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

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November 2023

Link to current version (updated frequently)

Abstract

Political action spreads through social networks, so citizens may have power to shape policy both through their own advocacy and by recruiting others to act. Do citizens try to spread grassroots action? If so, do they work to build broad, bipartisan coalitions or to recruit others like them? We focus on the climate movement, where most citizen advocates are Democrats. Mobilizing bipartisan action could more effectively promote climate policy in Congress, but record-high affective polarization—animosity towards counter-partisans—may impede cross-party grassroots cooperation. In online experiments with over 20,000 participants, we connect Democrats with other Americans across the political spectrum (all of whom believe climate change is human-caused) to understand whether and how they try to recruit others to push for climate policy. Democrats are motivated to recruit others—they are 10% more likely to email Congress when doing so allows them to invite others to act. Even while Democrats say that a bipartisan climate movement would be more effective, however, they are 27% more likely to invite other liberals than conservatives to email Congress. This gap does not arise from Democrats' own distaste for engaging with counter-partisans, but rather can be explained by their correct beliefs that their invitation will have about half as much impact on conservatives' action. Anticipated affective polarization drives these beliefs: Democrats estimate that conservatives would respond three times more to invitations that did not identify them as liberals.

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

1 Introduction

Across issues, passing legislation in Congress typically requires bipartisan support. Split-party governmental control is the norm in the US—a single party controlled the presidency, Senate, and House for only 16 of the last 52 years—and passing legislation usually requires minority-party votes even under single-party control (Curry and Lee, 2020).[1] Then, achieving long-term policy progress on any issue likely requires building a cross-party legislative coalition to support it.

Mobilizing grassroots citizen lobbying on both sides of the political aisle may be an effective tool in building these bipartisan coalitions in Congress. Citizen lobbying is common—over 20% of Americans contact politicians at local, state, and national levels every year (Oliphant, 2018)—and constituent lobbying has been shown to change legislators' behavior (Bergan, 2009; Bergan and Cole, 2015; Butler and Nickerson, 2011; Broockman and Skovron, 2018). If legislators differentially respond to the concerns of their voter base, the composition of grassroots movements could crucially affect government policy (Fenno, 1977; Kastellec et al., 2015; Lax et al., 2019). Citizens themselves may play key roles in shaping this composition. Long-standing research in sociology (e.g. McAdam (1986); Gould (1991, 1993); Opp and Gern (1993); McAdam and Paulsen (1993)) and more recent causal work in economics and political science (Nickerson, 2008; Bond et al., 2012; González, 2020; Bursztyn et al., 2021) find that political and social movements spread through networks, so citizens may have power to shape political outcomes both through their own advocacy and by recruiting others into grassroots coalitions.

Do citizens try to engage others in political advocacy? If so, do they differentially attempt to mobilize broad, bipartisan citizen coalitions or to recruit others like them? Even if citizens recognize that bipartisan grassroots pressure could more effectively contribute to policy change, record-high affective polarization (Iyengar and Krupenkin, 2018; Boxell et al., 2022)—dislike or distrust of those across the political aisle—could prevent citizens from building coalitions across party lines. Americans have been shown to favor co-partisans over counter-partisans in domains ranging from job callbacks (Gift and Gift, 2015) to romantic relationships (Klofstad et al., 2013; Huber and Malhotra, 2017; Iyengar et al., 2018). Does affective polarization hold back efforts to build bipartisan grassroots coalitions?

We use a large online experiment to examine these questions in the context of US climate advocacy. While the environmental movement started in the 1970s with bipartisan action, support and advocacy for climate policy today skew heavily to the political left, with starker partisan belief gaps than on any other issue (Atske, 2020; Uhlmann, 2020; Leiserowitz et al., 2023; Newport, 2023). Mobilizing climate advocacy by conservatives may then be even more differentially impactful than mobilizing non-stereotypical activists on other issues. Many Americans on the political right could in theory be recruited to act: over 50% of GOP-leaners support a range of policies to reduce carbon emissions (Tyson, 2021), and 19% of moderate Republicans say they would contact government officials about climate change (Leiserowitz et al., 2023). We examine how Democrats, modal members of the climate movement, work to build this coalition.

A key challenge in identifying whether citizens differentially act to build political movements along or

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

across party lines is that social networks are politically segregated, so activists likely have differential access to co-partisans (Jones et al., 2022). To solve this challenge, we recruit 20,000 participants from social media and online panels to an online survey in which we exogenously connect Democrats with other Americans across the political spectrum. Within this basic social network, we provide Democrats with a series of opportunities to recruit others for climate advocacy.

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

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

Democrats internalize these impacts and try to recruit others for climate action. The *Wave-1 Action experiment* (N = 8,937) tests whether Wave-1 Democrats are more likely to email Congress when doing so lets them invite others to join. We randomize participants across three arms. In a pure-control group, participants decide whether to email Congress with no mention of others knowing that they did so. In an "Invitation" arm, participants are told that if they email Congress, we will show invitations to join them (Figure 2b) to up to 10 future Wave-2 participants. While comparing email rates between the Invitation and pure-control groups captures the policy-relevant impacts of opportunities to inspire others, Invitation participants may differentially email Congress both due to influence motives—attempts to affect whether Wave-2 participants email Congress—and because they derive self- or social-image benefits from telling others that they did so. To identify influence motives, we randomize the final third of Wave-1 participants to a "Tell-after" arm: we will tell up to 10 Wave-2 participants if they email Congress, but only *after* those targets decide whether to do so or not. The Tell-after and Invitation groups only differ in that Invitation

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

participants' choice to email Congress or not can affect whether Wave-2 participants do the same; comparing email rates across these groups cleanly identifies whether Democrats act to mobilize others.

In total, 47.1% of those in the Invitation group email Congress, compared to 31.0% of those in the pure control group and 44.3% of those in the Tell-after group. We reject equality between the Invitation and Tell-after groups with $p = 0.042$. Even as people value telling others that they emailed Congress when it cannot affect their in-survey action, influence motives are also strong: at least one fifth of the total effect of the Invitation treatment arises from Democrats' attempts to mobilize others' political action.[3]

While Democrats work to build the climate movement overall, they are more likely to try to recruit other liberals than to reach across party lines. We test *whom* Democrats seek to engage in climate advocacy in a better–powered experiment embedded among participants who email Congress in the Wave-1 pure control group. In the *Willingness-to-Pay (WTP) experiment*, we show each participant (N = 1,023) twenty past survey-takers who will be returning to take a second survey, during which they will have an opportunity to email Congress via our form. One of these returning participants will be randomly chosen to see a profile of the WTP participant. For each of the 20 possible matches, we ask WTP participants to choose between two options: (1) passing on a basic demographic profile and making a small carbon-offset donation from our research funds, or (2) passing on a profile that includes their demographics, states that they emailed Congress, and invites the match to join in doing so (Figure 2b). We randomize whether each possible match would see the WTP participant's profile before or after themselves deciding whether to email Congress;[4] we then identify efforts to mobilize others by how much more likely participants are to choose profiles saying that they emailed Congress when doing so can affect matches' action. Observing 20 binary choices for each WTP participant powers us to test for heterogeneous influence motives by match traits.

As in the Wave-1 action experiment, WTP participants strongly value opportunities to invite possible matches to email Congress. Even when Democratic WTP participants know that all matches believe that climate change is mostly human-caused, however, they are 27% more likely to try to mobilize other liberals than conservatives. This partisan gap dwarfs gaps in attempts to influence targets by any other trait.

Motivated by a conceptual framework, we then use a series of follow-up experiments to decompose the preferences and beliefs that hold back Democrats' efforts to recruit bipartisan action. The partisan influence gap could arise from three sources: (i) Democrats may believe that their invitations have differential impact on liberals' versus conservatives' action; (ii) they may expect an email from a liberal versus conservative to be differentially impactful in helping to pass climate policy; or (iii) they may derive differential emotional returns from trying to engage co- versus counter-partisans in the climate movement. Affective polarization could then matter in two ways: Democrats may get higher emotional benefits from engaging with liberals because they dislike counter-partisans, or Democrats might expect their invitations to have less impact on conservatives because they expect counter-partisans to dislike them.

We find that Democrats expect their invitations to have about twice as much impact on liberals as on conservatives. In a sample (N = 194) recruited alongside Wave 1, Democrats estimate that their invitations

[3]The action gap between the Tell-after and pure control groups could also reflect influence motives if Tell-after participants email Congress in part to motivate Wave-2 targets' climate action after the survey.

[4]When possible matches would see a WTP participant's profile after deciding whether to email Congress, the extended profile in option (2) states that they emailed Congress but does not include an explicit invitation to join in action, as in Figure 2c.

will make liberals about 6pp more likely to email Congress, while making conservatives only about 3pp more likely to do so; we reject that these effects are equal (p = 0.029). Democrats' beliefs here closely match the true impacts of invitations in the Wave-2 sample.

Additional experiments suggest that this gap in influence beliefs can fully explain Democrats' preference to reach out along party lines. First, we find no evidence that Democrats prefer emails from liberals versus conservatives when they can be *obtained with certainty*. We recruit a separate sample of 1,083 Democrats and ask them to choose, for a range of demographic groups, whether our research team should donate a moderate amount to carbon offsets or enlist someone like that group to email Congress for certain. Here, Democrats are equally likely to choose them from liberals and conservatives. 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. Thus, Democrats' relative unwillingness to reach across the political aisle does not arise from differential beliefs about emails' impacts or affective preferences to engage liberals and conservatives in the climate movement per se.[5] Rather, Democrats differentially try to engage other liberals in climate action because they don't expect cross-party outreach to succeed.

On the other hand, second-order affective polarization crucially shapes Democrats' beliefs about the impacts of their invitations on liberals versus conservatives. In a second round of the WTP experiment (N = 995), we randomly allow some participants to hide their own political leanings from the profiles they can choose to pass on. While Democrats hide their political leanings from profiles shown to fellow liberals only 8% of the time, they hide their political leanings from 56% and 66% of profiles that conservatives matches would see after versus before deciding whether to email Congress, respectively. In quantitative belief elicitations, Democrats estimate that hiding their political leanings from conservative recipients would make their invitations more than three times as effective.

In sum, even while Democrats' *own* distaste for engaging with counter-partisans does not restrict their efforts to build a more bipartisan movement, these efforts are still held back by a context of partisan mistrust. Democrats' reluctance to reach across the aisle may then be even stickier than if it arose from their own partisan disaste: promoting cross-party mobilization would require durably changing Democrats' beliefs about counter-partisans' polarization. Democrats' beliefs about conservatives' reactions to invitations are accurate in this setting, so no obvious intervention would close these gaps. Our results suggest that unified bipartisan coalitions among citizens are unlikely to form, and that achieving bipartisan policy support in Congress may require parallel liberal and conservative movements at the grassroots.

Our paper contributes to five primary literatures in economics and political science. First, we contribute to a growing literature on the diffusion of social and political movements across space (Qin et al., 2019; Aidt et al., 2022; García-Jimeno et al., 2022) and within social networks (Nickerson, 2008; González, 2020; Bursztyn et al., 2021).[6] Our paper is the first to show that citizens internalize their spillovers on others'

[5]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 liberal versus a conservative 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.

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

political action and, in fact, intentionally try to spread political movements.[7]

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. In addition to extending the literature on influence motives to politics, we also decompose for the first time the beliefs and preferences that underlie these motives.

Third, we add to a growing literature on the role of affective polarization and partisanship across American life.[8] Most related, Broockman and Ryan (2016) find that citizens differentially contact co-partisan legislators. We show that Democrats are also more likely to invite co-partisans into a grassroots movement, even when common-ground beliefs are clear.[9] Both of these political-engagement gaps could hinder efforts to build bipartisan policy support in Congress. We also provide some of the first evidence on how beliefs versus preferences contribute to differential engagement with co-partisans. 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, arising because they expect to have less influence on conservatives. At the same time, *anticipated* affective polarization drives these beliefs and may impede cross-party cooperation.

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

[7] We also document substantial diffusion from a particular form of interaction: sending invitations to join in emailing Congress. In experimentally introducing and evaluating a tool to facilitate diffusion, our work is most closely related to Bond et al. (2012) and Jones et al. (2017), who find that Facebook banners showing friends who reported voting in national elections increased voting both among viewers and their friends. Our paper is unique in experimentally creating interactions between political agents not connected by organic social or geographic networks, allowing us to causally estimate how spillovers are mediated by both political and non-political similarity.

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

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

on political advocacy on climate change in particular. Prior work has studied the determinants of beliefs about climate policy (e.g. Drews and van den Bergh, 2016; Maestre-Andrés et al., 2019; Dechezleprêtre et al., 2022), low-carbon behavior (e.g. Allcott, 2011; Bilen, 2023; Ho and Page, 2023), and climate-friendly donations (Andre et al., 2021; Bernard et al., 2023). Together with Page et al. (2023), this project is to our knowledge the first experimental work studying real-world measures of political climate action.

Finally, we add to the long and growing literature on the political-economy constraints to efficient environmental policy (Hahn and Stavins, 1992; Oates and Portney, 2003; Anthoff and Hahn, 2010; Besley and Persson, 2023). Recent work, for example, considers how domestic re-election concerns constrain international climate treaties (Battaglini and Harstad, 2020), how political lobbying by upstream industries creates implicit trade subsidies to emissions-intensive goods (Shapiro, 2021), and how distributional concerns shape citizens' support for climate policy (Dechezleprêtre et al., 2022). This paper suggests that frictions to bipartisan grassroots action may reduce pressure for policy change in Congress, both on climate change and across policy issues.

The paper proceeds as follows. First, Section 2 describes the experimental context and Section 3 lays out a framework for influence motives. Section 4 describes the impacts of invitations in the Wave-2 sample, and Sections 5 and 6 experimentally test whether and to whom Democrats try to build the climate movement. Finally, Section 7 examines the mechanisms underlying Democrats' differential efforts to reach out along party lines, Section 8 tests the robustness of our main results, and Section 9 concludes.

2 Experimental context: A constructed social network for climate action

2.1 Motivating descriptive evidence

We start with motivating 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 this political coalition. Throughout the project, we recruit these Democrats via ads on social media for a survey about climate change, yielding a sample that is highly politically engaged and interested in climate action. Alongside our experimental sample (see Section 5.1.2 for details), we recruit a sample of about 200 Democrats among whom we document key motivating facts.

Democrats in our sample want a bipartisan climate movement: Democrats want bipartisanship in Congress on climate change: 89% at least somewhat agree that building support for climate policy among both Democratic and Republican politicians in Congress is crucial for reducing emissions, and 87% agree that the US government can only pass ambitious climate legislation in future if lawmakers of both parties support it (Appendix Figure A1). Moreover, they see bipartisan citizen advocacy as a key tool to build bipartisanship in Congress: 84% at least somewhat agree that advocacy by conservatives, rather than liberals, could more effectively increase Republican lawmakers' support climate policy (Panel B, Appendix Figure A2), and 97% say that a bipartisan climate movement would be more effective than a purely liberal movement in advancing US climate policy (Panel A, Appendix Figure A2). Democrats see a role for liberals like themselves in building this bipartisan climate movement: 82% agree that liberals should try to get more conservatives involved in political climate advocacy (Appendix Figure A3).

But they have primarily recruited other liberals: Few Democrats in our sample have themselves tried to recruit conservatives for climate action. 58% says that they've invited someone to join in climate advocacy in the last 5 years, but they've primarily reached out to other liberals (Appendix Figure A4). Only 50% can recall a particular instance in which they invited a conservative to join in political advocacy in the last 5 years, while 95% can recall a particular instance of inviting a liberal. While this gap could arise simply because Democrats are primarily close to other liberals (Appendix Figure A4), it could also arise from more subtle preferences or beliefs. For example, the large literature on affective polarization—dislike or distrust of counter-partisans (e.g. Iyengar and Krupenkin 2018; Iyengar et al. 2019)—suggests that Democrats may simply dislike engaging with conservatives.

Democrats in our sample are highly affectively polarized: Indeed, Democrats in our sample match or even exceed record-high affective polarization in national samples. The American National Election Survey (ANES) identifies affective polarization using a "feelings thermometer:" participants rate how warmly they feel towards Democrats and Republicans on a scale from 0 degrees (cold) to 100 degrees (warm), and affective polarization is calculated as the gap in warmth towards their own party and the opposing party. 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 in the nationally-representative 2020 ANES (Panel A, Appendix Figure A5). When asked their friendship preferences on a scale from 1 (Strongly prefer a Republican friend) to 7 (Strongly prefer a Democratic friend), 70% of Democrats in our sample select six or seven.

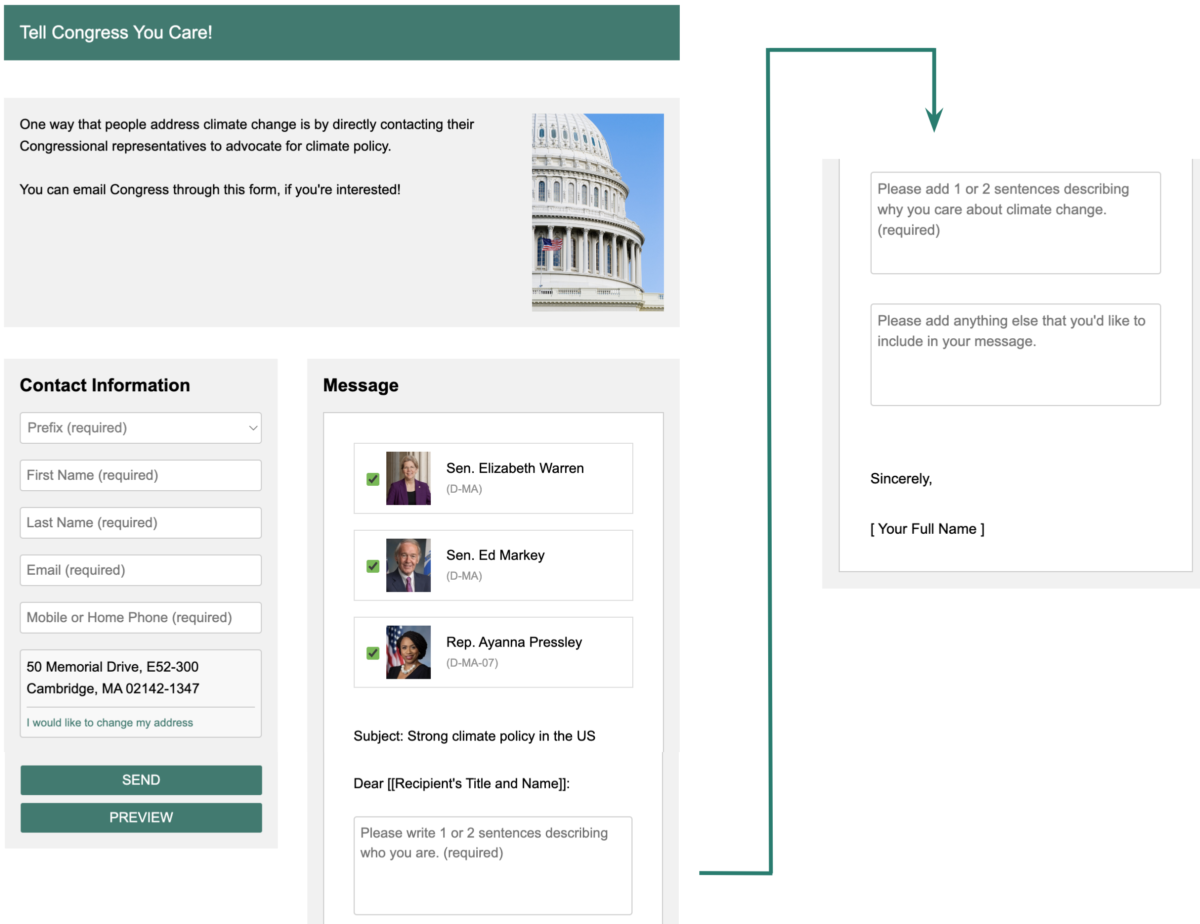
This project: In this project, we connect Democrats with Americans across the political spectrum to examine their efforts to expand climate advocacy when they are unconstrained by segregated social networks. We then test how affective polarization affects Democrats' efforts to reach across party lines.

2.2 Real-stakes email advocacy

In the study, we measure climate advocacy by whether participants email their national Senators and US House Representative about climate change via a form embedded in our online survey (Figure 1). Participants enter their name and address into the form, it connects them with their national representations, and participants then write a customized email message to those representatives. We observe all emails, and we analyze both whether participants themselves email Congress and whether they invite others to do the same.

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 the Soft Edge, a software company that provides nearly identical forms to advocacy organizations across the US. Second, emailing Congress via our survey is a meaningful form of climate action. Politicians systematically misperceive constituents' opinions (Broockman and Skovron, 2018), and experimental evidence suggests that citizen lobbying by email and phone can affect politicians' voting behavior (Bergan, 2009; Bergan and Cole, 2015). Finally, emailing Congress in our setting requires effort and may be relatively robust to experimenter demand effects. Participants must customize the email body, and 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 main experimental samples, about 31% email Congress.

Figure 1: The form for emailing Congress



Note: This figure shows a screenshot of the form that is embedded in our online survey and through which participants can email Congress. Participants are brought to this page, which automatically identifies their national representatives, after entering their address on a previous page. Note that the email subject line is uneditable and supports climate action, though it varies across senders. Appendix Section B.2 shows screenshots of the full process of emailing Congress.

One additional feature of this email form warrants particular note: while the email body is fully customizable, the email subject line is uneditable and always strongly supports climate action.[10] We impose these fixed subject lines to mitigate the possibility that Democrats avoid mobilizing conservatives because they expect their emails to oppose climate policy; throughout the experiment, participants know that no email sent via our form can fully oppose climate action. This restriction allows us to analyze whether Democrats choose to pursue advocacy *for climate action* from Americans across the political spectrum.

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

We examine Democrats' efforts to build the climate movement by constructing a simple online network connecting them with Americans spanning the contiguous US, political groups, age, gender, and education.

Our basic network relies on recruiting two waves of study participants: a Wave 1 of "influencers" and a

[10] In particular, the subject line is randomized across a set of phrases such as "My strong support for US climate policy," "Please address climate change", and "Ambitious climate action in the US." See Appendix Section B.2 for details.

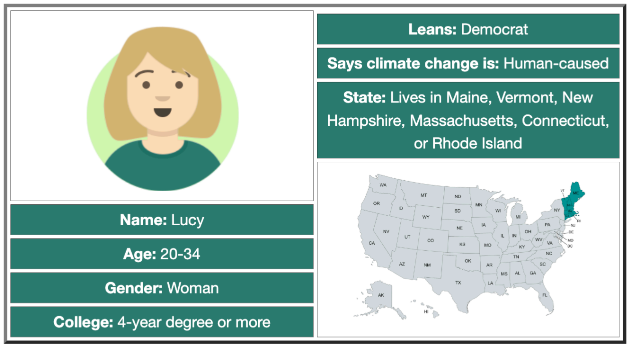
Wave 2 of "targets," who will see information about each other via semi-anonymous demographic profiles (Figure 2a). We first recruit Wave 1, all of whom have an opportunity to email Congress about climate change. Later, we will recruit Wave 2 and give them the same email opportunity. Across several experimental designs, we give some Wave-1 participants costly opportunities to invite future Wave-2 participants to join in emailing Congress when they take our survey. In particular, they can pass on profiles (Figure 2b) that show their demographics and avatar, say that they emailed Congress, and invite the Wave-2 participant to do the same. We then truthfully and randomly pass on Wave-1 invitations when we recruit Wave 2, 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 real-world climate movement. In particular, we design the Wave-1 sample of influencers to approximate typical activists in the US climate movement, restricting to members of the Democrat party who believe that climate change is mostly human-caused (Atske, 2020; Uhlmann, 2020; Leiserowitz et al., 2023; Yeo, 2023). We then connect these Wave-1 Democrats with Wave-2 participants who belong to or lean towards either the Democratic or Republican parties, but all of whom also believe that climate change is mostly human-caused; for simplicity, we refer to these Wave-2 groups henceforth as "liberals" and "conservatives," respectively. In this set-up, Wave-1 participants' efforts to invite Wave-2 participants to email Congress capture whether modal members of the US climate movement differentially try to engage potential allies across the political spectrum.

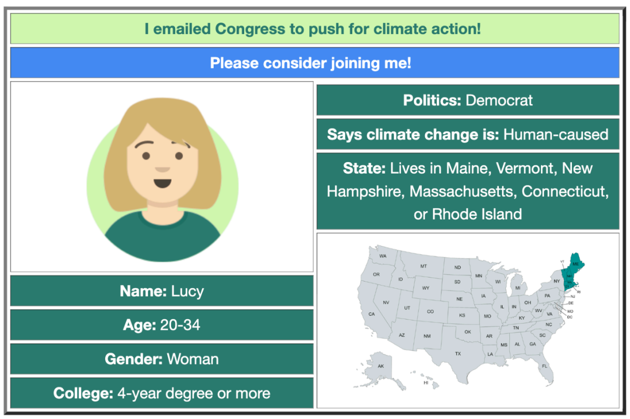
Sections 5.1.2 and 4.1.2 detail how we recruit Waves 1 and 2, respectively, and Appendix Section A.2 describes how we screen participants by climate beliefs and political affiliation.

Figure 2: Demographic profiles and invitations to act

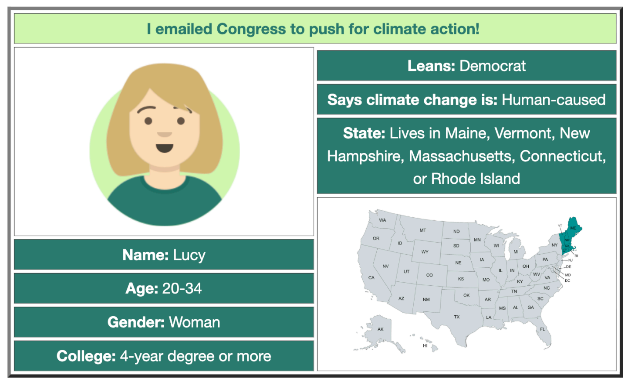
(a) Sample basic demographic profile



(b) Sample invitation for Wave-2 participants



(c) Sample profile saying emailed Congress



Note: Sub-figure (a) shows a sample demographic profile, which form the basis of all interactions between participants in this project. 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. Across several experimental designs, we isolate influence motives by comparing how often participants pass on invitations, like that in sub-figure (b), to how often they pass on profiles, like that in sub-figure (c), that cannot affect whether recipients' email Congress.

Matching Wave-1 and Wave-2 demographic groups: To test how Democrats target their efforts to spread the climate movement, we randomly pair Wave-1 Democratic influencers with Wave-2 targets across demographic and partisan groups. We first construct demographic cells defined by 15-year age bins, gender, educational attainment, and 13 clusters of politically similary and geographically close states, described below.[11] We then randomly pair each Wave-1 demographic cell with a demographic cell in Wave 2. Wave-1 participants in a particular demographic cell will have opportunities to influence Wave-2 participants in their paired demographic cell; within demographic cells, we vary whether Wave-1 Democrats are paired to influence either Wave-2 liberals or conservatives. In turn, some Wave-2 participants will be randomized to see invitations to email Congress from members of their paired Wave-1 demographic group.[12]

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

[12] Pairing up demographic groups of Wave-1 and Wave-2 participants, rather than individually randomizing Wave-1 participants

Feasibly implementing this network structure—in particular, passing on invitations from Wave-1 participants to paired Wave-2 participants—requires ensuring that all demographic cell in Waves 1 or 2 are sufficiently large. To do so, we restrict both the Wave-1 and Wave-2 experimental samples to participants who live within the contiguous United States, are within the ages of 20 and 79, identify as a man or woman, and identity as white. Appendix Section A.2 details how we screen participants by demographics.

Characterizing targets' climate-policy marginality: In addition to testing how Democrats differentially try to recruit climate action by liberals versus conservatives, we will also test whether they differentially recruit climate advocacy in states where it may be more impactful. To do so, we classify states into categories of climate-policy marginality: "blue states," where legislators would be likely to vote in favor of a climate bill even if not many residents called to support it; "red states," where legislators would be unlikely to vote in favor of a climate bill even if many residents called to supported it; and "purple states," where legislators could be convinced to vote for a climate bill if enough of their constituents supported it.

We use surveys of Democrats recruited on Facebook and Twitter to construct 13 groups of geographically nearby states that participants tend to assign to the same policy-marginality group (see Appendix Section A.3 for details.) Across our experimental designs, Wave-1 participants see demographic profiles (Figure 2a) for Wave-2 participants that show the state group in which they live, letting us test whether Wave-1 Democrats differentially seek to mobilize climate action where it may be particularly impactful in advancing climate policy. In particular, we would expect influencers who care about impact to differentially try to mobilize emails in purple states.

3 Conceptual framework

In this section, we lay out a simple conceptual framework for how influence motives may depend on key beliefs and preferences. We then use this framework to outline the full experimental structure.

3.1 The value of inviting others

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

An "effective-altruist" model: Consider first a simple model of influence motives in which the influencer cares only about the impacts of her invitation on the likelihood that Congress passes climate policy. Then, the value of inviting a future participant is as follows:

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

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

First, $\Delta P(Email)$ captures the sender's beliefs about the impact of her invitation on the probability that the target emails Congress, and $V(Email\ Impact)$ captures how highly she values the email's expected impact; the product of these terms then captures the net value of the influencers' invitation via the impact of the target's email. Next, inviting participants to join in emailing Congress during the survey could also affect whether they take pro-climate action after the survey. The "effective-altruist" value of an invitation thus also includes the product of how much more likely the target is to take post-survey pro-climate action—$\Delta P(Target\ Acts\ After)$—and her valuation of this action's impact—$V(Action\ After\ Impact)$.[13] Finally, we subtract C, any opportunity cost of sending this invitation.

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

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

Just as people value telling others that they voted (Dellavigna et al., 2017; Gerber et al., 2017), passing on an invitation saying that one emailed Congress could activate self-image or social-image benefits or costs, $A(Image)$. Next, the sender may derive emotional returns from knowing that she's doing her part to spread climate action during the survey or after, given by $A(Try\ Influence\ Email)$ and $A(Try\ Influence\ After)$, respectively. Finally, how much the influencer values her target's action may comprise not only her valuation of their action's impact, but also the affective benefits or costs of knowing that they are involved in the climate movement, $A(Target\ Involved)$.

3.2 Identifying efforts to build the climate movement

The full expression for $V(Invite)$ above highlights a key empirical challenge in measuring Americans' attempts to expand the climate movement: participants' choices to pass on invitations may be driven both by image returns and by influence motives per se.[14] Across a series of experimental designs, we identify

[13] This framework assumes that the inviters value the *absolute* impacts of their invitations on targets' action, rather than their proportional impact on targets' action. If influencers care only about their impact on the likelihood that climate policy is passed, proportional impacts would only matter if inducing citizens with a lower baseline email probability to email Congress differentially affected their likelihood of continuing climate action outside of the survey. For simplicity, our framework assumes that whether Wave-2 participants email Congress during the survey does not affect their likelihood of pro-climate action after the survey. Proportional effects on action may also matter if influencers derive differential affective benefits from reaching out to or engaging an "unlikely" advocate in the climate movement. Any such effects will be captured in $A(Target\ Involved)$ or $A(Try\ Influence)$, defined below.

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

participants' attempts to spread climate action by varying the timing of when Wave-1 Democrats can pass on profiles saying that they emailed Congress.

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

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

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

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

We use this logic across a series of experimental designs, empirically identifying influence motives by randomly varying whether Wave-1 participants can tell Wave-2 targets that they emailed Congress before versus after those targets decide whether to do the same.

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

3.3 Affective polarization and differential influence motives by party

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

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

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

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

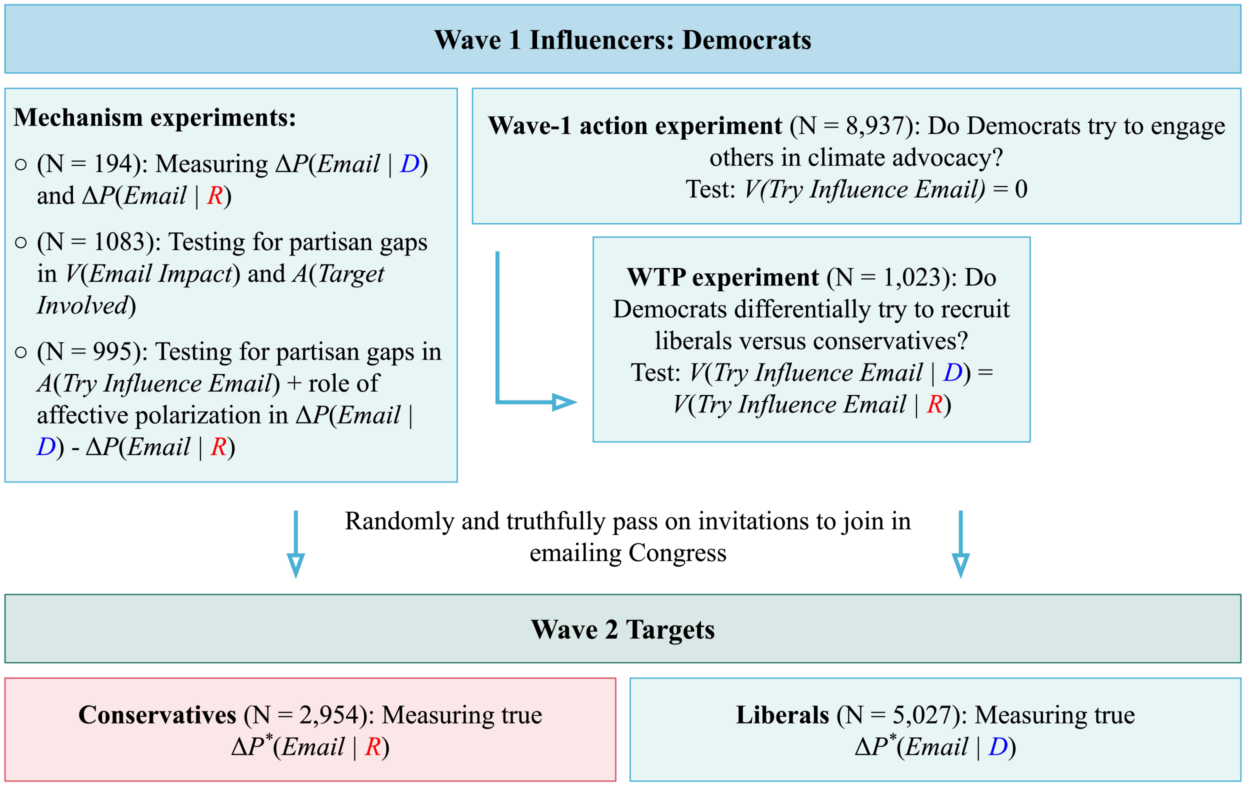
3.4 A roadmap to the experiments

This framework for influence motives guides the project design, which consists of numerous connected experiments (Figure 3). In the Wave-1 sample, we first document the existence of influence motives in the *Wave-1 action experiment* (Section 5) and show that Democrats differentially try to influence other liberals in the *Willingness-to-pay (WTP) experiment* (Section 6), a sub-experiment embedded in the pure control group of the Wave-1 action experiment.

Guided by the framework laid out in this section, we then run a series of follow-up experiments in separate samples of Democrats to identify what underlies this influence gap. In particular, we will measure Democrats' beliefs about their influence on liberals and conservatives, $\Delta P(Letter|D,R)$; test for partisan gaps in the perceived impact of emails from conservatives versus liberals, $V(Email\ Impact|D,R)$; and test for partisan gaps in the affective returns to trying to influence liberals versus conservatives, $A(Try\ Influence\ During|D,R)$, and to having them involved in the climate movement, $A(Target\ Involved|D,R)$. We test also for the role of second-order affective polarization in Democrats' influence beliefs, $\Delta P(Letter|D,R)$.

We then randomly and truthfully pass on Wave-1 invitations to Wave-2 participant in the Wave-2 experiment, testing the true impacts of invitations on whether liberals and conservatives email Congress, $\Delta P^*(Letter|D,R)$, and assessing whether Wave-1 Democrats' influence beliefs are accurate.

Figure 3: An overhead view of the project design



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

4 The Wave-2 experiment: Do action invitations work?

Before examining Democrats' attempts to build the climate movement, we first establish a key feature of our context: Democrats can actually influence Wave-2 participants' action. In an experiment among Wave-2 participants, we estimate $\Delta P^*(Letter|D)$ and $\Delta P^*(Letter|R)$, the true effects of invitations on whether Wave-2 recipients email Congress. Seeing an invitation from a Wave-1 Democrat makes Wave-2 participants substantially more likely to email Congress, but the absolute influence on conservatives' action is only about half that on liberals' action. Section 4.1 describes our Wave-2 experimental design and sample, Section 4.2 presents our empirical specifications, and Section 4.3 presents the Wave-2 results.

4.1 Experimental design, sample, and fidelity

4.1.1 Experimental structure

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

See basic demographic profile of Wave-1 match: Eligible Wave-2 participants first build a basic demographic profile (Figure 2a). We next show them the comparable profile for a Wave-1 participant, saying that they've been randomly paired to see this profile to give them a sense of who else is involved in our study; see Panel A of Appendix Figure B1 for a sample profile and explanation. Note that we show all participants the demographics of an earlier participant at this stage, regardless of their later treatment status, to ensure that any impacts of seeing an action invitation do not arise just from seeing details about a Wave-1 participant. To encourage Wave-2 participants to pay attention to their match's demographics, we then ask them to rate how similar they are to this recent participant and explain their answer.

Treatment: Show participants action invitations from Wave-1 match: After a series of questions about their climate beliefs, we show all participants a preview of the upcoming opportunity to email Congress. (See Appendix B.2 for details of the preview.) Next, we randomly implement the invitation treatment. In particular, participants in the Wave-2 treatment group see an invitation like that in Figure 2b 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 B1).[15] 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 over several steps: interested participants can first opt into the email process, next choose whether to continue the process after being informed that the email form will require an address, and finally can write and send their emails. Appendix Section B.2 lays out the survey flow for the process of emailing Congress.

[15] 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. Section 5 describes the Wave-1 treatment variations under which some participants knew these invitations would be sent. To facilitate this truthful matching, Wave-2 participants are randomly assigned to treatment status—whether they will later see an invitation or not—before being paired with a Wave-1 match and seeing their basic demographic profile. After randomly assigning Wave-2 participants' treatment status, we then pair them with a Wave-1 participant in their paired demographic cell (footnote 12) for whom we can truthfully pass on an invitation or not. Appendix Section B.1 describes this matching process in detail.

Table 1: Wave-2 sample summary and balance

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

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

4.1.2 Wave-2 sample recruitment and summary

Recruitment: We recruit Wave-2 participants from three sources: directly via ads on Facebook, Twitter, and Instagram; by redirecting qualifying conservatives or Democratic-leaning Independents who started but were ineligible for the Wave-1 survey; and from Qualtrics, which aggregates respondents from partnering market-research panels and online samples. Participants recruited from each source were subject to the same screening criteria on demographics, political affiliation, and climate beliefs (Section 2.3). In total, 8,685 participants were randomized to a Wave-2 treatment, of which 3,002 lean towards the Republican party and 5,683 lean towards the Democratic party. We recruited nearly all conservatives in our sample from Qualtrics, while we recruited about 85% of liberals from social media. Appendix Section B.3.1 describes Wave-2 recruitment in more detail.

Attrition: About 92% of participants randomized to a Wave-2 treatment arm advance through sending an email to Congress or not, with no differential attrition by treatment (column 1, Appendix Table B1). We define the Wave-2 experimental sample as the 7,981 participants for whom we observe email choices.

Comparing Wave-2 liberals and conservatives: Columns 1 and 2 of Table 1 present summary statistics for liberals and conservatives in the Wave-2 experimental sample, respectively. Liberals and conservatives in the Wave-2 sample differ notably both in demographics and in political engagement and beliefs. On demographics, liberals are older, more likely to live in state groups that we classify as blue or purple, more educated, and wealthier than conservative Wave-2 participants. On politics, liberals report higher baseline engagement in activities like contacting politicians and signing petitions,[16] worry about climate change, desire for additional government action on climate change, and perceived local climate impacts than do conservatives. (See Appendix Figure B10 for the distributions of these variables separately among liberals and conservatives). Finally, Wave-2 liberals report a stronger preference for friends of their own party than do Wave-2 conservatives, so they may be more affectively polarized.[17] Section 8.1 shows that the differential impacts of Democrats' invitations on Wave-2 liberals and conservatives cannot be explained by basic demographic gaps between these groups.

Balance: The Wave-2 sample is balanced across treatment arms both among liberals and conservatives (Columns 3 through 8 of Table 1).

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

[17]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 B2), 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.

4.1.3 Survey attention

Wave-2 participants pay close attention to the information they receive about Wave-1 matches. We elicited all comprehension measures at the end of the survey among a randomly-chosen half of participants.[18] See Appendix Section B.3.3 for detail on these measures.

First, treatment participants are much more likely to believe that their Wave-1 match emailed Congress when they took our survey. Participants who saw an invitation are 60pp more likely to state that their paired Wave-1 participant emailed Congress (Panel A, Appendix Figure B11). Even when treatment participants state that they don't know whether their paired Wave-1 participant emailed Congress, they guess that their match did so with higher probability than do control participants (Panel B, Appendix Figure B11). Second, Wave-2 participants also attend to their paired matches' demographics. In a series of multiple-choice questions at the end of the survey, participants correctly recall matches' political leanings, gender, age, and state group of residence at least 85% of the time (Panel A, Appendix Figure B12). Then, Wave-2 participants may react differentially to invitations by Wave-1 inviters' traits.

4.2 Empirical specifications

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

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

where $Contact_i$ indicates that participant i emails Congress, $Treatment_i$ is an indicator for being assigned to see a Wave-1 invitation, and X_i is a vector of control variables. Here, β is our primary coefficient of interest. We describe the outcome and control variables in more detail below.

Primary outcome variables for emailing Congress: Our primary Wave-2 outcome variable is whether participants match to an email record to Congress sent via the form embedded in our experimental survey. We match participants to email records by first name and email address, their treatment status—since we embedded separate forms by treatment arms—and the timing of their survey completion. Note that we often cannot simply match on email alone because participants may use different emails when consenting to the survey and emailing Congress. In total, we successfully merge 91% of all recorded emails sent via the embedded Wave-2 forms to Wave-2 participants, with no differential success by treatment group. Appendix Section B.4.1 describes the match process in detail.

In addition to our main treatment effects on whether participants match to an email record, we also estimate effects on whether participants initially opt in to the process of emailing Congress via our form. Across the full Wave-2 sample, only 54% of those who initially opt into the email process ultimately match to an email record.[19]

[18]Participants are only included in our final sample if they successfully pass a simple attention check in which we explicitly ask them to choose a particular option in a multiple-choice question. Participants recruited via Qualtrics answered this attention check before randomization and were excluded from randomization if they failed it. In contrast, participants recruited via social media answered the attention check after randomization and were excluded at that point if they failed it.

[19]Participants who opt into the email process may drop out at several stages. First, 16% of those who initially express interest in emailing Congress leave the process after we warn them that the form will ask for their address. Second, participants who expressed

Wave-2 control variables: Our main Wave-2 specifications control for participants' recruitment source, demographics, baseline beliefs about politics and climate change, and baseline political engagement. Appendix Section B.4.2 describes these control variables in detail, and Section 8.1 shows that our main Wave-2 results are fully robust to which controls we include.

Our Wave-2 demographic controls include participants' gender, age in 5-year bins, state of residence, income bin, educational attainment, and whether they identify as Hispanic or not. Our controls for baseline climate and politics beliefs include standardized measures of participants' stated worry about climate change, desire for additional government action, perception of the impacts of climate change in their local area, an index of beliefs about the extent to which the government responds to citizen advocacy, and indicators that participants identify as members of the Republican party, Republican-leaning Independents, Democratic-leaning Independents, or members of the Democratic party. Finally, we control for participants' baseline political engagement with a standardized index summing over whether participants have undertaken a series of political actions in the last two years, such as emailing, writing letters to, or phoning their political representatives or phone-banking for a political or social issue.

4.3 Results: Invitations increase Wave-2 action

Invitations from Wave-1 participants increase both Wave-2 liberals' and conservatives' political action (Table 2). Pooling across political affiliations, seeing an invitation makes Wave-2 participants 5.7pp (16%) more likely to opt into the email process and 4.4pp (24%) more likely to match to an email record.[20]

Larger absolute impacts among liberals: While invitations increase action both among Wave-2 liberals and conservatives, their absolute impacts are larger among liberals. About 43% of liberals in the Wave-2 control group start the process of emailing Congress and 25% ultimately match to email records (columns 2 and 5); seeing an invitation increases these rate by 6.9 pp (16%) and 5.8pp (23%), respectively. Conservatives in the Wave-2 sample have substantially lower baseline rates of action—about 24% start the process of emailing Congress and only 8% ultimately match to an email record—but also react strongly to invitations from Wave-1 participants. Invitations make conservatives 4.4 pp (18%) and 2.1 pp (25%) more likely to start the process of emailing Congress and match to an email record, respectively (columns 3 and 6).[21]

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

interest in emailing Congress at both elicitations may simply not follow through with sending an email once they reach the form itself, or they may not match to an email record because their letter used a different name or email address: 36% of those who say they want to email Congress at the second elicitation do not match to an email record. Appendix Section B.2 lays out the survey flow for the full process of emailing Congress.

[20] In Appendix Section B.5, we explore the mechanisms by which invitations increase action among liberals and conservatives. Seeing an invitation from an earlier participant increases participants' reports of how worthwhile it is to email Congress and shifts up their beliefs about the share of other study participants who emailed Congress, with similar patterns among liberals and conservatives. Simple mediation analysis controlling in turn for each of these intermediate outcomes suggests that invitations' impacts on action can be primarily explained by their impacts on beliefs about others' action, consistent with the idea that invitations work by changing participants' perceived norms of political action in our context.

[21] Appendix Table B6 show that the treatment effects on Democrats are statistically and economically similar among those recruited from Qualtrics and social media. While the effects of invitations on whether conservatives start the email process is substantially larger among those recruited from social media (N = 163) than those recruited from Qualtrics (N = 2791), we do not interpret this small-sample result. All future Wave-2 analysis will pool respondents recruited from the two sources.

matter for the "effective-altruist" value of inviting others to join in climate action (see footnote 13). We can reject (p = 0.023) that invitations have the same absolute impact on whether liberals and conservatives match to email records. Section 8.1 below shows that these and the following Wave-2 results are highly robust to changes in sample restrictions, controls, and corrections for experimenter demand effects.

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

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

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

The role of affective polarization: Affective polarization may make invitations substantially less effective among conservatives (Appendix Figure B17, Appendix Section B.6.4). Our point estimates suggest that invitations from Wave-1 Democrats are at most half as effective among Wave-2 conservatives with above-median versus below-median affective polarization, though we are under-powered to statistically reject homogeneous effects in most cases. As expected, there is no consistent heterogeneity in invitations' impacts by polarization among liberals, who are interacting with invitations from co-partisans.

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 Section B.6.2). While invitations increase action by an additional 3.8pp when Wave-2 recipients are liberal—and thus match senders' political affiliation—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 (column 4, Appendix Table **??**). This coefficient is statistically indistinguishable from zero. Increasing a continuous measure of Wave-2 participants' perceived non-political similarity to their Wave-1 match by one standard deviation increases the invitations' impacts by a statistically-significant 1.8pp (column 3, Appendix Table **??**).[22]

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

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

5.1 Experimental design, recruitment, and fidelity

5.1.1 Experimental structure

The Wave-1 action experiment tests whether Democrats are more likely to email Congress when doing so allows them to invite others to join them. In particular, we randomly assign a subset of Wave-1 Democrats to know that if they email Congress, we will pass on invitations like those in Figure 2b to Wave-2 participants on their behalf. To separate influence motives from any social- or self-image concerns from simply telling others that one took action, we randomize other Wave-1 participants to know that if they email Congress, we will tell Wave-2 participants that they did so *after* those targets decide themselves whether to email Congress or not. This design maps closely to the conceptual logic laid out in Section 3.2, and Appendix Figure C1 summarizes the experimental structure.

Told that Wave-2 matches will see demographic profile: Wave-1 participants begin their survey by building a basic demographic profile (Figure 2a). After showing each Wave-1 participants their own profile, we say that this profile will be shown to up to 10 future Wave-2 participants in a particular demographic group to give those future participants a sense of who else is participating in the study. (See Appendix Figure C2 for the explanation to participants.) As we describe in Section 2.3, Wave-1 participants in a given demographic cell are randomly paired with either liberals or conservatives in a matched Wave-2 demographic cell.

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

Treatment variations to identify influence motives: Next, we show all Wave-1 participants a preview of the upcoming opportunity to email Congress. (See Appendix B.2 for details.) We then randomize participants to treatment variations that cleanly identify whether they try to influence others' political action.

First, we randomize some participants into a pure control group (denoted A0): these participants are told nothing about whether their choice to email Congress will be reported to any future participants. Next,

[22] Appendix B.6.2 details how we estimate Wave-2 participants' perceived non-political similarity to their Wave-1 matches.

we randomize other participants into an **"Invitation" group (denoted A2)**: they are told that if they email Congress, the same Wave-2 participants who will see their basic demographic profile will also see a profile saying that they emailed Congress and inviting them to join in doing so (Figure 2b); Wave-2 targets would see this invitation **before** deciding whether to email Congress or not. If the Wave-1 participant does not email Congress, their Wave-2 matches would see nothing about whether they or any others did so.

While comparing email rates between the A2 and A0 groups would answer the policy-relevant question of how opportunities to influence others affect action, this effect combines influence motives and any social- or self-image benefits of telling others that one emailed Congress (see the framework in Section 3). To isolate whether Democrats try to mobilize Wave-2 participants' action, we randomize others to the **"Tell-after" group (denoted A1)**: they are told that if they email Congress, their paired Wave-2 participants would see a profile saying that they emailed Congress (Figure 2c), but only **after** deciding whether to do so themselves. The extra profiles passed on from A1 participants do not include a statement explicitly inviting the Wave-2 participants to join them in action.[23] Then, the only difference between the A1 (Tell after) and A2 (Invitation) groups is that A2 participants' choice to email Congress or not can affect whether their paired Wave-2 participants do so as well, and comparing email rates between these groups identifies whether Wave-1 Democrats act to inspire others.[24]

Ensuring treatment comprehension: To ensure that Wave-1 participants in the A1 and A2 groups understand the experimental set-up, we show them flowcharts indicating when their paired Wave-2 matches would see their extra profile (Appendix Section C.1.1). Finally, we ask and correct participants' answers to incentivized comprehension questions on what their paired participants will see if they email Congress, when these participants will see this extra profile, and what the future participants will see if they do not email Congress (Appendix Section C.1.2). We make these questions as subtle as possible by emphasizing that we ask them to ensure that participants know how their basic information will be used in future surveys.

Choose whether to email Congress or not: All Wave-1 participants then decide whether to email Congress or not, following the same process as Wave-2 participants detailed in Appendix Section B.2.

5.1.2 Wave-1 sample recruitment and summary

Recruitment: We recruit Wave-1 participants using ads on Facebook, Instagram, and Twitter; these ads explicitly recruit for a survey on climate change or environmental issues, allowing us to attract a sample of engaged citizens who well approximate the mainstream climate movement. In total, 29,596 unique participants consented to the survey. Of these, 12,755 participants met our demographic screening criteria, agreed that climate change is mostly human-caused, and identified as members of the Democratic party. We randomized 8,937 participants into the Wave-1 experimental sample and randomized an additional 2,004 participants into parallel experimental samples described in Sections 6 and 7 below. Appendix Section C.2.1 describes this recruitment process in detail.

[23] As in A2, A1 participants know that if they do not email Congress, their Wave-2 matches would be told nothing about whether they or other participants emailed Congress. Appendix Section C.1.1 lays out the text of the A1 and A2 treatment variations.

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

Attrition: While the A0 control group advances immediately from randomization to answering whether they want to email Congress, the A1 and A2 treatment groups go through several pages of explanation and comprehension questions between randomization and deciding to email Congress or not. Thus, we directly observe whether only 87% of the A1 and A2 groups email Congress, compared to 99% of the A0 group (Appendix Table C1, Panel A column 1). There is no differential attrition between the A1 and A2 groups.

In our main analysis, we make the conservative assumption that participants who leave the survey between learning about the upcoming opportunity to email Congress and deciding whether to do so or not would not have emailed Congress had they continued.[25] Participants see the email preview before randomization, so this assumption allows us to define whether all randomized participants emailed Congress. All of our main Wave-1 conclusions are robust to restricting the sample to participants for whom we can directly observe choices to email Congress or not (Section 8.2).

Sample description: The experimental sample of Wave-1 Democrats well approximates the mainstream environmental movement (column 1, Table 3). This sample is quite politically engaged: 73% of participants report that they've contacted elected representatives about a political or social issue in the last two years, while in 2018 only 40% of Americans reported having contacted an elected official in the last 5 years (Oliphant, 2018). Next, Wave-1 participants are highly educated and wealthy: over 80% have a 4-year college degree or more, and 51% and 15% have total pre-tax household income over $100,000 and $200,000, respectively. Recall, in addition, that the Wave-1 sample is fully white by construction (Section 2.3). While our sample therefore cannot represent the full mainstream environmental movement, it does reflect a movement that remains predominately white, affluent, and educated.[26]

Balance: Columns 2 through 6 of Table 3 show that the sample is largely balanced on baseline characteristics across treatment groups.[27] There are several small imbalances on age categories and some forms of political engagement, but our results are fully robust to the baseline controls we include (Section 8.2).

[25]The attrition patterns support this assumption. A1 and A2 participants who drop out of the survey before deciding to email Congress or not are more likely to identify as men, are older, are more likely to live in a purple state, are less likely to have a 4-year college degree, and report substantially lower past political participation than those who remain (Appendix Table C1, Panel B). Nearly all of these baseline traits, especially past political participation, predict lower likelihood of emailing Congress in the A0 control group (Appendix Table C2).

[26]In recent years, commentators on the mainstream US environmental movement have increasingly focused on its longstanding dominance by white and educated members of the middle-class (Jones 2020, Ortiz 2021), even as evidence shows that racial minorities and lower-income communities disproportionately bear the burdens of environmental harms (e.g. Jbaily et al. 2022, EPAP 2021) and may report higher environmental concern than whites (Pearson et al. 2018). Beginning in the late 1960s, several studies document that environmental activists were overwhelming white and disproportionately likely to have a college degree (Taylor 2014). This socio-economic skew has persisted strongly over time, even as an environmental-justice movement led predominantly by people of color rose to prominence in the 1980s (Mohai et al. 2009). The Bureau of Labor Statistics found in 2015 that 3% of white Americans reported volunteering for an environmental or animal-care organization in 2015, compared to 1% of Blacks, 1.2% of Asian Americans, and 1.6% of Hispanics. This racial imbalance extends to the leadership of the mainstream environmental movement; in 2014, ethnic minorities filled only 16% of general staff and 12% of leadership positions in government environmental agencies and mainstream environmental organizations (Taylor 2014).

[27]The sample is also largely balanced across treatment arms when split by whether Wave-1 participants are paired with liberal or conservative members of their matched Wave-2 demographic group. These results are available upon request.

Table 3: Wave-1 sample summary and balance

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

Note: This table summarizes and tests for balance within the Wave-1 experimental sample, which we define as all those randomized to a Wave-1 treatment group. Column 1 presents means in the full sample on a range of baseline traits, and column 2 presents means among participants assigned to the A0 pure control group. Appendix B.4.2 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.

5.1.3 Set-up comprehension and attention

Most Wave-1 participants understand whether their action can influence others, providing a sizable implicit first stage on influence beliefs (Appendix Figures C8 and C9); see Appendix C.1.2 for details on how we elicit comprehension. Over 85% of respondents in both the A1 and A2 groups correctly state that their matched Wave-2 participants would be told if they email Congress (Appendix Figure C8), and participants generally attend to *when* Wave-2 matches would be told that they emailed Congress (Panel A of Appendix Figure C9). Before correcting their answers, A2 participants are 56 pp more likely than A1 participants to say that future participants will see that they emailed Congress before deciding whether to do the same. After we correct their initial answers, this gap rises to 76 pp. Finally, A2 participants are 51 pp more likely than A1 participants to answer at the end of the survey that their choice to email Congress or not could in theory affect whether their paired Wave-2 participants do the same (Panel B of Appendix Figure C9).

Attention to Wave-2 participants' demographics: Wave-1 participants also attend to their paired Wave-2 matches' demographics, so they may differentially seek to influence Wave-2 participants with certain traits. In multiple-choice questions at the end of the Wave-1 survey, about 75% or more of both A1 and A2 participants correctly identify their Wave-2 matches' political leanings, gender, age, education, and state group (Panel B of Appendix Figure B12).

5.2 Empirical specifications

Our Wave-1 analysis rests on the following simple regression:

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

where $Tell_i$ indicates that participant i is in group A1 or A2, $Invite_i$ indicates that participant i is in group A2, and X_i is a vector of control variables.[28] As in Wave 2, the primary outcome variable throughout our Wave-1 analysis is whether each participant matches to a record for an email sent via our form. In total, we successfully merge 94% of all recorded emails sent by Wave-1 participants to individuals. We also present impacts on whether participants initially opt into the email process.

Here, β_2 captures the effects of knowing that up to 10 future participants will be told that one emailed Congress: these effects could be driven by any social- or self-image benefits, as well as any efforts to affect Wave-2 participants' action after the survey (Section 3). The β_3 coefficient isolates attempts to influence Wave-2 participants' political action during the survey: are Democrats more or less likely to email Congress via our form when doing so can affect whether others do the same?

5.3 Results: Democrats act to mobilize others

5.3.1 Impacts of opportunities to invite others

Opportunities to invite others to join in emailing Congress substantially increase rates of climate advocacy. First comparing just the A2 Invitation and A0 control groups (column 1, Table 4), A2 participants are 16pp more likely to initially opt into the email process and 16pp more likely to ultimately match to an email record, increases of 34% and 48% over the A0 control means.

[28] Our main regression specifications include the same baseline control variables as in Wave 2 (see Section 4.2), and Section 8.2 shows that our results are fully robust to varying these controls.

Table 4: Main Wave-1 results

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

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

Separating out influence motives: About one-fifth of the total effect of knowing that future participants will see your action *before* deciding whether to act (column 1) can be attributed to participants' efforts to influence whether those participants email Congress during the survey itself. To isolate these motives, Column 2 of Table 4 incorporates the A1 Tell-after arm and estimates equation 3. Participants who know that future participants will be told if they email Congress *before* deciding whether to do the same rather than *after* (i.e. assigned to the A2 Invitation versus A1 Tell-after groups) are 3.3 pp more likely to start the email process (Panel A of Table 4, column 2) and 2.7 pp more likely to have an email record (Panel B of Table 4, column 2). These "influence motive" effects amount to 7% and 9% of the relevant A0 control means, respectively.[29]

The remaining four-fifths of the total A2 effect arise from a combination of self- and social-image concerns and attempts to influence what paired participants do after the survey. Being assigned to the A1 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 (Panels A and B of Table 4, column 2).

Though we cannot decompose the A1 effect into the role of social- or self-image and attempts to influence targets' post-survey behavior, our evidence suggests that influence motives may play a role in this large effect. About 54% and 48% of participants in the A1 and A2 groups correctly answer that their decision to email Congress could in theory affect paired future participants' behavior after the survey, with nearly all other participants answering that they do not know if it could (Appendix Figure C10, Panel A). Furthermore, when asked how likely it is that their decision to email Congress or not would affect what paired participants do after the survey, about 60% of A1 and A2 participants report at least 4 on a scale from 1 (Not at all likely) to 7 (Extremely likely) (Appendix Figure C10, Panel B). Of course, self- or social-image concerns are also likely to be strong drivers in this setting, as in other political and non-political domains (Bursztyn and Jensen, 2017; Dellavigna et al., 2017).

5.3.2 Democrats may act to influence liberals more than conservatives

We find suggestive evidence that Democrats differentially act to mobilize liberal Wave-2 participants; we will explore *who* Democrats try to recruit in more detail in Section 6 below.

Partisan gaps in the total A2 Invitation effect: Democrats respond less overall to opportunities to invite Wave-2 conservatives than to opportunities to invite Wave-2 liberals (columns 3 and 5, Table 4). Among Democrats randomly paired with Wave-2 liberals (column 3), A2 Invitation participants are 18pp (40%) more likely to opt into the email process and 18pp (56%) more likely to match to an email record than those in the A0 control group. In contrast, the A2 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 they are paired with Wave-2 conservatives (column 5). We reject ($p = 0.03$) that the overall impacts of A2 on opting into the email process are equal across those paired with liberals versus conservatives.

[29] The A2 versus A1 variation could affect action through a mechanism other than influence beliefs if participants expect many or all other survey-takers to face the same information structure—i.e. to also have opportunities to influence others' action—and revise their beliefs about aggregate participation in emailing Congress via our survey in response. As we describe in Appendix B.5, the impacts on action of any such belief updating are theoretically ambiguous (Miller and Prentice, 2016; Gerber et al., 2018; Rogers et al., 2018; Cantoni et al., 2019; Hager et al., 2022). However, A2 has no differential effect relative to A1 on participants' incentivized beliefs for how many out of 100 people who took the survey chose to email Congress via our form (column 1, Appendix Table C3).

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

Moreover, liberals in our sample may differentially act to motivate other liberals, rather than conservatives, to email Congress during the survey. 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 liberals do the same, 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 conservatives. We cannot reject that these coefficients are equal, but they suggest that liberals may differentially act to build the climate movement within, rather than across, party lines.[30] Section 6 tests these partisan gaps with higher power.

5.3.3 Evidence for influence motives: Theory-predicted heterogeneity

The impacts of the A2 Invitation treatment (relative to the A1 Tell-after group) are positively correlated with several Wave-1 traits that should correlate with stronger influence motives (Section 3.2); these correlations help to validate that the A2-A1 treatment gap indeed captures Democrats' efforts to mobilize others.

Heterogeneity by climate worry: Our framework predicts that influence motives are stronger among those who care more about climate change and thus more highly value future participants' emails to Congress. Early in the Wave-1 survey, we ask participants how worried they are about climate change, on a scale from 1 (Not at all worried) through 7 (Extremely worried). Our point estimates for influence motives are substantially larger among Democrats with above-median climate worry, and we reject equal effects on opting into the email process at the 10% level (Figure C11).

Suggestive heterogeneity by influence beliefs: The framework in Section 3 also predicts that influence motives are stronger among participants who estimate that ΔP, the impacts of invitations on Wave-2 participants' action, is larger. At the end of the survey, we randomize one-third of A1 and A2 participants to estimate a fictional future participants' likelihood of emailing Congress during our survey if they do or do not see an invitation from the Wave-1 participant.[31] While our estimates are imprecise, they suggest that influence motives are stronger among those with stronger influence beliefs (Figure C11).

[30] In contrast to these patterns by partisanship, we do not find consistent evidence that Democrats differentially act to influence Wave-2 targets who match them in non-political ways (Appendix Figure C11 and Appendix Table **??**); in fact, Democrats may differentially avoid influencing targets from the same state group. Pooling across all non-political demographic dimensions, we find zero heterogeneity in either the A1 versus A0 gap or the A2 versus A1 gap when Wave-1 participants and their Wave-2 matches share an additional non-political demographic trait or have higher predicted non-political similarity. We also find no evidence that Wave-1 Democrats differentially respond to opportunities to invite Wave-2 participants with particular non-demographic traits overall (Appendix Figure C11), though all of this heterogeneity analysis is under-powered.

[31] One concern with this measure is that it may itself be affected by treatment status through motivated reasoning. However, we find no evidence for such effects (Appendix Table C3, column 2).

6 The WTP experiment: Who do inframarginal activists invite?

While the Wave-1 action experiment shows that Democrats act to build the climate movement, it is not designed to look in detail at *who* these citizens seek to recruit to the climate coalition. This section describes a sub-experiment embedded in the Wave-1 A0 pure control group, called the Willingness-to-pay (WTP) experiment, that tests whom Democrats invite into the climate movement.[32] We find that Democrats differentially seek to mobilize other liberals, and that these partisan influence gaps dwarf attempts to mobilize political action by state or any other match traits.

6.1 Experimental design

We start the WTP experiment by telling participants that we will soon show them a list of 20 past study participants who we will be recontacting to take a second short survey.[33] (Appendix D.1.1 details our explanation of the set-up to participants.) Each WTP participant will be randomly paired with one of these 20 participants, who will see a profile of the WTP participant when they return for the follow-up survey.

Choosing whether to say that one emailed Congress: For each of the 20 possible matches to whom each WTP participant could be paired, we ask them to choose between the following two options:

- *Option 1. Basic profile and carbon offsets:* We show the returning match the WTP participant's basic demographic profile (Figure 2a) 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.[34]

- *Option 2. Extended profile:* We show the returning match an extended profile saying that the WTP participant emailed Congress and, in some cases, inviting them to join in action.[35]

Thus, participants face a tradeoff in each choice between carbon offsets and showing their match that they emailed Congress.

Identifying influence motives by randomizing profile timing: As in the Wave-1 action experiment, we then identify influence motives by inducing variation in when possible matches would see that the WTP participant emailed Congress. Within participant, we randomize whether each possible match would see their profile *before* or *after* themselves choosing whether to email Congress, motivating this variation by

[32]The WTP experiment also identifies influence motives in a different population than does the Wave-1 action experiment. While the Wave-1 action experiment tests whether influence opportunities can *induce* Democrats to act, the WTP experiment identifies influence motives among those who email Congress in the A0 control group. These activists are "inframarginal" with respect to influence motives: they chose to email Congress when they had no opportunity to tell others.

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

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

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

telling WTP participants that matches would see their profiles at different times based on the "structure of the survey they're signed up to complete." A match seeing an extended profile *before* deciding to email Congress would see the profile shown in Figure 2b, while one assigned to see the extended profile *after* deciding would see the profile shown in Figure 2c, which does not include an invitation to join in action.

Then, we identify whether WTP participants value recruiting others to email Congress by testing whether they are more likely to make the costly choice to tell possible matches that they emailed Congress when doing so can affect whether those matches email Congress themselves.

Variation in possible matches: The 20 matches to whom WTP participants can be paired are all real people who vary in their demographics and political leanings; we construct the match groups so that each WTP participant sees 10 conservative and 10 liberal possible matches. Participants see a demographic profile of each possible match (Appendix D.1.2). These profiles include an additional attribute, denoted "Status," where matches 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. With 20 binary choices per participant that vary in profile timing and match traits, this set-up powers us to look in detail at *who* Democrats try to recruit for climate action.

Varying climate-belief information: Finally, we randomize WTP participants either to see profiles of possible matches that say they believe climate change is human-caused or that include no information about their climate beliefs (Appendix D.1.2). This cross-participant variation lets us test how establishing this common-ground belief affects any gap in Democrats' attempts to recruit other liberals versus conservatives.

6.2 WTP sample recruitment and experimental fidelity

Recruitment: We recruit WTP participants from those in the Wave-1 A0 pure control group who report that they emailed Congress during our survey.[36] In total, 1,350 participants begin the main WTP survey.

Attrition: Just before participants begin making choices for each of their 20 possible matches, we randomize the 1,109 participants who remain to either see profiles that include matches' climate beliefs or not. A total of 1,023 participants (76% of those who start the experiment and 92% of those randomized) complete profile choices for all 20 possible returning participants, with no differential attrition by treatment arm (column 2, Appendix Table D4). These 1,023 participants compose our sample for analysis.

Sample description and balance: Relative to the full Wave-1 sample, participants who ultimately email Congress and complete the WTP survey are more likely to be women, are somewhat more concerned about climate change, and have higher baseline political engagement (columns 1 and 2, Appendix Table D5). The WTP sample is largely balanced between those who see information on matches' climate beliefs and those who do not (columns 3 through 5, Appendix Table D5). Our main analysis includes participant fixed effects, controlling for any remaining baseline differences.

Set-up comprehension: We invest heavily in making sure that participants understand the WTP experimental set-up. After describing the set-up once, we go through it again while asking a series of 8 incentivized

[36] Appendix Section D.2 details WTP recruitment. We also recruited some participants to take a survey equivalent to that completed by A0 participants, but who were not included in Wave-1 and instead funneled directly into the WTP experiment.

comprehension questions and correcting participants' answers (Appendix D.3). Participants answer an average of 5.8 comprehension questions correctly, and Appendix Figure D17 plots the distribution of number of questions that participants initially answered correctly; note that we reiterate the correct answers immediately after each question. Our results are robust to restricting to participants who initially answered at least half of the 8 comprehension questions correctly (Section 8.3).

6.3 Empirical specifications

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

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

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

Here, β_1 captures whether Democrats in the WTP sample try to influence other liberals: whether they are more or less likely to tell a liberal match that they emailed Congress when it can affect whether that participant does the same. β_2 captures whether Democrats are more or less likely to show a conservative, rather than a liberal, an extended profile when it cannot affect their in-survey action. Finally, β_3 captures whether WTP participants differentially try to influence matches who are conservative, rather than liberal.

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

6.4 WTP Results: Democrats differentially try to mobilize other liberals

Overall influence motives in the WTP set-up: WTP participants strongly value opportunities to invite others to join in emailing Congress. Overall, participants choose to pass on extended profiles about 23% of the time when matches would see them after deciding whether to email Congress or not. When matches would see their profile *before* deciding whether to email Congress or not, on the other hand, WTP participants are 42 pp and 48 pp more likely to choose to show they emailed Congress, without and with climate-belief information, respectively (columns 1 and 3, Table 5).[37]

The partisan influence gap: WTP participants are less likely to try to influence conservatives than liberals. When they don't know that all possible matches believe climate change is human-caused (column 2), WTP participants are 30 pp more likely to show a conservative an extended profile when it can affect their in-survey action; this "influence effect" is 24 pp (81%) higher when the Before match is liberal. The partisan

[37] One key question in interpreting this result is whether WTP participants value carbon offsets. We randomize 132 participants to choose between passing on a basic demographic profile plus a carbon-offset donation, passing on an extended profile, and passing on a basic demographic profile plus receiving a take-home gift card that could be redeemed at hundreds of stores (Appendix D.4). 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 only 1.2 choices; about 80% of participants never chose the take-home gift card. Thus, most participants value carbon offsets more highly than the same amount in a take-home giftcard.

Table 5: Main WTP results

	(1)	(2)	(3)	(4)
	Showed extended profile			
	No info		Has info	
Before	0.416***	0.296***	0.481***	0.424***
	(0.020)	(0.021)	(0.019)	(0.020)
Liberal		0.135***		0.037***
		(0.016)		(0.014)
Before * Liberal		0.239***		0.115***
		(0.019)		(0.015)
Mean: After Dem + Rep	0.233		0.225	
Mean: After Rep		0.165		0.206
Num. participants	475		548	
Num. choices	9500	9500	10960	10960

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

gap in influence effects falls when participants know that all possible matches believe climate change is human-caused, but the influence effect remains 11.5 pp (27%) larger for liberal than conservative matches (column 4).

Alongside these gaps in attempts to influence liberals versus conservatives, Democrats are more likely to tell liberal matches that they emailed Congress even when it cannot affect those matches' action during the survey. When WTP participants do not know that all matches believe climate change is human-caused, they are 13.5 pp (82%) more likely to choose an extended profile for an After match when that match is liberal, rather than conservative. This partisan gap falls to 3.7pp (18%) when climate beliefs are revealed. These partisan gaps could arise from differential social-image concerns, efforts to affect matches' action after the survey, or the affective benefits of interaction.

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

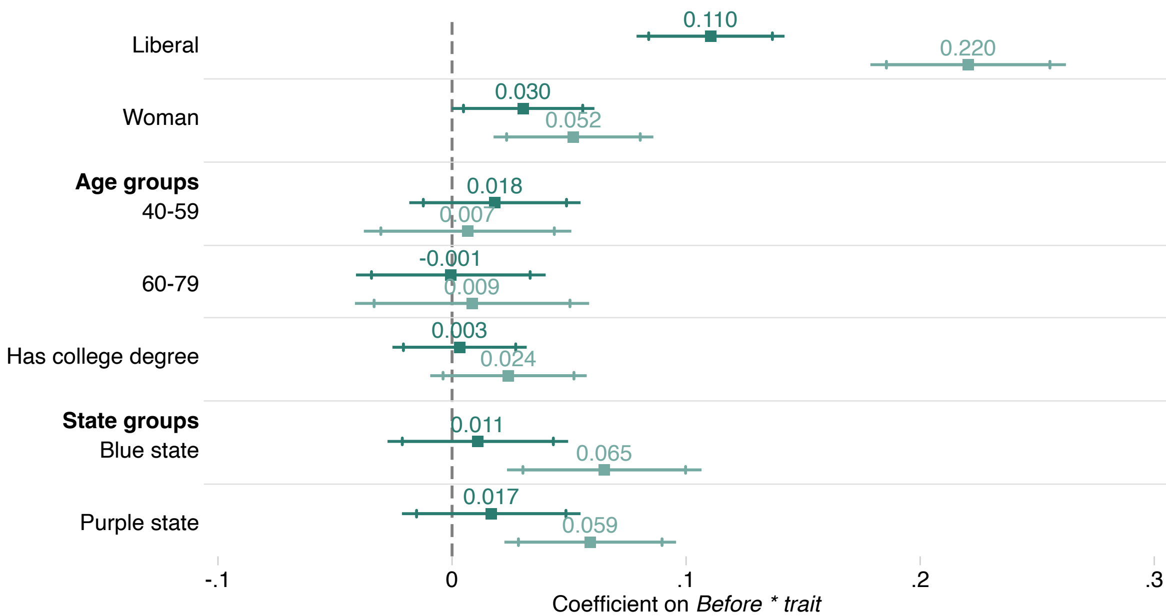
Figure 4). When WTP participants lack information on possible matches' climate beliefs, they more strongly try to influence participants who are women or live in blue or purple states, and our point estimates suggest that they may differentially try to mobilize those with 4-year college degrees. However, these gaps are much smaller in magnitude than participants' differential efforts to mobilize liberals. Informing participants that all possible matches believe climate change is mostly human-caused eliminates or attenuates all of these differentials, consistent with the idea that participants may use possible targets' demographics as signals of their likely stances on climate change. Indeed, each of these traits is associated across with US with stronger beliefs in anthropogenic climate change (Hornsey et al., 2016; Marlon et al., 2022). Notably, there is no evidence that Democrats differentially try to mobilize climate advocacy in red or purple states, where this advocacy might be more impactful (Appendix Section A.3).

Politics dwarfs heterogeneity by non-political similarity: WTP participants also respond much more to political similarity than to similarity on any other observable measure (Panel B, Figure 4). When WTP participants lack information on matches' climate beliefs, they are 6-7pp more likely to try to influence matches with whom they share state or gender. Overall, participants are about 3pp more likely to try to influence matches with whom they share an additional non-political trait or score 1sd higher on predicted non-political similarity.[38] While notable, these effects are much smaller than the differential influence attempts when participants match on political party. Moreover, these differential influence attempts by non-political similarity fall in magnitude and become statistically insignificant when participants have information about possible matches' climate beliefs, while differential influence attempts by partisan leaning strongly persist.

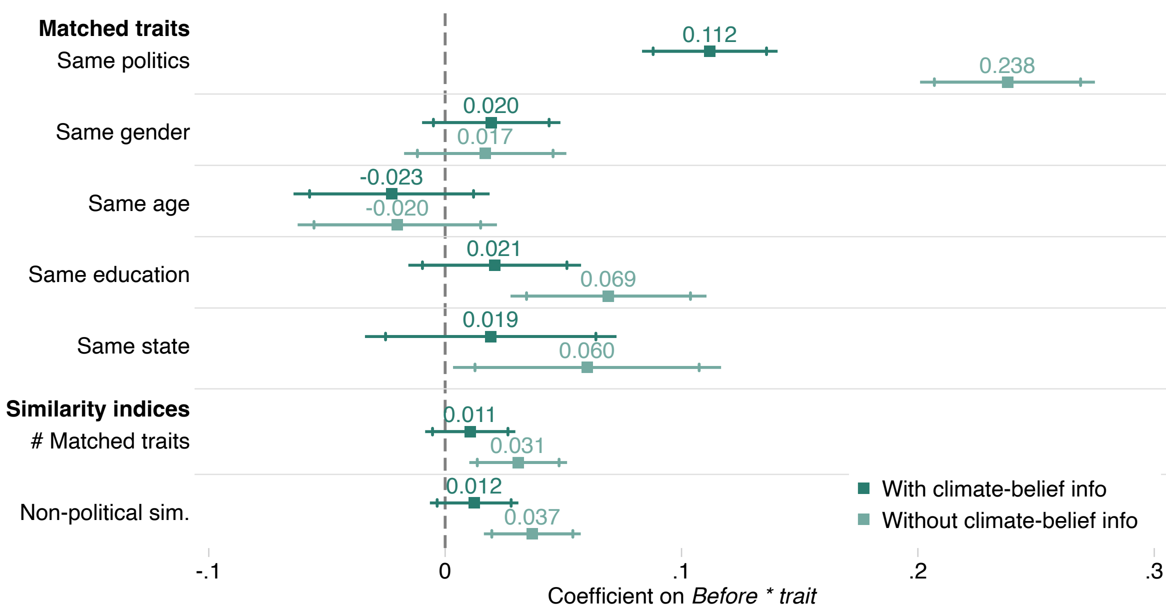
[38] We calculate this index by predicting non-political similarity from coefficients on demographic matches estimated in the full Wave-1 sample (Appendix Figure B15), subtracting predicted similarity from partisan match, and then standardizing this variable in the WTP sample.

Figure 4: Differential WTP influence attempts by other match features

Panel A. By matches' demographic traits



Panel B. By match-influencer similarity



Note: Panel A tests whether WTP participants differentially seek to mobilize possible matches with certain demographic traits. In particular, we estimate versions of equation 4 in which we interact $Before_{ij}$ with indicators that possible match j leans towards the Democratic party, identifies as a woman, is ages 40-59 or 60-79 (relative to 25-39), has a college degree, and lives in a blue or purple state group (relative to a red state group), along with main effects for each of these traits. Panel B tests whether WTP participants differentially seek to mobilize possible matches with whom they share certain traits. In particular, we estimate versions of equation 4 in which we interact $Before_{ij}$ with indicators that possible match j and WTP participant i have the same political leanings, gender, age group, educational attainment, and state group, along with main effects for each of these traits. Both panels plot the estimated coefficients on these interaction terms when we estimate them jointly in a single regression, with separate estimates for participants with and without information on possible matches' beliefs about climate change. These regressions use the same sample restrictions and control variables as in Table 5. The capped and uncapped lines denote 90% and 95% heteroskedasticity-robust confidence intervals, respectively.

7 Follow-up experiments: Decomposing partisan influence gaps

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

7.1 Accurate partisan gaps in influence beliefs: ΔP

Eliciting Democrats' influence beliefs: We first consider participants' beliefs about how much invitations affect action, ΔP. We elicit Democrats' beliefs about the impacts of their invitations on action by liberals or conservatives in a "belief sample" of 521 Democrats recruited with Wave 1. Like all Wave-1 participants, each participant in the belief sample belongs to a demographic cell that has been randomly matched to a parallel demographic cell of Wave-2 participants (Section 2.3). During the belief survey, we ask each participant to estimate how many out of 100 liberal and 100 conservative participants in their matched Wave-2 demographic group would choose to email Congress if they either did or did not see an invitation (Figure 2b) from a member of the belief participant's own demographic cell.[39] In total, 397 participants complete all four belief elicitations, of whom 194 were incentivized for accuracy. We focus throughout this section on belief elicitations from incentivized participants.[40] We then estimate the following regression:

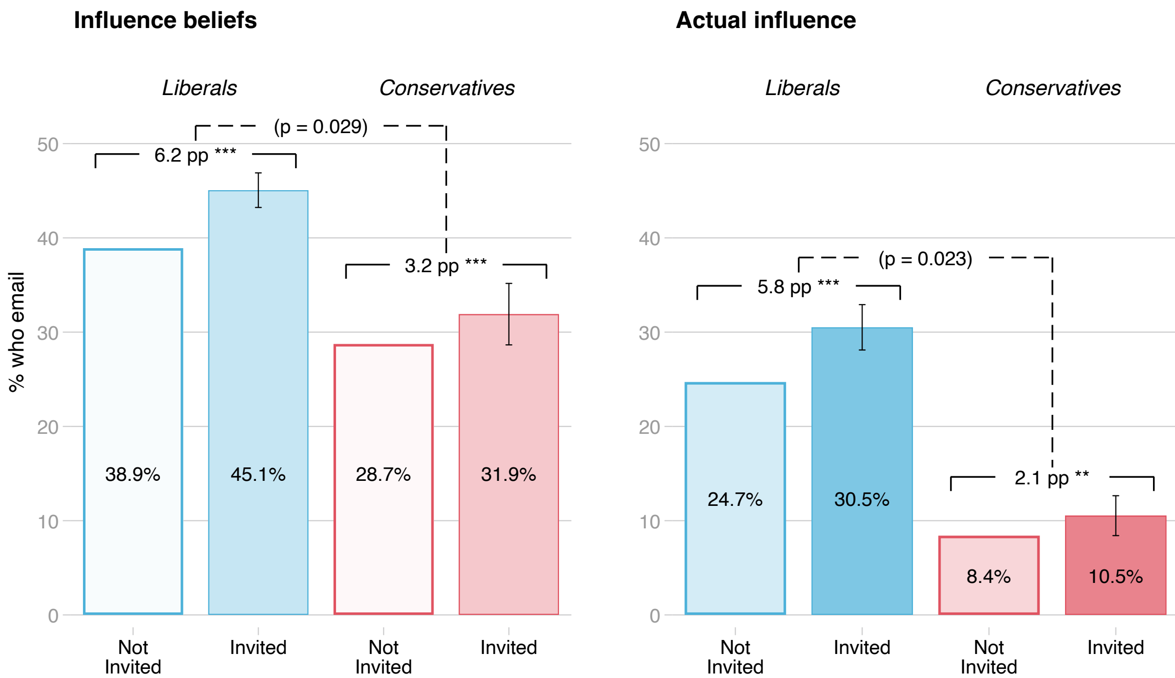
$$ProbContact_{ij} = \alpha_i + \beta_1 Profile_{ij} + \beta_2 Repub_{ij} + \beta_3 Profile_{ij} * Repub_{ij} + \Theta P_{ij} + \varepsilon_{ij} \qquad (5)$$

where $ProbContact_{ij}$ gives belief participant i's estimates of how many of 100 participants in group j would email Congress, $Profile_{ij}$ indicates that the belief elicitation is in the case where group j sees invitations from members of participant i's demographic group, and $Repub_{ij}$ indicates that group j leans towards the Republican party. Here, β_1 captures the impacts of invitations on liberals' action, while β_3 captures any differential impact on conservatives.

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

[40] Appendix Table E6 compares the demographics of this belief sample with the Wave-1 action-experiment sample and the WTP sample. Participants who complete the belief elicitations are younger, wealthier, and more likely to be men than the Wave-1 sample overall, but they match the Wave-1 sample on climate beliefs, political beliefs, and baseline political engagement. Like the main Wave-1 sample, the belief sample is less politically engaged and concerned about climate change than the WTP sample.

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



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

Democrats accurately perceive lower ΔP for conservatives: The left panel of Figure 5 plots participants' estimates of the share of Wave-2 participants who would email Congress, split by political leanings and whether they see an invitation from an earlier participant; the right panel plots the true rates of action among Wave-2 participants. This comparison reveals several key points. First, the belief sample over-estimates rates of action across all groups. On the other hand, their beliefs about the absolute impacts of invitations on Wave-2 liberals and conservatives are approximately correct. The belief sample estimates that seeing an invitation would make Wave-2 liberals and conservatives about 6.2pp and 3.2pp more likely to email Congress, respectively, while we estimate true values in the Wave-2 sample of 5.8pp and 2.1pp, respectively. We cannot reject here that the belief sample's beliefs are accurate on average.[41]

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

7.2 No partisan gap in preferences for emails with certainty

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

7.2.1 The certainty experiment: Experimental design

We run two rounds of a "certainty experiment," in which Democrats make incentivized binary choices between carbon-offset donations and emails to Congress, sent with certainty, from a range of demographic groups. While these rounds differ slightly in experimental design, their basic structure remains the same and we pool the data for analysis. In both rounds of the certainty experiment, we show participants a series of profiles for 10 to 14 demographic groups (Appendix Figure C2); these profiles are evenly split between leaning towards the Republican and Democratic parties.[42] 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 34).

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.[43] (See Appendix E.2.1 for the full text of the survey explanation.)

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

Recruitment and sample summary: In total, 459 participants and 574 participants complete all 10 choices in rounds 1 and 2 of the certainty experiment, respectively. See Appendix Section E.2.3 for more detail on the recruitment process. Like the belief sample, participants in the final certainty sample are somewhat younger and wealthier than the main Wave-1 action sample (Appendix Table E6); certainty participants are

[42]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.2.2 for more details on the process of creating these profiles and for details on the design differences between the two experimental rounds.

[43]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.

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.[44]

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

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

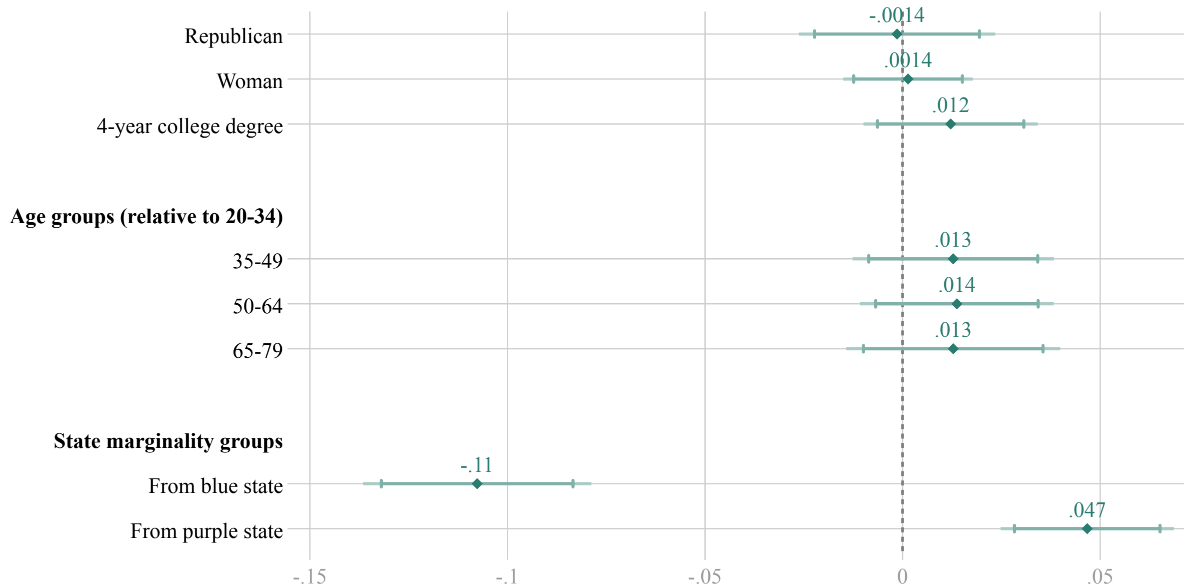
where $ChooseEmail_{ij}$ indicates that certainty participant i chooses an email from demographic group j over a carbon offset donation, α_i are participant fixed effects, and $Repub_{ij}$, $Woman_{ij}$, and $College_{ij}$ indicate that members of demographic group j lean towards the Republican party, 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.

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

Figure 6 plots our our estimated point estimates in the combined certainty sample. Democrats show strong preferences over the states in which emails are sent: they are about 11pp less likely to choose an email over offsets for senders in blue states than red states, a decrease of 17% of the mean for possible senders in red states. In turn, they are 5pp more likely to choose an email from senders in purple states than red states, an increase of about 7%. This pattern suggest that when Democrats can choose emails with certainty, they do so strategically: participants target letters to red and purple states, where they could potentially help to increase support for climate policy, relative to blue states, where past survey evidence suggests Democrats think legislators will support climate policy regardless of citizen advocacy (Appendix Section A.3).

[44] Samples in the two rounds of the certainty experiment differ somewhat in demographics, climate beliefs, and political attitudes (Appendix Table E7). 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.

Figure 6: Determinants of choosing emails with certainty



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

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 E8).

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

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

While our motivating survey suggests that Democrats expect letters from conservatives to be more impactful in achieving climate policy progress than letters from liberals in the aggregate, Democrats in our 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.[45] In Appendix Table E8, we regress these standardized impact beliefs on the set of

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

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,[46] 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.

7.3 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 experiment 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.

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.

[46] 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.

However, we find that allowing participants to hide their politics generates only a very small first stage effect on influence beliefs, ruling out this strategy.

7.3.1 Experimental design, recruitment, and fidelity

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

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

Attrition: Midway through explaining the WTP experimental set-up, we randomize the 1,278 participants who remain to either have or not have the option to hide politics. A total of 995 participants (70% of those who start the experiment and 78% of those randomized) complete all 20 binary choices, with no differential attrition by treatment arm (column 3, Appendix Table D4).

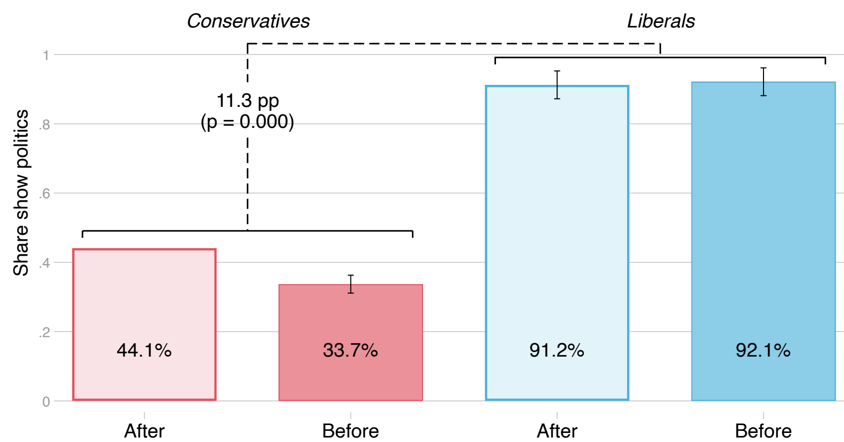
Sample description and balance: Columns 9 through 12 of Appendix Table E6 summarize the demographics of the round-2 WTP sample. Like the main WTP sample, these participants are more concerned about climate change and politically engaged than the main Wave-1 sample (column 10). The round-two WTP sample 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 E9).

7.3.2 The role of second-order affective polarization

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

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

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



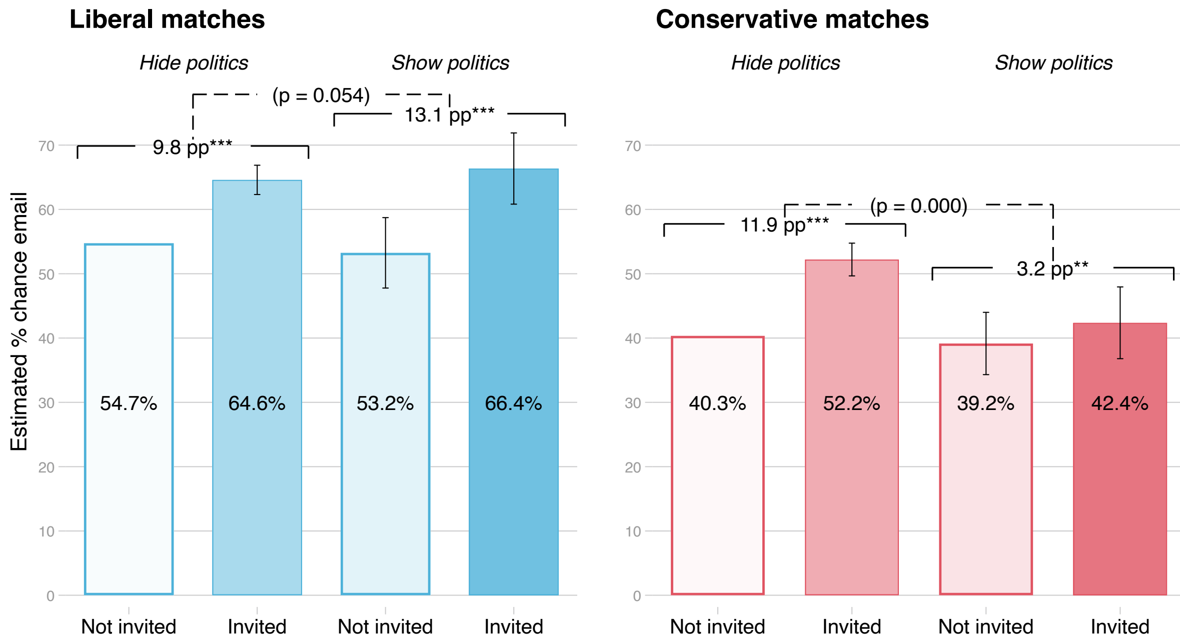
Note: This figure summarizes round-two WTP participants' choices to include their political leanings in profiles shown to possible matches; we restrict here to the 490 participants who are randomized to have the option to hide their politics, each of whom makes 20 choices. We simply regress an indicator 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.

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 2b); 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.[47]

[47] 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, ... ?

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



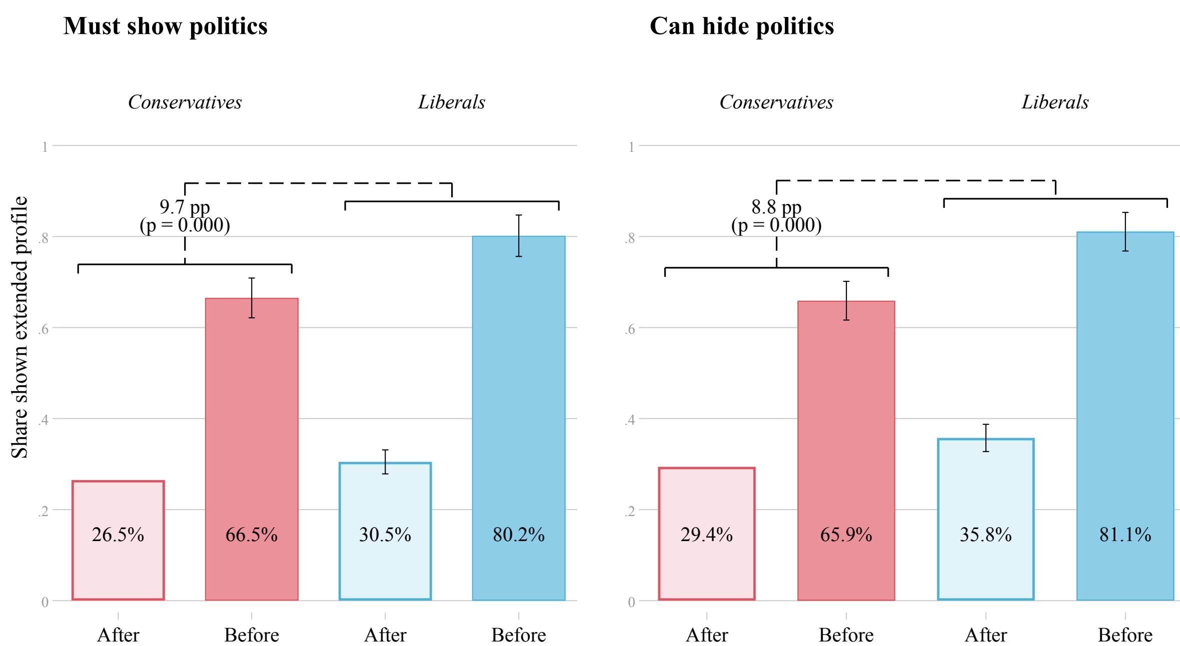
Note: This figure summarizes participants' beliefs in round two of the WTP experiment about the probability that possible WTP matches would email Congress when they return for a follow-up survey. The figures at left and right are split between potential liberal versus conservative matches. Within each 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.

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

Under reasonable assumptions, the ability to hide one's politics should only impact the partisan influence gap in the WTP experimental set-up (coefficient β_3 in equation 4) through its effects on $\Delta P(Email|D) - \Delta P(Email|R)$, the gap in Democrats' beliefs about their ability to influence conservatives versus liberals. Then, we could in theory use the round-two WTP experiment to benchmark the role of this partisan belief gap in Democrats' differential efforts to reach out to liberal versus conservative matches. Unfortunately, we find that allowing WTP participants to hide their politics in each profile does not substantially affect the partisan belief gap; thus, we cannot use this strategy.

The effective first stage on beliefs: Even while participants with the option to hide their politics predict that invitations with and without their politics would have substantially different impacts, we do not find that giving people the option to hide politics shrinks the partisan belief gap. This null effect arises because participants who cannot hide their politics are more optimistic about the impacts of baseline profiles on conservatives: while participants with the option to hide their politics estimate that an invitation showing their politics would make a conservative only 3.2pp more likely to email Congress (Figure 8), participants who cannot hide their politics estimate that the same invitation would make conservatives 8.5pp more likely to email Congress (Appendix Figure E26).[48] 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.

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



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

No impact on influence attempts: Consistent with the null first-stage effects on participants' beliefs about their relative impact on liberal versus conservative matches, the option to hide one's politics does not significantly reduce the gap in WTP participants' efforts to mobilize liberal versus conservative matches (Figure 9). With or without the option to hide one's politics, Democrats are about 19% less likely to try to influence conservatives than liberals. We cannot distinguish whether this null effect arises from a true null effect on how much influence participants believe they can have on liberals versus conservatives, or because Democrats have differential preferences over directly trying to engage liberals versus conservatives in ac-

[48]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.

tion during the survey, $A(Try\ Influence\ During|D,R)$. Nevertheless, our results here are fully consistent with Democrats' beliefs about their impacts on liberals versus conservatives driving their reluctance to reach across the aisle.

7.4 Summing up: Drivers of the partisan influence gap

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

8 Robustness

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

8.1 Robustness of the Wave-2 results

Sample definitions: Our main Wave-2 results are highly robust to changes in the sample definition (Appendix Figure F27). First, they are robust to expanding the Wave-2 experimental sample by assuming that all participants who leave the experimental survey after seeing a preview of the upcoming email opportunity would not have emailed Congress had they continued; we make this assumption in our main Wave-1 analysis (Section 5.1.2).[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 (Section 4.1.3).

Control variables: Our Wave-2 estimates are also fully robust the following control specifications (Appendix Figure F27): 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

[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 F.1.1)

[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.

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 F27). Next, our results are robust to assuming that recognizing the study purpose either increases or decreases our main outcomes by 0.2sd (Appendix Figure F28), 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 4.1.2). Our goal here is not to identify the differential impact of invitations on liberals or conservatives who are otherwise identical; indeed, gaps in concern about climate change and education would be at play in real-world attempts to mobilize liberals versus conservatives for climate action. Rather, Appendix Figure F29 shows that the partisan gap we estimate is largely robust to controlling for the interaction of treatment status with key traits associated with our differential recruitment strategies for liberals and conservatives: whether a participant is recruited from Qualtrics, political engagement, and income.

8.2 Robustness of the Wave-1 results

Sample definitions: Our main Wave-1 results are highly robust to changes in the experimental sample (Appendix Figure F30). While we pre-registered a total Wave-1 sample size of about 8,200 participants, we did not specify whether this total would include or exclude participants who left the survey before explicitly choosing to email Congress or not. The Wave-1 estimates are robust to restricting to the first 8,200 participants randomized into a Wave-1 treatment arm (filling in zeros for outcomes among those who attrit), all of the 8,269 participants for whom we observe an explicit choice to email Congress or not, and the first-recruited 8,200 participants for whom we observe an explicit choice to email Congress. As expected, restricting to those who make explicit email choices substantially increases our estimates for the impacts of the A1 arm (Tell After) relative to A0, the pure control: participants in the A1 and A2 groups are substantially less likely to finish the Wave-1 survey (Section 5.1.2) and thus were more likely to have outcome variables set to zero. However, this shift simply means that our main estimates for the impacts of A1 versus A0 are conservative. None of the sample redefinitions changes our estimates for the effect of A2 versus A1, our estimate of Democrats' efforts to influence others.

Next, our results for the A2 versus A1 gap are robust to restricting to participants who correctly understand the experimental set-up (Appendix Figure F30). In particular, we re-estimate our main specifications among participants who correctly reported when Wave-2 participants would see that they emailed Congress, if they did so, and separately, among participants who correctly reported whether their choice to email Congress could affect whether their Wave-2 matches do the same.[52]

Control variables: Next, our Wave-1 results are fully robust to the set of control variables we include (Appendix Figure F30): 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 various forms of past political engagement.

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

For completeness, we show that our estimates of influence motives are robust to eliminating any differential demand effects that remain. On the same Likert-style demand measure we describe in Section 8.1, perceived experimenter demand is 0.06sd higher among A2 participants than A1 participants (columns 3 of Appendix Table C3). In 500 simulations, we randomly drop A2 participants who report experimenter demand at 6 on the 6-point scale until there is zero differential demand between the A1 and A2 groups; we then re-estimate our main Wave-1 regressions in each adjusted sample. Our point estimates fall on average by only a small amount when we correct for differential demand (Appendix Figure F31).

8.3 Robustness of the WTP results

Sample definitions: Our WTP results are fully robust to using the following adjusted samples (Appendix Figures F32 through F34): among participants who finished all 20 WTP choices in either the Basic or Money groups; excluding the 166 participants in our main analysis sample who answered fewer than 5 comprehension questions correctly; and excluding the 7 participants in our main sample who explicitly mentioned being concerned that conservatives would email Congress opposing climate policy.

Control variables: Next, our WTP results are fully robust to the set of control variables we include (Appendix Figures F32 through F34). First, our main estimates are robust to including only individual fixed effects and controlling for choice number (matching columns 1 and 3 of Table 5), adding in controls for basic match traits and their interactions with $Before_{ij}$, and adding in a richer set of controls and interactions for possible targets' age groups and states. Next, we also show that our main results are robust to including Lasso-selected controls drawn from the full set of possible controls for possible match profiles, including

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

dummy variables for avatar traits like hairstyle and color. We show in particular that the coefficients on $Repub_{ij}$ and $Before_{ij} * Repub_{ij}$ are robust to controlling for all traits selected by Lasso to predict $Repub_{ij}$, as well as for their interactions with $Before_{ij}$. Finally, we show that our results are robust to excluding participant fixed effects and including the individual-level controls included in our main Wave-1 specifications.

9 Conclusion

In a large online experiment with over 20,000 participants recruited from social media and online panels, we create opportunities for Democrats to invite Americans across the country, demographic groups, and the political aisle to join them in political climate advocacy. Democrats are motivated to get others engaged in climate action, but they differentially reach out to other liberals rather than building a more bipartisan political coalition. Even as affective polarization reaches record highs, however, we find no evidence that this gap arises from a distaste for engaging with counter-partisans. Rather, Democrats' reluctance to reach across party lines can be fully explained by their accurate beliefs that their invitations have less impact on action by conservatives, rather than liberals. These beliefs themselves reflect second-order affective polarization: Democrats believe that they have higher impacts on conservatives when they hide their own political leanings. If beliefs are more malleable than preferences, our beliefs-based account of Democrats' influence gaps suggests that cross-party cooperation may be easier to achieve than the dominant narrative on affective polarization would suggest. On the other hand, Democrats' beliefs about their influence on liberals and conservatives are accurate in our setting, so no obvious belief-correction intervention would promote bipartisan outreach. These results suggest that parallel partisan movements, with liberals and conservatives each trying to engage co-partisans, may be efficient.

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