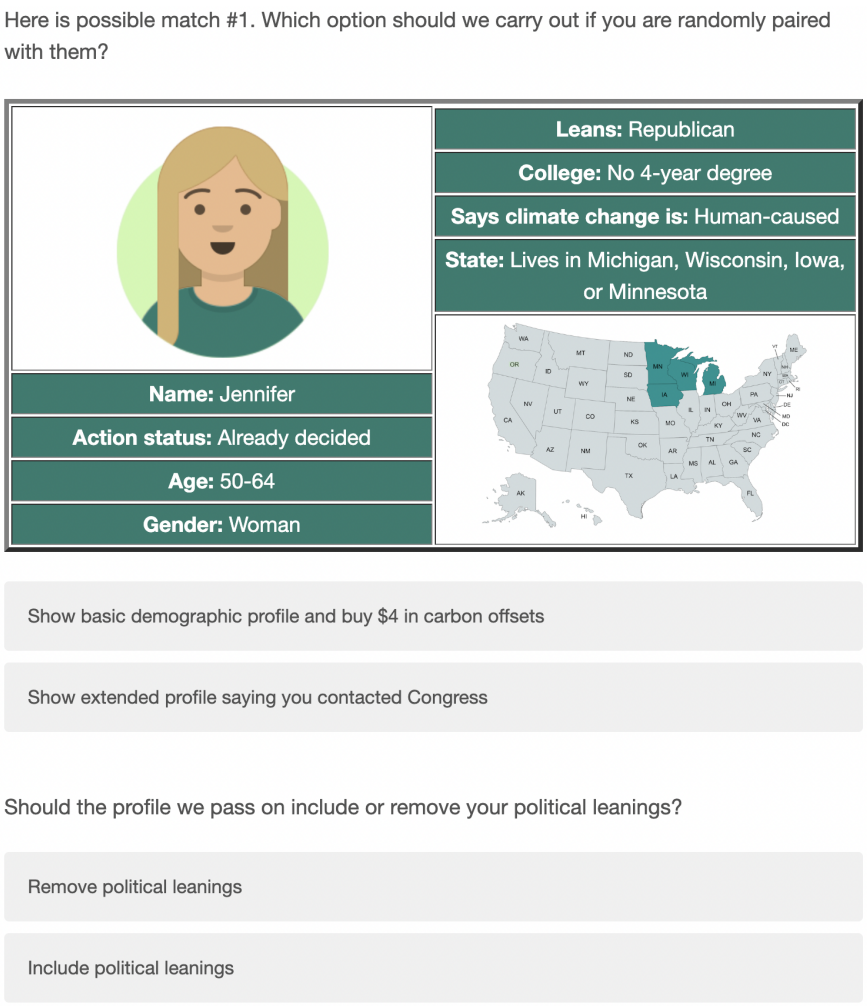
If you removed your political leanings from the extended profile shown above, for example, it would look like this:



E.7.2 Making binary choices to hide politics

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

Figure E16: Making binary choices with the option to hide politics



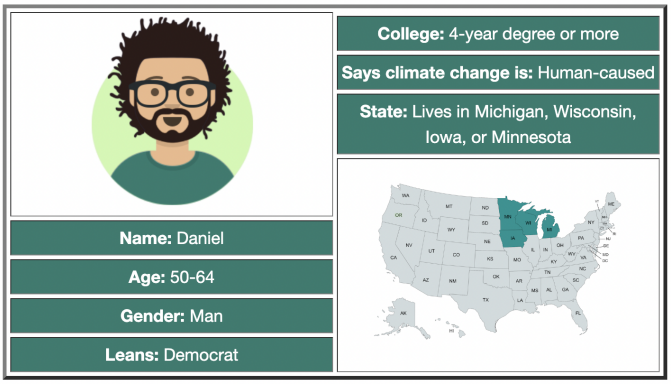
E.7.3 Comprehension check for option to hide politics

Figure E17: Additional comprehension check for those with the option to hide politics

Comprehension check text:

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

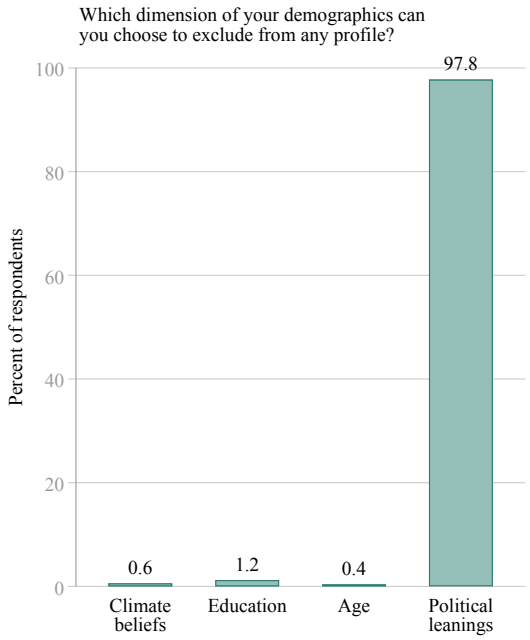
Remember that, as a default, the basic demographic and extended profiles you can choose to pass on will include all of the demographic information below:



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

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

Performance on comprehension check:



E.7.4 Recruitment for round-two WTP

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

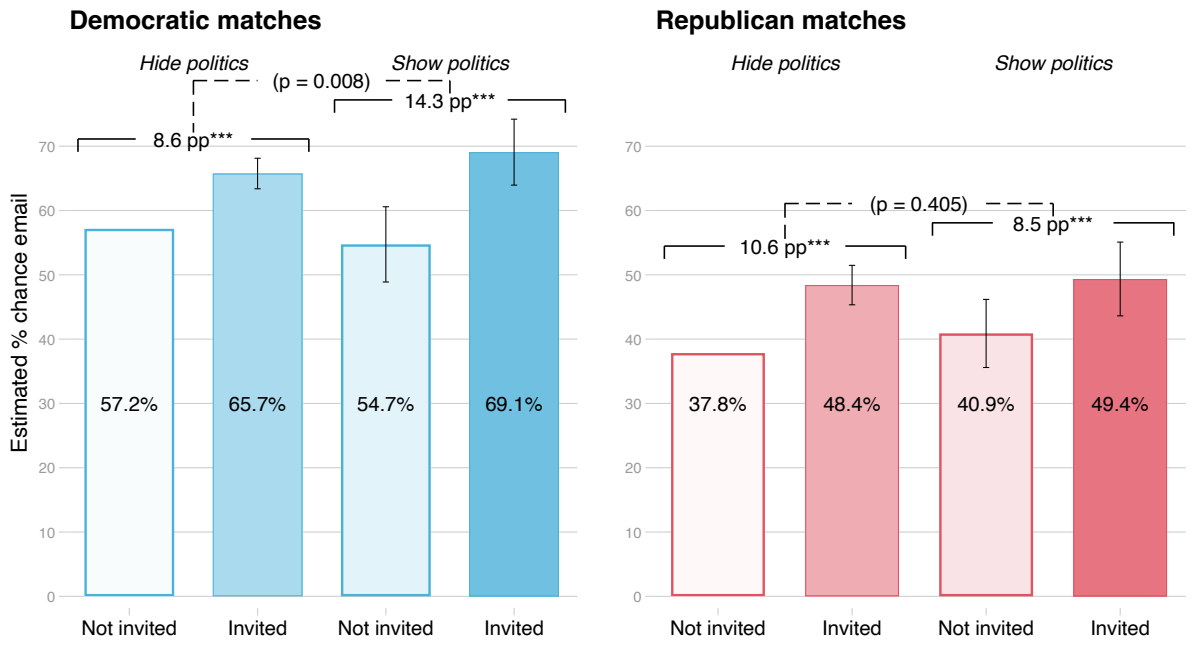
Table E13: Balance table, Round 2 WTP

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

Table E14: 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				

Figure E18: Influence beliefs if hide politics or not: Those without the option



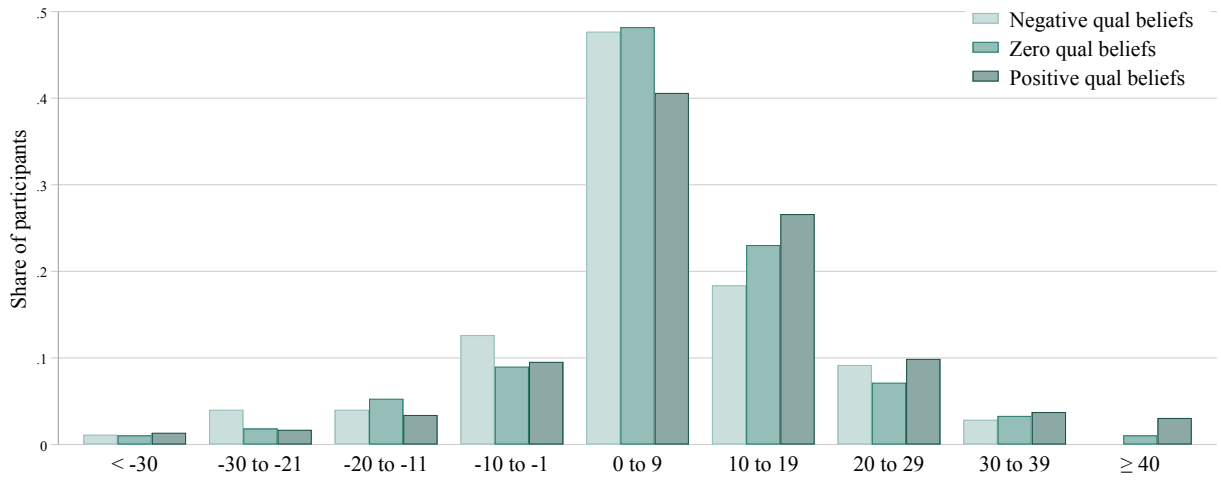
F Deviations from pre-analysis plan

F.1 Measuring influence beliefs

We pre-registered that we would measure Wave-1 participants' influence beliefs (a proxy for $\Delta P(Email)$ in Section 3's framework) by asking them to answer the following baseline question on a scale from 1 (Much less likely) to 4 (No change) to 9 (Much more likely): "If you suggested to someone you don't know personally that they join you in contacting your Senators about a political issue, would it make them more or less likely to do the same?"

In practice, however, this measure is uninformative of participants' precise influence beliefs in our experimental context. On average, those who report negative or positive influence beliefs in the qualitative elicitation only report 1.4pp lower and 2.6 pp higher numeric influence beliefs at endline than those who report zero qualitative influence beliefs (Appendix Table F15), respectively, and the distributions of numeric influence beliefs are dispersed across negative and positive values for all three qualitative belief categories (Appendix Figure F19). See Appendix B.5.1 for more detail on the quantitative measure of influence beliefs. In turn, we find that influence motives (as measured with the β_3 coefficient in Equation 5) are lower among those with positive qualitative influence beliefs; these results are available upon request.

Figure F19: Distribution of quantitative influence beliefs by qualitative baseline measure



Note: This figure plots the distribution of quantitative influence beliefs by participants' baseline answers to the following qualitative question on a scale from 1 (Much less likely) to 4 (No change) to 9 (Much more likely): "If you suggested to someone you don't know personally that they join you in contacting your Senators about a political issue, would it make them more or less likely to do the same?" We elicit quantitative influence beliefs at the end of the Wave-1 survey among a random 30% of Tell-after and Invitation participants. See the note to Appendix Figure B19 for more detail on this measure. Among the sample for whom we observe these quantitative influence beliefs, N = 174 express negative qualitative influence beliefs, N = 755 express qualitative influence beliefs of 0, and N = 293 express positive qualitative influence beliefs.

Table F15: Correlations between qualitative influence beliefs and other Wave-1 baseline traits

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Zero		**Negative**			**Positive**		
	Mean	N	Δ Mean	N	*p-value*	Δ Mean	N	*p-value*
Numeric influence beliefs	5.468	755	-1.416	174	(0.179)	2.590**	293	(0.011)
Woman	0.640	5393	-0.026*	1205	(0.083)	0.005	2339	(0.677)
Hispanic	1.026	5393	0.007	1205	(0.243)	-0.001	2339	(0.803)
Has $\geq$ 4-year college degree	0.825	5393	-0.029**	1205	(0.026)	0.019**	2339	(0.035)
Age ranges:								
20-39	0.111	5393	0.006	1205	(0.549)	0.007	2339	(0.382)
40-59	0.343	5393	-0.010	1205	(0.505)	-0.004	2339	(0.739)
60-79	0.546	5393	0.003	1205	(0.851)	-0.003	2339	(0.803)
Income bins (USD):								
Less than 50,000	0.165	5393	0.013	1205	(0.279)	0.001	2339	(0.912)
50,000-99,999	0.315	5393	0.025*	1205	(0.096)	0.008	2339	(0.505)
100,000-149,999	0.243	5393	-0.033**	1205	(0.011)	-0.018*	2339	(0.072)
150,000-199,999	0.129	5393	-0.006	1205	(0.585)	0.009	2339	(0.261)
200,000 or more	0.148	5393	0.001	1205	(0.928)	-0.000	2339	(1.000)
Residence by state group:								
Red state	0.239	5393	0.028**	1205	(0.046)	-0.024**	2339	(0.016)
Blue state	0.431	5393	-0.024	1205	(0.134)	0.053***	2339	(0.000)
Purple state	0.330	5393	-0.003	1205	(0.841)	-0.029***	2339	(0.008)
Climate beliefs:								
Climate worry (1-7)	6.401	5393	-0.095***	1205	(0.001)	0.127***	2339	(0.000)
Desire for climate action (1-7)	6.729	5393	-0.058***	1205	(0.004)	0.052***	2339	(0.000)
Perceived local impacts (1-7)	5.465	5393	0.035	1205	(0.289)	0.112***	2339	(0.000)
Political engagement and beliefs:								
Political-engage. index (std)	-0.113	5393	-0.049	1205	(0.114)	0.456***	2339	(0.000)
Political-efficacy index (std)	0.047	5393	0.122***	1205	(0.000)	-0.242***	2339	(0.000)
Degree prefer Dem friends (1-7)	6.047	5393	-0.043	1205	(0.179)	-0.014	2339	(0.560)

Note: This table tests for differences in baseline traits by participants' baseline answers to the following qualitative question on a scale from 1 (Much less likely) to 4 (No change) to 9 (Much more likely): "If you suggested to someone you don't know personally that they join you in contacting your Senators about a political issue, would it make them more or less likely to do the same?" Appendix B.2.2 details each of these baseline traits. 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 1 and 2 present means and sample sizes for each trait among those who answered 5 on the qualitative scale of influence beliefs. Columns 3 and 7 then present the difference in baseline means between those who answered 1-4 or 6-9 on the qualitative scale, respectively, and those who answered 5. Columns 4 and 6 give the sample sizes of these groups, and columns 5 and 8 then present heteroskedasticity-robust p-values testing the null that the differences in means in columns 3 and 6 are zero. In columns 3 and 6, we indicate statistical significance at the 10%, 5%, and 1% levels by *, **, and ***, respectively.