Content That's as Good as Contact? Vicarious Intergroup Contact and the Promise of Depolarization at Scale

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Abstract

Can observing opposing partisans engage in dialogue depolarize Americans at scale? Partisan animosity poses a challenge to democracy in the United States. Direct intergroup contact interventions have shown promise in reducing partisan polarization, but are costly, time-consuming, and sensitive to subtle changes in implementation. Vicarious intergroup contact—observing co-partisans engage with outparty members—offers a possible solution to the drawbacks of direct contact, and could potentially depolarize Americans quickly and at scale. We test this proposition using a pre-registered, placebo-controlled trial with a nationally representative sample of Americans. Using both attitudinal and behavioral measures, we find that a 50-minute documentary showing an intergroup contact workshop reduces polarization and increases interest but not investment in depolarization activities. While we find no evidence that the film mitigates anti-democratic attitudes, it does increase optimism about the survival of democratic institutions. Our findings suggest that vicarious intergroup contact delivered via mass media can be an effective, inexpensive, and scalable way to promote depolarization among Americans.

Keywords: polarization; vicarious contact; partisanship; democracy; intergroup relations

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Introduction

Fear and hatred across partisan lines has grown dramatically in the United States in recent years (Iyengar et al., 2019). "Affective polarization" of this sort activates, and is sustained by, a range of psychological processes that erode trust, foster misperceptions, and foment mutual hostility across the partisan divide, all of which may damage democracy (Ahler and Sood, 2018; Moore-Berg et al., 2020; Mernyk et al., 2022; Finkel et al., 2020). Affective polarization may also cause citizens to prioritize partisan interests over democratic principles (Graham and Svolik 2020; Iyengar et al. 2019; Mason 2018; McCoy et al. 2018; Svolik 2020; c.f. Broockman et al. 2020).

In this paper we experimentally evaluate a promising but understudied mechanism for reducing affective polarization: vicarious intergroup contact, whereby individuals who observe or learn about direct intergroup contact enjoy its benefits without actually participating in it (Wright et al., 1997). Vicariously reading about or watching members of one's ingroup engage in civil, mutually respectful dialogue with outgroup members may generate many of the same benefits of direct contact while avoiding some of the potential limitations and adverse unintended consequences, as we discuss below. Indeed, a recent study comparing 25 "light touch" depolarization interventions found that a 4-minute advertisement depicting harmonious cross-partisan interactions in the UK produced the largest reductions in partisan animosity (Voelkel et al., 2024). Yet to our knowledge, only four studies have examined the effects of vicarious cross-partisan contact in any rigorous, systematic way (Huddy and Yair, 2021; Voelkel et al., 2023, 2024; Wojcieszak and Warner, 2020). (We describe these studies in further detail in Supplementary Information (SI) A.)

We advance this nascent literature by reporting results from a pre-registered, placebo-controlled randomized trial designed to test the effects of a 50-minute documentary depicting a direct contact workshop administered by Braver Angels, a nationwide nonprofit that seeks to reduce affective polarization among Americans.[1] During the workshop, which took place shortly after the 2016 US presidential election, Democrats and Republicans convened to interact with and learn about outgroup members

[1] A link to the documentary is available in SI B.

in a structured, facilitated environment. The documentary features footage of the workshop and interviews with the organizers and participants—rank-and-file American voters who expressed policy preferences that are fairly typical of their respective parties (e.g., opposition to abortion among Republican participants, and support for universal health care among Democrats). The creators of the film partnered with us because they have preexisting ties to media production companies and sought an independent impact evaluation to inform their case for distributing the documentary more widely.

To evaluate the documentary, we recruited a nationally representative sample of 2,000 Americans from YouGov's online panel in the summer of 2022. We contacted participants in three waves. In wave 1, we collected information on demographics and potential sources of treatment effect heterogeneity, and assigned participants to one of five treatment conditions: (1) the 50-minute Braver Angels documentary; (2) a 50-minute placebo nature documentary about wildebeest migration; (3) a 5-minute version of the Braver Angels documentary; (4) a second 5-minute version that emphasized partisan misperception correction; or (5) a pure control group.[2] We measured outcomes in waves 2 and 3, which were administered on average five and 50 days after treatment, respectively. Here we focus primarily on our wave 2 results, and on the comparison between the Braver Angels and placebo documentaries.

Consistent with our pre-registered hypotheses,[3] we find that vicarious contact via the documentary reduced affective polarization, our primary attitudinal outcome of interest. Effects on our primary behavioral outcomes are more mixed: vicarious contact increased interest but not investment in future depolarization programming. The documentary also curbed stereotyping of out-partisans—one of the mechanisms that we hypothesized might explain the reduction in affective polarization. Consistent with some previous studies (Baron et al., 2025; Santoro and Broockman, 2022; Voelkel et al., 2023), we find no evidence that the documentary mitigated anti-democratic attitudes. We do find, however, that it increased optimism about the survival of democratic institutions and bolstered faith in the efficacy of dialogue as a tool for political change. The documentary had larger effects on self-identified Democrats, although the difference with self-identified Republicans is not statistically significant. We

[2]See SI B for further details and links to each video.

[3]We pre-registered our study prior to implementation. The pre-analysis plan is available here: `https://osf.io/ymdxq`

find some suggestive evidence that these effects decayed over time.

Our study makes at least five contributions to the growing literature on depolarization. First, as noted above, ours is one of only a small handful of studies of vicarious contact across partisan (rather than, say, ethnic) lines, which, despite its promise, remains an under-explored mechanism for reducing partisan animosity. It is not obvious that findings from studies of other forms of vicarious intergroup contact will generalize to vicarious cross-*partisan* contact, as research on partisan animosity explicitly draws on a "distinct theoretical literature" (Paluck et al., 2021, 537). Second, whereas most studies of cross-partisan contact (vicarious or otherwise) involve interventions that were designed and implemented by researchers, we evaluate the effects of a documentary produced by an established nonprofit with a nationwide profile that is committed to depolarizing the American public and, importantly, to disseminating the documentary as widely as possible. This increases the probability that the intervention we evaluate will be scaled up outside the context of the study itself. We also extend beyond the "light touch" interventions that have become popular in depolarization research (Voelkel et al., 2024) and the prejudice reduction literature more broadly (Paluck et al., 2021). Indeed, in their landmark review of the literature, Paluck et al. (2021, 536) lament the "scarcity of prejudice reduction research on the kinds of programs that are frequently called for in the real world, specifically entertainment and mass media interventions." Our study helps fill this gap.

Third, whereas most depolarization studies focus on measuring attitudes alone (Hartman et al., 2022), our evaluation combines both attitudinal and behavioral outcomes. We also measure a wider range of attitudinal outcomes than most depolarization studies,[4] and a more realistic set of behavioral outcomes[5] designed to capture the extent to which participants might support and seek out future depolarization programming—an important outcome in itself, since durable depolarization likely requires reinforcement over time. Fourth, whereas most cross-partisan contact studies (vicarious or otherwise) measure outcomes immediately or only very shortly after treatment is administered (Hartman et al.,

[4]Most other studies focus on measuring partisan animosity alone.

[5]Some depolarization studies have measured outcomes using behavioral games, but these are highly stylized and the stakes are typically very low. For example, Voelkel et al. (2023) and Voelkel et al. (2024) include dictator games in which participants were given an endowment of just 50 cents each.

2022), we measure outcomes in both the short (five days) and medium (50 days) terms,[6] thus generating important insights into treatment effect durability while also mitigating concerns about social desirability bias and experimenter demand effects, which are likely to diminish over time. Our use of a nationally representative sample should also increase our ability to make credible inferences about the likely effects of the documentary on the U.S. public once it is distributed at scale.

Finally, the vicarious contact intervention we study (the Braver Angels documentary) was a byproduct of a direct contact intervention (a Braver Angels workshop), illustrating how vicarious contact may help extend the reach and magnify the impact of direct contact. The contact occurred in the wake of a highly contentious presidential election, thus showcasing the possibility of civil cross-partisan relations even at a moment of heightened partisan animosity. Equally important, the workshop on which the documentary was based was the subject of a previous experimental evaluation that used very similar attitudinal outcome measures (Baron et al., 2025), thus providing a plausible benchmark for our treatment effect estimates. This can help inform the durability-scalability trade-off that is inherent to any comparison between direct and vicarious contact interventions, helping practitioners decide where to direct their efforts and funds (Hartman et al., 2022; Littman et al., 2023). As discussed further below, we estimate that the reach of the documentary is roughly equivalent to 10 direct contact workshops implemented by the same nonprofit, achieved in 12% of the time and at a fraction of the cost. Taken together, these findings indicate that scalable, inexpensive vicarious contact interventions can reduce affective polarization, at least in the short term.

Theoretical Framework

One possible avenue for reducing intergroup animosity—including affective polarization—is through direct intergroup contact (e.g., Mousa, 2020; Paluck et al., 2021; Pettigrew and Tropp, 2006; Scacco and Warren, 2018; Weiss, 2021). While most studies of the "contact hypothesis" (Allport, 1954) focus on prejudice between racial, ethnic, or sectarian groups, the effects of contact may extend to animosity

[6] As we discuss in SI A, the longest-term outcome in any other study of vicarious cross-partisan contact was measured two weeks after treatment (Voelkel et al., 2024).

across the partisan divide. For instance, recent research has shown that in-person cross-partisan workshops can reduce affective polarization and increase donations to depolarization initiatives, with effects lasting at least six months (Baron et al., 2025; Levendusky and Stecula, 2021). Other studies have shown that online cross-partisan conversations can reduce affective polarization as well (Combs et al., 2023; Santoro and Broockman, 2022; Tausch et al., 2024).

Overall, however, direct contact has yielded mixed results, and recent studies suggest the effectiveness of these interventions may be sensitive to small changes in program design and scope (Paluck et al., 2019; Santoro and Broockman, 2022; Zhou and Lyall, 2022). Moreover, direct contact interventions tend to be time- and resource-intensive; typically rely on competent, experienced facilitators; and require recruiting and matching participants across partisan lines—a challenge given that many Americans feel threatened by and thus avoid exposure to out-partisans (Dorison et al., 2019; Enos, 2014; MacInnis and Page-Gould, 2015). This may cause a dynamic whereby only those who are already interested in engaging with out-partisans agree to participate in direct contact interventions. Indeed, previous studies suggest that individuals are more likely to seek out direct contact when they believe that doing so will help them achieve a specific goal, such as making new friends or learning new skills (Kauff et al., 2021). Conversely, others may find themselves forced into direct contact (e.g. in school or at work)—a loss of volition that may heighten the risk of backfire effects (Paolini et al., 2024). These limitations may diminish the prospects for direct contact to reduce partisan animosity at scale.

Vicarious intergroup contact offers a potential solution to these problems. Like direct contact, vicarious contact is rooted in the intuition that cooperative, mutually respectful intergroup interactions can reduce intergroup animosity, especially when participants are united by a common goal, have equal standing, and enjoy social and institutional support (Allport, 1954). Vicarious contact likely activates both cognitive and emotional mechanisms among participants, which work in tandem. Cognitively, it may transmit information that corrects (excessively) negative stereotypes about the outgroup while also fostering perceptions of intergroup similarity (Wojcieszak and Warner, 2020)—for instance, by convincing participants that their policy preferences are closer to those of the outgroup than they

previously realized. Vicarious contact may also increase open-mindedness to outgroup views.

Emotionally, vicarious contact may promote intergroup empathy and reduce the anxiety that may otherwise arise when individuals think about or interact with outgroup members (Mazziotta et al., 2011; Vezzali et al., 2014). Vicarious contact may also mitigate the perception of threat (to values, to the country, to one's ingroup, etc.) posed by outgroup members—likely a complex process involving both cognition and emotion. Individuals may also extrapolate beyond their own experiences of vicarious contact to make inferences about intergroup relations more generally. For example, they may become more optimistic about the efficacy of dialogue as a mechanism for promoting intergroup harmony; about the prospects for avoiding intergroup violence and promoting intergroup civility; and—to the extent that endemic intergroup conflict is perceived as a threat to democracy—about the survival of democratic institutions in the future.

The theory underlying vicarious contact posits that individuals can experience these benefits even if they merely *observe* intergroup contact, rather than participating in it themselves. Unlike direct contact, vicarious contact can be experienced in private, without raising concerns about social sanctions or unpleasant intergroup interactions. Individuals are also likely to enjoy greater discretion over their exposure to vicarious contact, since they can simply look away from contact they find unappealing. In this sense, the psychological barriers to entry may be lower for vicarious contact than for direct contact because individuals may find it less threatening to simply observe intergroup contact rather than participate in it themselves. Observing positive direct contact may also help establish civil, mutually respectful intergroup interactions as a social norm, making participants believe that intergroup interaction is both common and desirable. Finally, vicarious contact may magnify the effects of direct contact interventions. Direct contact may help depolarize those who actually engage in it, while vicarious *exposure* to direct contact may help depolarize those who merely witness it.

While scholarship on vicarious contact is still nascent (Paluck et al., 2021), previous studies suggest that it can improve intergroup relations (Mazziotta et al., 2011; Vezzali et al., 2014). But there may also be limits to what contact—direct, vicarious, or otherwise—can achieve. For example, one observational study from Denmark finds that while (self-reported) direct contact across partisan lines

is negatively correlated with affective polarization, the association is null among those who strongly identify with their political party (Thomsen and Thomsen, 2023). The effects of contact in the US may similarly depend on the strength of participants' party identification—for example, if partisan motivated reasoning induces individuals to resist the cognitive mechanisms that contact activates (Mason, 2018). The effects may also be concentrated among particular partisan or ideological groups. For instance, previous studies suggest that conservatives are more likely than liberals to believe that "to compromise with one's political opponents is dangerous because it usually leads to the betrayal of our own side" (Jost et al., 2017; Jost, 2017). This may dampen the impact of cross-partisan contact among more conservative participants. We examine these and other potential moderators below.

Materials and Methods

Recruitment and randomization

We conducted a pre-registered, placebo-controlled trial using a nationally representative sample of Americans ($n = 2000$) recruited from YouGov's online panel. We fielded wave 1 of the study between June 27 and July 15, 2022. In total, 2,573 participants were randomized into one of the five conditions described in the introduction. From those 2,573 participants, YouGov created a nationally representative sample of 2,000 by matching participants' demographic characteristics to the 2019 American Community Survey and voter files, as described in SI C. Participants who were assigned to one of the three treatment conditions (the full Braver Angels documentary or one of the two shorter films) or the placebo condition were then invited to watch the video to which they were assigned; participants who were assigned to the pure control condition were not shown a video. After participants finished watching the videos, we presented them with comprehension checks and three open-ended qualitative questions about the film.

In wave 2, we re-contacted all 2,573 participants who were randomized into one of the five conditions to complete a 10-15 minute survey, which we use to measure the study's main outcomes. Participants were recontacted at least 24 hours after treatment; they completed the survey 5 days after treatment

on average. 2,105 participants completed wave 2. Out of this wave 2 sample, YouGov created a nationally representative sample of 1,600 participants. We fielded wave 3 between August 16 and 22, 2022. Again, all 2,573 participants were re-contacted in wave 3; 1,612 completed the survey. From this latter sample, YouGov created a nationally representative sample of 1,360 participants. Descriptive statistics and balance tests are available in SI D and SI F, respectively. We discuss attrition below and in SI G.

Outcomes and measurement

We test the effect of the documentary on two main outcomes: (1) affective polarization, measured attitudinally, and (2) interest and investment in depolarization, measured behaviorally. Vicarious contact is likely to have the strongest and most lasting effects if it not only reduces affective polarization, but also inspires Americans to learn more about and participate in future depolarization initiatives, and to support the organizations that implement those initiatives. In other words, vicarious contact is likely to be most effective if it not only changes attitudes, but alters behaviors as well.

Following Baron et al. (2025), we measured affective polarization using an index of five items[7] which were standardized and averaged (Cronbach's α=0.744):

1. Feeling thermometer: We asked respondents how "warmly" they feel towards the inparty and outparty on a scale of 0 to 100. We then calculate the difference between these two numbers.[8]

2. Social distance: We asked respondents how comfortable they would feel having out-partisans as (1) close personal friends and (2) neighbors, and (3) how comfortable they would feel if their best friend married an out-partisan.

3. Trust: We asked respondents how often they believe they can trust the inparty and outparty to "do what is right for the country" on a scale of 1 to 5, where 1 indicates almost never and 5 indicates almost always. We then calculate the difference between these two numbers.

[7]Missing values and "don't know" responses on each item were removed via listwise deletion.

[8]For example, if a Democrat rated other Democrats as an 80 and Republicans as a 30, then she would receive a score of 50 on the feeling thermometer.

4. Outparty threat (binary): We asked respondents whether they believe out-partisans constitute a "serious threat" to the United States.

5. Negative partisanship (binary): We asked respondents whether they identify with their party primarily out of support for the inparty or primarily out of opposition to the outparty.

Our pre-registered affective polarization index thus benchmarks trust and warmth towards the outparty against trust and warmth towards the inparty. We also construct an alternative version of the index that does not subtract outparty warmth from inparty warmth or outparty trust from inparty trust (Cronbach's α=0.766).[9] This alternative index uses outparty warmth and outparty trust alone. While it was not pre-registered, this approach to operationalizing out-partisan animus is more standard in the literature (Hartman et al., 2022).

We measured interest and investment in depolarization in three ways. First, in wave 2, we asked participants whether they were interested in signing up for the Braver Angels newsletter, which features news about upcoming depolarization workshops and events. At the end of the survey, we provided participants who expressed interest with the link to sign up for the newsletter and tracked whether they clicked the link. Unfortunately we were not able to track whether they actually signed up for the newsletter. We instead assume that if respondents expressed interest in signing up *and also* clicked a link to do so later in the survey, then they were genuinely interested in learning more about depolarization.[10]

Second, in wave 2, we gave participants the option of donating some of their study compensation to three different depolarization organizations: Living Room Conversations, AllSides for Schools, and Braver Angels.[11] Finally, in wave 3, we gave participants the opportunity to take part in one-on-one,

[9]For correlations between the different components of the index, see SI D.2.

[10]It is possible that some respondents might have signed up for the newsletter not because they were interested in additional depolarization programming, but because they wished to track future research or financial opportunities. We think this is unlikely given the way we described the newsletter before offering respondents the option of signing up: "Braver Angels is an organization that brings Americans together to bridge the partisan divide and strengthen American democracy. Braver Angels offers a diversity of workshops and events with the aim of depolarizing Americans. Are you interested in signing up for the Braver Angels newsletter to get updates on their work, find events near you, and learn how to get involved?"

[11]Due to university and IRB regulations, we were unable to make donations on participants' behalf. Instead, we

online cross-partisan conversations run by the organization Unify America,[12] which is independent of Braver Angels. Participants who expressed interest in taking part in these conversations were redirected to the Unify America website using a special link. We tracked whether they clicked the link. Unify America also informed us whether participants signed up after clicking the link, and whether they actually participated in a conversation.

Secondary outcomes and mechanisms

We also test the effects of the Braver Angels documentary on two secondary outcomes. First, we expected that by mitigating affective polarization, vicarious contact might have the salutary side effect of increasing participants' optimism about the prospects for reducing hostility and fostering civility across partisan lines, and for ensuring the longevity of American democratic institutions. We asked respondents to rate how optimistic they are about the survival of democratic institutions, the restoration of civility between Democrats and Republicans, and the ability of American democracy to overcome polarization (on a scale of 1-5). We also asked them to rate how effective they believe dialogue is as a tool for change (on a scale of 0-100), and how long they believe it will take to rebuild trust across the partisan divide (ranging from 0-5 years to never).

Second and relatedly, we expected that vicarious contact might also strengthen participants' commitment to democratic norms and procedures. While scholars continue to debate the causal relationship between affective polarization and commitment to democracy (Broockman et al., 2020), recent research suggests that highly polarized citizens are often willing to trade off democratic principles for partisan gain (Graham and Svolik, 2020). We hypothesized that by reducing affective polarization, vicarious contact might weaken this tendency. We thus asked participants to rate how likely they would be to support anti-democratic practices that benefit their own party in six hypothetical scenarios.[13]

asked what percentage of their compensation participants wished to donate, then provided links to the corresponding websites where they could make the donations themselves. Importantly, we informed participants that we could not make donations on their behalf only *after* they indicated how much they wished to donate. Participants therefore should have interpreted their decision to donate as costly and final.

[12] See `https://www.unifyamerica.org/` for details.

[13] For instance (aimed at Republicans in this case): "Imagine a right-leaning officeholder in your jurisdiction sought to restrict or ban rallies by far-left groups, on the grounds that even peaceful far-left rallies have the potential to

We then standardized and averaged these items into an index.

Finally, we posited several mechanisms through which the documentary might mitigate affective polarization. First, vicarious contact might encourage participants to question negative stereotypes about out-partisans and potentially adopt positive stereotypes instead. We asked participants to rate how well different characteristics describe members of both their own party and the outparty. Some characteristics were positive (patriotic, honest, generous, intelligent, and open-minded) while others were negative (mean, selfish, and hypocritical). We construct two standardized indices of positive and negative stereotypes. Second, vicarious contact might encourage open-mindedness to outparty views and greater willingness to consider the perspectives of outpartisans. Third, vicarious contact might increase empathy towards outgroup members and their concerns.[14] Fourth, vicarious contact might weaken participants' belief that Democrats and Republicans hold irreconcilable views on key policy issues. We asked participants to rate on a scale of 0-100 how strongly they believe Democrats and Republicans disagree on three issues in particular: abortion, paid family leave, and same-sex marriage. Vicarious contact may also mitigate beliefs that partisans are excessively divided.[15]

Analysis strategy

For most of the analyses reported in this paper, we estimate the intention-to-treat (ITT) effect of the documentary using a simple bivariate regression comparing mean outcomes among participants assigned to the full-length Braver Angels documentary to mean outcomes among participants assigned to the placebo nature documentary. We privilege the matched over the full sample because this is what we pre-registered, but also because we are most interested in how the documentary would affect a representative sample of Americans. Additionally, the matched sample exhibits no differential attrition between the Braver Angels and placebo documentary conditions, allowing us to make more reliable inferences. We weight all observations by the product of the inverse probability of being

turn violent. To what extent would you support restrictions of this sort?" See SI I.1.2 for the full list of hypothetical scenarios.

[14]See I.1.3 for a full description of the measurement of mechanisms

[15]Vicarious contact might correct misperceptions of out-partisans that frequently accompany polarized psychologies (Moore-Berg, 2023). Since we do not have a direct measure of partisan misperception correction, we test this mechanism through a more exploratory analysis in SI I.2.

included in the matched nationally representative sample and the inverse probability of completing the survey. We describe our procedure for constructing these weights below and in SI G.2. We also report results for the full sample in SI I.1.[16]

To test for treatment effect heterogeneity by party ID and other potential moderators, we simply interact each moderator with our indicator for assignment to the Braver Angels documentary.

Attrition

Given that we asked participants to consent to take part in the study before knowing their treatment assignment, we were concerned about participants dropping out due to the length of the Braver Angels documentary. We were especially concerned about the possibility of differential attrition across treatment conditions.[17] Beyond including a placebo group, we took several additional steps to mitigate this risk. First, as part of the informed consent process, we told participants that they might be asked to watch a 50-minute documentary, and asked them to consent only if they were certain they would be able to watch. Second, we informed them that they would receive extra compensation (in the form of a $20 Amazon gift card) if they were assigned to watch a documentary. Third, we used a second informed consent filter to attempt to screen out likely attriters: after participants gave informed consent, we reminded them again that they might be asked to watch a 50-minute documentary, and gave them another opportunity to opt out of the study prior to randomization. Patterns of attrition reveal both that our concerns were justified—we observe significantly higher attrition in the two 50-minute documentary conditions—and that our attempts to mitigate differential attrition between the full-length Braver Angels film and the placebo documentary of the same length were effective. See Table SI.9 for further detail.

We thus focus on comparing participants assigned to the full-length Braver Angels documentary with those assigned to the placebo nature documentary in the nationally representative sample, where we

[16]We report these results since it is possible that the standard matching procedure conducted by YouGov introduces some amount of post-treatment bias by excluding less attentive participants who were nevertheless randomized. See SI C for a full description of the matching procedure. On the other hand, estimates from the full sample may be susceptible to bias as a result of differential attrition.

[17]We discuss attrition in greater detail in SI G.

observe no differential attrition between treatment conditions. As we show in SI G, observable differences between attriters and non-attriters in this sample are substantively small, with standardized mean differences (SMDs) close to 0 for most covariates. Levels of compensation and the amount of time spent watching a film as part of the study are identical for these two groups of participants, ensuring greater comparability.[18] Compliance rates for both the Braver Angels documentary and the placebo were high. 94% of respondents assigned to the placebo answered an attention check question correctly[19], while 95% of respondents assigned to the Braver Angels documentary correctly answered one of two manipulation check questions.[20] There was no statistically significant difference in compliance between the two groups ($p = 0.42$).[21]

As an additional precaution, we use inverse probability weights (IPWs)[22] to correct for the small and statistically insignificant levels of differential attrition that we observe between the Braver Angels and placebo nature documentary groups. See SI G.2 for further details on construction of these weights.

Results

Primary Outcomes: Affective Polarization and Interest and Investment in Depolarization Activities

Consistent with our hypotheses, relative to the placebo nature documentary, we find that the Braver Angels documentary reduced scores on our pre-specified affective polarization index by 0.14 standard

[18] In our PAP we pre-specified that we would focus on comparing participants in our nationally representative sample who watched the Braver Angels documentary to those who watched the placebo nature documentary *or* who were assigned to the pure control group. We believe this latter comparison is potentially misleading, since participants in the pure control group were paid less and were not asked to watch a film of any kind, making them a less reliable counterfactual group. Moreover, as we discuss in SI G, we observe significant differential attrition between the control group and the two documentary groups. We therefore focus on comparing the two documentary groups to one another. We discuss this and all other deviations from our PAP in further detail in SI H, and we present results using our pre-specified approach in SI I.2.

[19] "In the video, which migration of animals is the primary focus of the video?"

[20] "In the video, two of the participants, Kuwar and Greg, become fast friends. At the conclusion of the workshop, they decide to spend time together in each other's hometowns. Where do they meet?" "In the video, the workshop participants describe stereotypes about each political party. Which of the following was NOT discussed?"

[21] However, we do observe some differential attrition across some of our other conditions, which limits our ability to reliably test all of our pre-registered hypotheses. Again, we discuss deviations from our PAP in SI H.

[22] We use 12 pre-treatment covariates as predictors of attrition: party ID, ideology, age, gender, race, education level, marriage status, religion, whether participant is a parent or not, employment, geographic region, and 2020 turnout.

deviations (β = -0.140, p=0.039, N=584), and reduced scores on a more conventional index focused on outparty animus by 0.163 standard deviations (β = -0.163, p=0.017, N=584).[23] Figure 1 visualizes these results. The figure reports the ITT of the Braver Angels documentary at wave 2, which participants completed five days after treatment on average.[24] We operationalize affective polarization using the five-item index described above, which we standardize for ease of interpretation. Larger values indicate greater affective polarization. We show in SI I.1 that our results are robust to alternative specifications that do not include these weights. We also show in SI I.1 that the ITT on our pre-specified index is consistently (albeit marginally) smaller than the ITT on our alternative index focused on outparty animus, implying that the documentary reduced affective polarization primarily by softening partisan animosity, rather than by closing the gap between perceptions of the inparty and outparty.

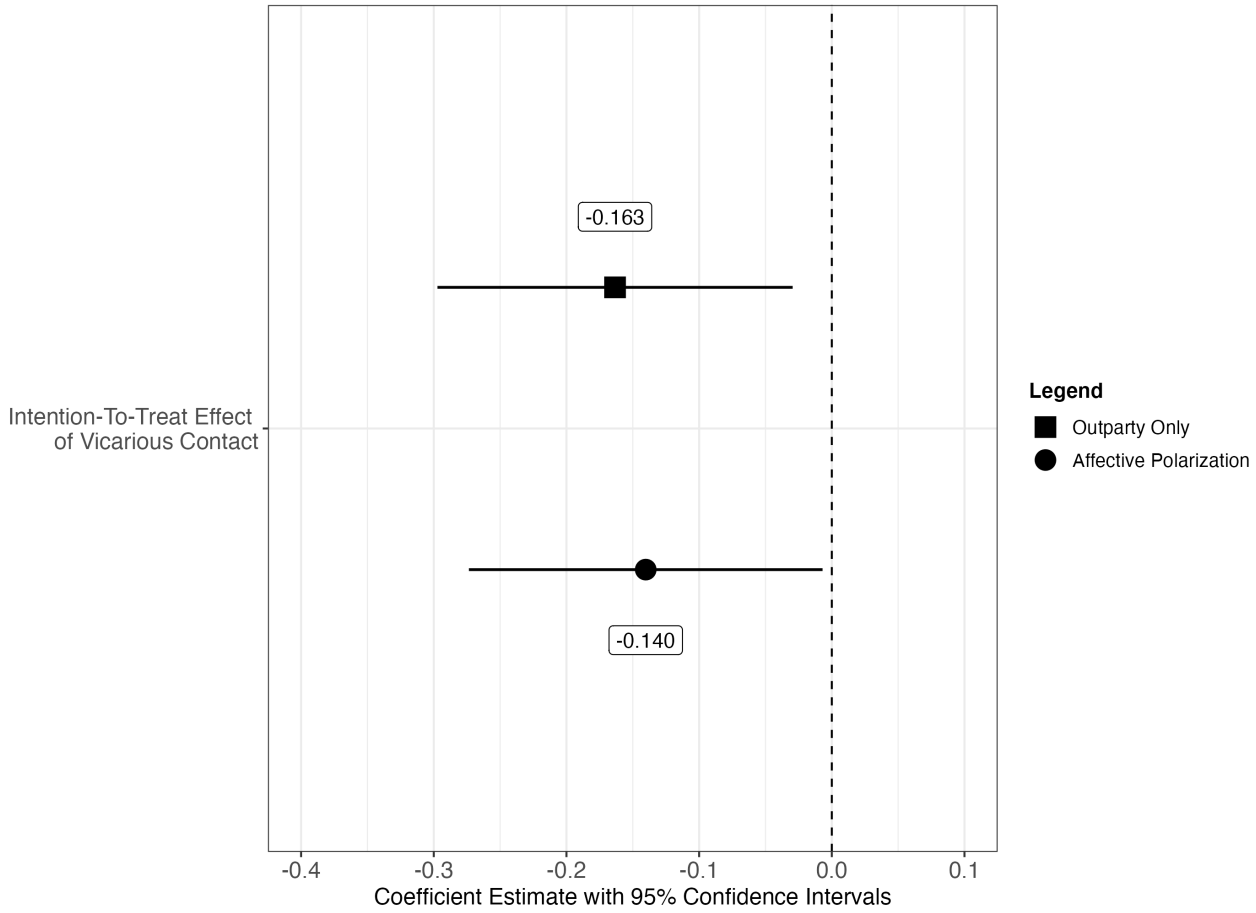
Results for our behavioral outcomes are more mixed, as we show in Figure 2. Consistent with our hypotheses, relative to placebo, we find that participants who were assigned to watch the Braver Angels documentary were 7.7 percentage points more likely to click a link to sign up for the newsletter ($\beta = 0.077$, $p = 0.035$, $N = 509$). However, contrary to our hypotheses, we find no evidence that the documentary increased investment in depolarization organizations. If anything, we find suggestive evidence for the opposite: relative to placebo, participants who were assigned to watch the Braver Angels documentary were 3.7 percentage points less likely to make any donations (β = -0.037, p = 0.369, N = 583), though this difference is substantively small and not statistically significant at conventional levels.[25] One possible explanation for these mixed effects on our behavioral outcomes is that YouGov respondents expected their compensation to be commensurate with the amount of time they spent completing the study; while the Braver Angels film appears to have stimulated their

[23] As we show in SI I.3, the ITT on affective polarization appears to be driven in particular by an increase in trust that out-partisans will "do what is right for the country" (β = -0.242, p= 0.021 , N=583), and a reduction in the belief that out-partisans represent a "serious threat" to the country (β = -0.310, p=0.005, N=583).

[24] This gap between treatment and outcome measurement should minimize the risk that any treatment effects we detect are artifacts of a temporary positive emotional response to the optimism of the documentary. In addition, in Table SI.36 we find no evidence of treatment effect heterogeneity by the gap between treatment and outcome measurement during wave 2.

[25] As we show in SI I, participants also donated 4.26% less of their compensation overall ($\beta = -4.26$, p =0.266, N = 581), and donated less to each of the three depolarization organizations individually, including Braver Angels itself (β = -0.022 , p = 0.594 , N = 583), though none of these effects is statistically significant at conventional levels.

Figure 1: Vicarious intergroup contact reduced affective polarization among a nationally representative sample of Americans



Notes: Intention-to-treat estimates from weighted OLS regressions comparing the effect of the Braver Angels documentary to the effect of a placebo nature documentary among a nationally representative sample of Americans ($N = 584$). Symbols denote coefficients; lines denote 95% confidence intervals. The dependent variable is a standardized five-item affective polarization index. The index in the bottom panel benchmarks warmth and trust towards the outparty against inparty warmth and trust (Affective Polarization); the index in the top panel (Outparty Only) does not. Observations are weighted by the product of the inverse probability of non-attrition and the sample weights provided by YouGov.

interest in future Braver Angels programming, it apparently did not convince them to forfeit any of their expected compensation for watching a relatively long documentary. It is possible that we would have observed more positive effects on donations if we had provided respondents with a windfall separate from their compensation, but we cannot be sure.

Secondary Outcomes: Democratic Optimism and Anti-Democratic Attitudes

Relative to placebo, we find that the Braver Angels documentary increased participants' optimism about the survival of democratic institutions ($\beta = 0.319$, $p = 0.001$, $N = 583$) and the restoration of civility between Democrats and Republicans ($\beta = 0.431$, $p = 0.000$, $N = 583$), and also strengthened participants' belief in the efficacy of dialogue as a tool for change ($\beta = 0.457$, $p = 0.000$, $N = 528$). While the documentary moderately increased participants' belief that non-violent change is possible, this effect is substantively smaller and not statistically significant at conventional levels ($\beta = 0.176$, $p = 0.135$, $N = 583$). Figure 3 displays these results.[26]

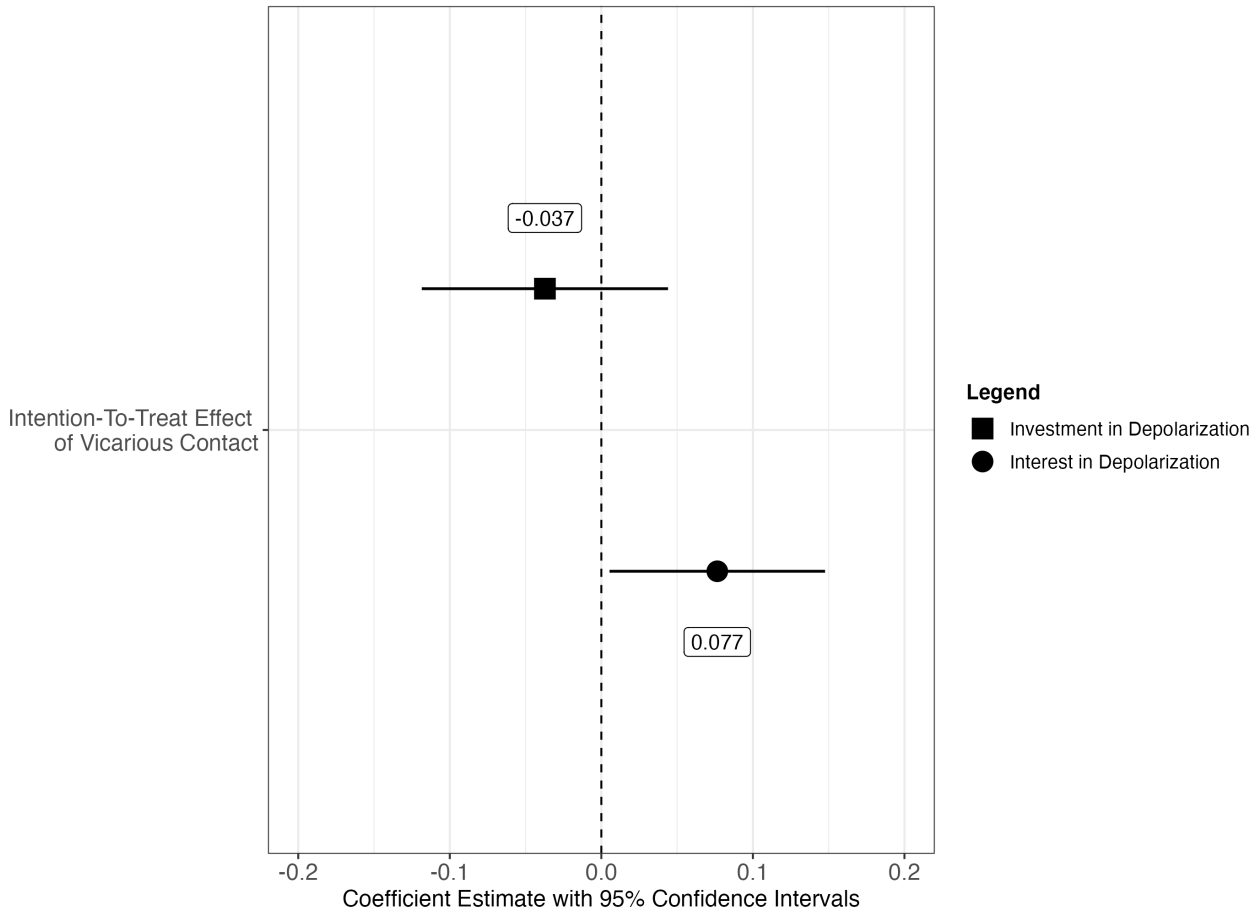
Contrary to our hypotheses, however, we find no evidence that the documentary mitigated (or exacerbated) anti-democratic attitudes ($\beta =$ -0.080, $p = 0.263$, $N = 583$). Figure 4 displays these results. This is consistent with other studies showing that interventions that reduce affective polarization do not necessarily affect attitudes towards democracy (Baron et al., 2025; Voelkel et al., 2023). Taken together, these results suggest that while interventions aimed at mitigating polarization may not have a direct effect on participants' willingness to serve as checks on anti-democratic behavior among elites, they may engender optimism about democracy and bolster commitment to dialogue.

Mechanisms: Stereotypes, Mass Perceptions of Policy Differences, Outgroup Empathy, and Open-Mindedness

In our PAP, we specified several possible mechanisms that might explain the documentary's effects on affective polarization and support for depolarization as a goal. Figure SI.8 in the SI presents the

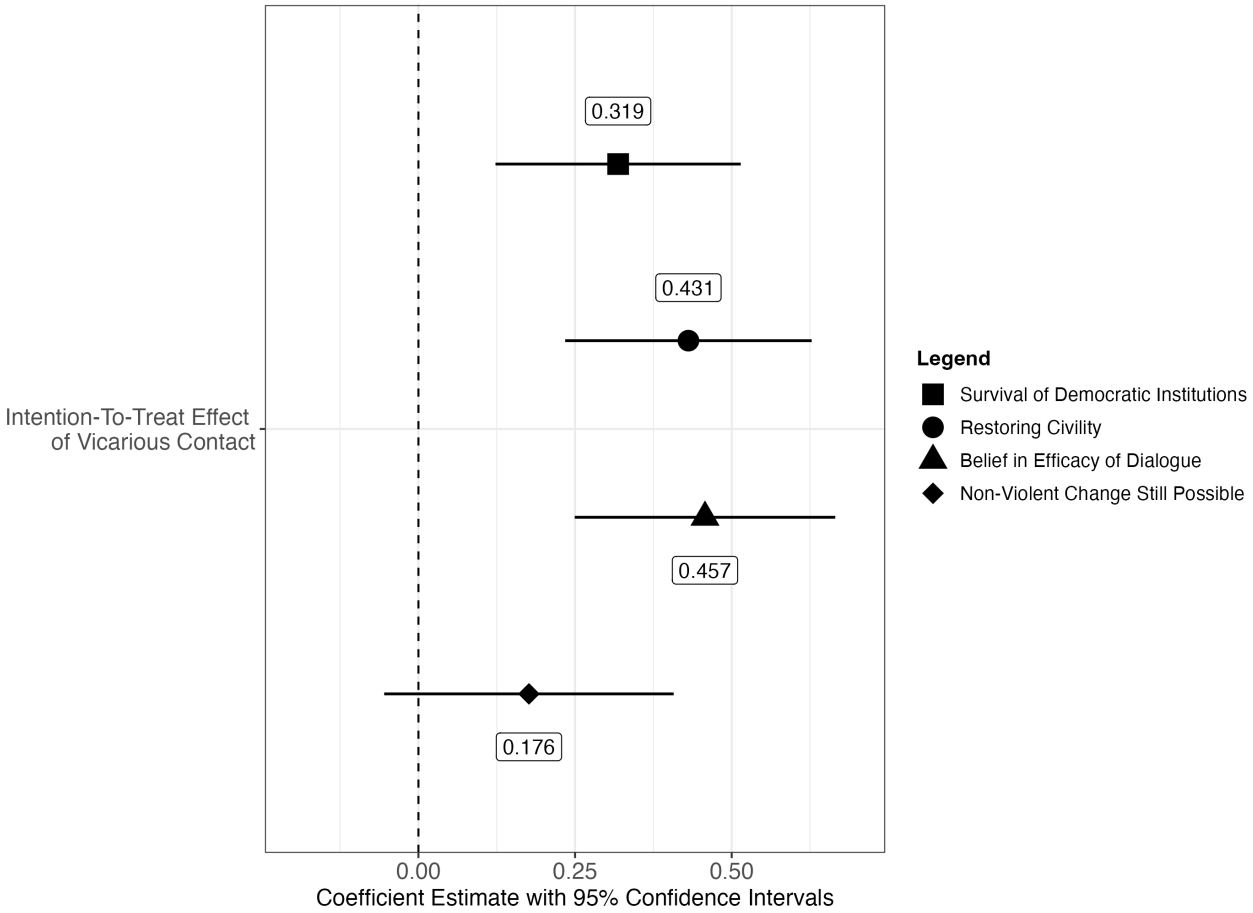
[26]These results are robust to an alternative specification in which we construct an index of these four measures by standardizing and then averaging them ($\beta = 0.332$, $p = 0.000$, $N = 581$).

Figure 2: Vicarious contact increases interest but not investment in depolarization activities



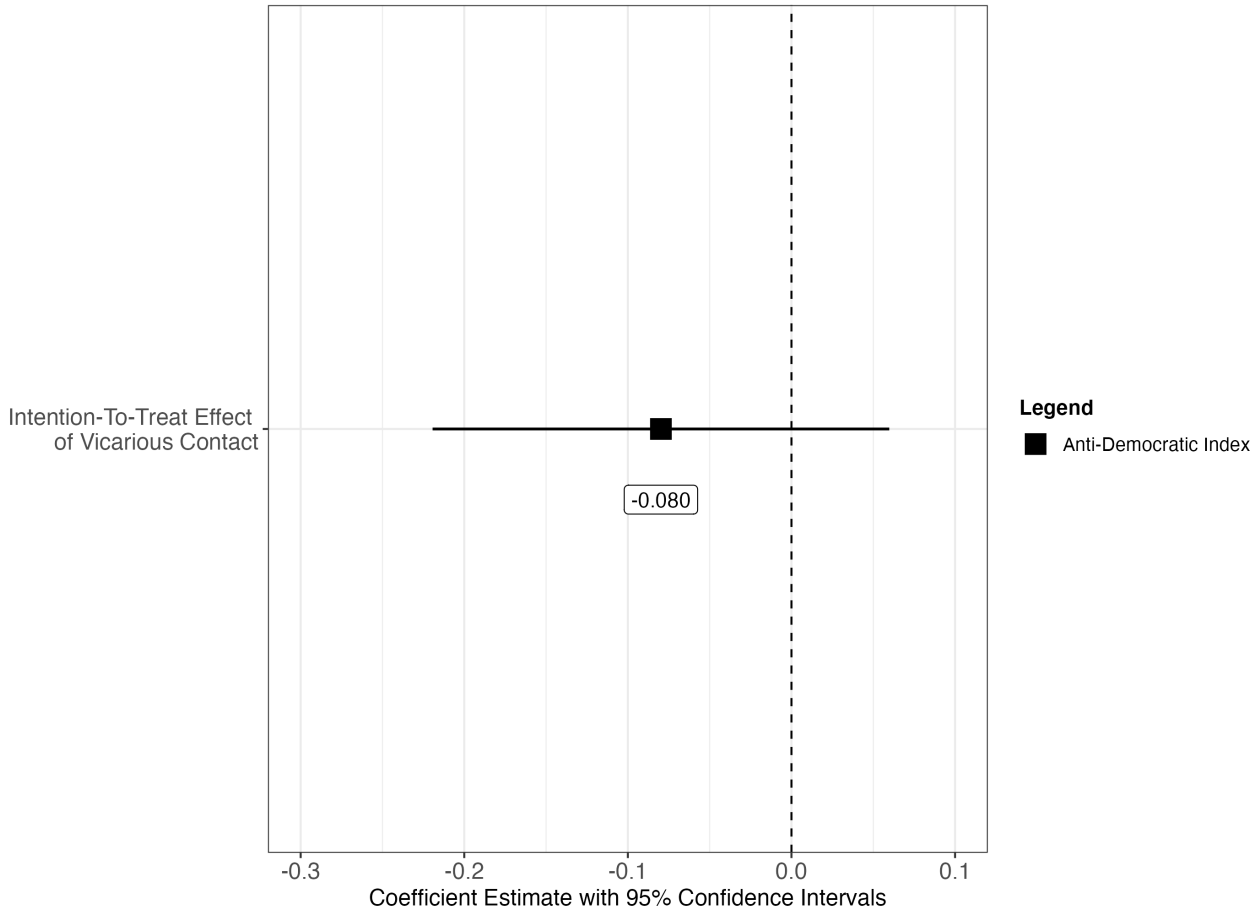
Notes: Intention-to-treat estimates from weighted OLS regressions comparing the effect of the Braver Angels documentary to the effect of a placebo nature documentary among a nationally representative sample of Americans ($N = 583$ for top panel; $N = 509$ for bottom panel). Symbols denote coefficients; lines denote 95% confidence intervals. The dependent variable for investment (top panel) is a binary indicator of whether participants donated any of their compensation to one of three depolarization organizations (Braver Angels, AllSides for Schools, and Living Room Conversations). The dependent variable for interest in depolarization (bottom panel) is a binary indicator of whether participants clicked a link to sign up for the Braver Angels newsletter. Observations are weighted by the product of the inverse probability of non-attrition and the sample weights provided by YouGov.

Figure 3: Vicarious contact increased optimism and strengthened belief in the efficacy of dialogue



Notes: Intention-to-treat estimates from weighted OLS regressions comparing the effect of the Braver Angels documentary to the effect of a placebo nature documentary among a nationally representative sample of Americans ($N = 583$ for all models except for belief in efficacy of dialogue, where $N = 528$). Symbols denote coefficients; lines denote 95% confidence intervals. The dependent variables are standardized measures of participants' optimism about (1) the survival of democratic institutions; (2) the restoration of civility and goodwill between Democrats and Republicans; (3) participants' belief in the efficacy of dialogue; (4) and participants' belief that non-violent change is still possible. Observations are weighted by the product of the inverse probability of non-attrition and the sample weights provided by YouGov.

Figure 4: Vicarious contact does not affect support for anti-democratic actions



Notes: Intention-to-treat estimates from weighted OLS regressions comparing the effect of the Braver Angels documentary to the effect of a placebo nature documentary among a nationally representative sample of Americans ($N = 583$). Symbols denote coefficients; lines denote 95% confidence intervals. The dependent variable is an index of six items measuring anti-democratic attitudes. These six items were standardized and averaged. Observations are weighted by the product of the inverse probability of non-attrition and the sample weights provided by YouGov.

results of these analyses. Consistent with our hypotheses, we find that the Braver Angels documentary strengthened positive stereotypes about the outparty ($\beta = 0.201$, $p = 0.062$, $N = 581$) while weakening negative ones (β = -0.202, $p = 0.043$, $N = 581$), though the former effect is only weakly statistically significant at conventional levels.[27] We believe that stereotyping is one plausible mechanism through which vicarious contact reduces partisan animosity. Similarly, the film improved measures of open-mindedness ($\beta = 0.196$, $p = 0.093$, $N = 488$) and empathy for the outgroup ($\beta = 0.227$, $p = 0.089$, $N = 489$), although both of these effects are only weakly statistically significant at conventional levels.

Contrary to our hypotheses, however, we find no evidence that the documentary mitigated participants' belief that Democrats and Republicans are irreconcilably divided on key policy issues (β = -0.036 , $p = 0.550$, $N = 536$). This is true even when breaking this measure down by issue: abortion (β=0.080, p=0.484, N=518), paid family leave (β = -0.139, $p = 0.180$, $N = 488$), or same-sex marriage (β = -0.101, $p = 0.365$, $N = 507$). While not decisive, these results cast doubt on perceived partisan agreement—at least with respect to policy issues—as a mechanism through which vicarious contact reduces affective polarization. We also find no evidence that the film affected respondents' perceptions of division or unity between Republicans and Democrats ($\beta = 0.168$, $p = 0.169$, $N = 545$). These results suggest that stereotyping, open-mindedness, and empathy for the outgroup may serve as mechanisms through which vicarious contact reduces affective polarization, while perceived similarity of policy positions or perceptions of division and unity more generally do not. Vicarious contact may persuade participants of the value of dialogue across partisan lines without erasing perceived, or real, disagreements between partisan groups.

This combination of findings should help assuage concerns that the documentary may have created a false sense of cross-partisan unity or fostered overly optimistic perceptions of cross-partisan agreement on divisive issues. Viewers appear to have become more convinced of the value of cross-partisan dialogue, more optimistic about the prospects for civility and democratic stability in the future, less prone to stereotyping out-partisans, and more inclined towards cross-partisan empathy and open-

[27]In our PAP we pre-registered that we would subtract stereotypes about the inparty from stereotypes about the outparty. Figure SI.8 instead focuses on stereotypes about the outparty alone, as this is more consistent with the prevailing approach to studying polarization in the literature (Hartman et al., 2022). We report results using our pre-specified approach in SI I.2.

mindedness, without also becoming pollyannaish about the cleavages that continue to divide the two parties. These mixed results should also mitigate concerns about social desirability bias, since we find beneficial effects on some outcomes that might be susceptible to social desirability concerns (e.g., optimism about the survival of democratic institutions) but null effects on others (e.g., belief in the possibility of non-violent change). If social desirability bias explained our results, we would expect to observe consistently beneficial effects across these outcomes. But we do not.

Extensions: Treatment Effect Heterogeneity and Persistence Over Time

Does the effect of vicarious contact vary by participants' partisan and ideological leaning? Our results suggest that it might, though our heterogeneous treatment effect analyses have lower statistical power than our ITT analyses, so we interpret them more cautiously. For compactness, we focus on affective polarization, our primary attitudinal outcome, in most of our analyses of treatment effect heterogeneity. In Figure 5 we show that the film's beneficial effect on affective polarization is driven disproportionately by self-identified Democrats (β = -0.191, $p = 0.047$, $N = 584$). Among Republicans, the effect is substantively small and statistically indistinguishable from zero (β = -0.070, $p = 0.461$ N = 584), though, importantly, the difference between these two conditional average treatment effects (CATEs), captured by the coefficient on the interaction term, is not statistically significant (β = 0.121, $p = 0.369$, $N = 584$). We show in SI I.3.5 that our results are substantively similar when we include covariates to partial out observable differences between Democrats and Republicans. (These latter analyses with covariate adjustment were not prespecified.)

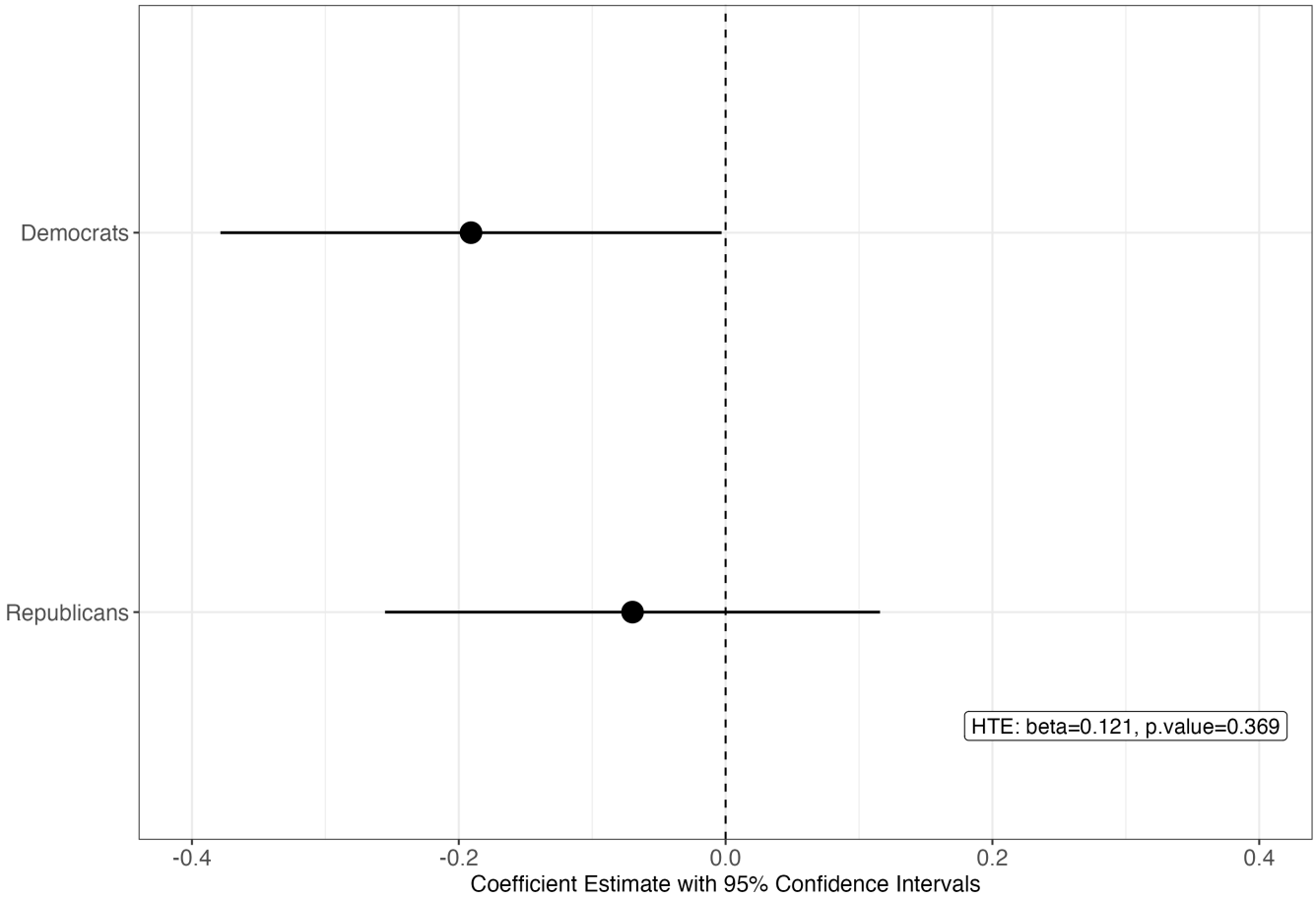
Additional analyses examining treatment effect heterogeneity by ideology (i.e., liberal or conservative) and relative confidence in liberal vs. conservative media outlets are consistent with our findings above: the negative ITT on affective polarization appears to be concentrated among more liberal participants, and among participants who express more trust in liberal media outlets, though again, the coefficients on the interaction terms are not consistently statistically different from zero, and we are careful not to over-interpret the CATEs to imply that the documentary is only effective among Democrats and liberals. (We present these figures in SI I.) Finally, SI I.3.2 presents CATEs on optimism and anti-

democratic attitudes by party ID. While we find no evidence of statistically significant differences between sub-groups, the results do offer directions for future research. For instance, it appears that the film mitigated anti-democratic attitudes among self-identified Republicans (though again, we interpret these results cautiously due to low statistical power). Examining whether different depolarization strategies are needed for Democrats and Republicans strikes us as a promising avenue for future research.

Finally, we test whether the effect of vicarious contact on affective polarization decays over time using wave 3 of the survey, administered approximately 50 days after treatment. As we discuss in SI G, we observe statistically significant differential attrition between the Braver Angels and placebo nature documentary groups in wave 3, raising the risk of bias in our treatment effect estimates. Fortunately, we have access to a rich array of pre-treatment covariates that we can use to diagnose and attempt to correct for differential attrition using IPW. Figure 6 suggests that the effect observed in the short term decays to a statistical null over time. This is true for both our pre-registered affective polarization index (β = -0.115, $p = 0.110$, $N = 506$) and our alternative index focused on outparty animus only (β = -0.119, $p = 0.143$, $N = 506$). These wave 3 ITT estimates for the two indices are, respectively, 82% and 73% as large as their corresponding wave 2 ITT estimates. While this evidence of decay is consistent with our priors, we interpret it somewhat cautiously due to the aforementioned differential attrition.

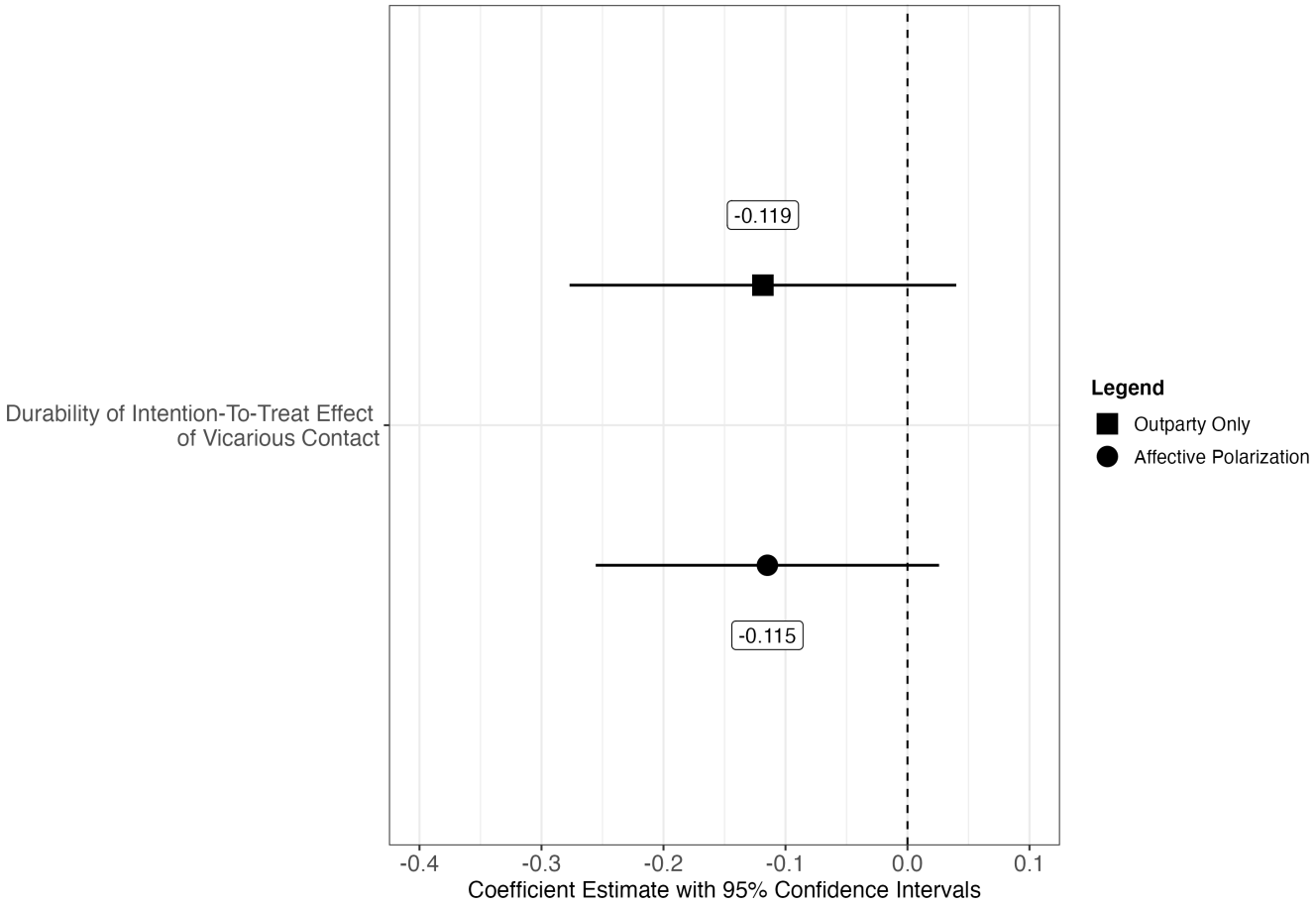
Finally, SI I.3.4 presents wave 3 results for the optimism outcomes. While the treatment effect appears to decay for most of these outcomes, the ITT on belief in the efficacy of dialogue remains substantively large and statistically significant, even 50 days after the intervention ($\beta = 0.364$, $p = 0.001$, $N = 465$). This estimate is approximately 80% as large as the wave 2 ITT estimate, indicating a rate of decay similar to the rate for our affective polarization outcomes.

Figure 5: Vicarious contact reduced affective polarization primarily among Democrats



Notes: Conditional average treatment effect estimates for Democrats and Republicans from weighted OLS regressions comparing the effect of the Braver Angels documentary to the effect of a placebo nature documentary among a nationally representative sample of Americans ($N = 584$). Heterogeneous treatment effect estimates are derived from the interaction between treatment assignment and an indicator for party ID. The interaction coefficient is reported in the label at the bottom right. Symbols denote coefficients; lines denote 95% confidence intervals. The dependent variable is a standardized five-item affective polarization index. Observations are weighted by the product of the inverse probability of non-attrition and the sample weights provided by YouGov.

Figure 6: The effect of vicarious contact on affective polarization decayed over time



Notes: Intention-to-treat estimates from weighted OLS regressions comparing the longitudinal effect of the Braver Angels documentary to the longitudinal effect of a placebo nature documentary among a nationally representative sample of Americans ($N = 506$), measured an average of 50 days following treatment. Symbols denote coefficients; lines denote 95% confidence intervals. The dependent variable is a standardized five-item affective polarization index. The index in the bottom panel benchmarks warmth and trust towards the outparty against inparty warmth and trust (Affective Polarization); the index in the top panel (Outparty Only) does not. Observations are weighted by the product of the inverse probability of non-attrition and the sample weights provided by YouGov.

Discussion

We conducted a pre-registered, placebo-controlled trial to test whether a mass media vicarious intergroup contact intervention—delivered in the form a 50-minute documentary film—can depolarize a nationally representative sample of Americans. We find that vicarious contact reduced affective polarization and increased interest but not investment in depolarization activities approximately five days after exposure. While vicarious contact did not mitigate anti-democratic attitudes, it did increase participants' optimism about the survival of democratic institutions and the ability of Americans to overcome polarization, and strengthened participants' belief in the efficacy of dialogue as a tool for change. Our results suggest that the documentary may have reduced affective polarization by correcting stereotypes of out-partisans; we find more suggestive evidence that it may have also increased open-mindedness and built empathy towards out-partisans. We find no evidence that the documentary reduced affective polarization by instilling the perception that Americans are less divided along partisan lines than participants previously believed, or that they are less divided on specific policy issues. We also find that the effects appear to be concentrated among participants who self-identified as Democrats, as ideologically liberal, and as more trusting of liberal media outlets.

Practically, what do these results suggest about the potential of vicarious contact to achieve depolarization at scale? One way to estimate cost-effectiveness is to benchmark our ITT estimates against a previous study evaluating the *same* in-person direct contact workshop featured in the documentary, run by the same organization (Baron et al., 2025). Recently, Littman et al. (2023) have called for precisely this type of benchmarking in intergroup contact research. The comparison is illustrative but should be interpreted with caution: while our study and Baron et al. (2025) use very similar measures to operationalize attitudinal affective polarization, the samples, experimental comparisons, and timing of outcome measurement differ. Baron et al. (2025) sampled college students, compared participants who were assigned to the workshop to an empty control, and measured outcomes 14 days after treatment; our study uses a nationally representative sample of Americans, compares participants who were assigned to the documentary to a placebo, and measures outcomes an average of

5 days following treatment.[28] Moreover, we view the workshops and documentary as complements rather than substitutes, as we discuss below. With this caveat in mind, using our pre-specified version of the affective polarization index, the workshop and documentary reduced affective polarization by 0.184 and 0.140 standard deviations, respectively. This implies that the effect of the 50-minute documentary is roughly three-quarters (approximately 76%) of the magnitude of the effect of an all-day in-person workshop.[29]

The effects of the documentary thus appear to be both smaller and less durable relative to the in-person workshop. In this sense, the documentary is perhaps best used as a booster to prior in-person contact or as a gateway to further engagement. That said, the potential for scale offered by the documentary remains compelling, especially relative to more standard interventions involving real in-person or virtual contact that have higher costs in terms of staff and participant time and reach a fraction of the people. Each in-person Braver Angels workshop of the type documented in the film brings together a maximum of 32 people[30] at a time for a full day, while 351 Americans watched the 50-minute documentary as part of our study alone. This is roughly equivalent to the reach of 10 workshops achieved in 12% of the time and at a fraction of the cost. By way of illustration, organizing 1,000 workshops reaches 32,000 people and is estimated to cost $1,130,000 ($1,130 per workshop, according to Braver Angels). Alternatively, even if one were to actively pay people $10 (well above the federal minimum wage of $7.25/hr) to watch the film, one would reach 113,000 people with the same amount of money.

Of course, other options for distribution are available and are arguably more realistic. According to Google Ads, a highly conservative digital advertising rule-of-thumb for a general U.S. population suggests that the average cost-per-view of a YouTube video is $1 per one view. This suggests that, for every dollar spent on advertising, we can reasonably expect a yield of one view of the documentary.

[28] The ideal way to compare the effects of vicarious and in-person contact would be to randomize participants from the same underlying population to one form of contact or the other. This was not possible for a combination of logistical and financial reasons. Our benchmarking exercise is a second best alternative.

[29] We describe the mechanics of this benchmarking exercise in further detail in SI J.

[30] Braver Angels "Red/Blue" workshops typically include 16 participants—8 Democrats and 8 Republicans—and 16 observers, though these numbers vary somewhat from workshop to workshop. These workshops are also offered in an online format which somewhat reduces time costs, but staff time and participant limits are similar.

Even if we assume that half the people who begin watching the documentary will not finish it, with a $1,000,000 budget, a depolarization intervention could still reach 500,000 Americans.[31]

An even more cost-effective approach would be mass dissemination over popular television networks or streaming platforms; indeed, this is what our partners had in mind when they asked us to evaluate the documentary's impact. For example, films that stream on Netflix are usually purchased by the company from the creators but are not normally advertised, thus reducing costs. While it is impossible to know how the Braver Angels documentary would be received, by way of illustration, Netflix provides a publicly available breakdown of the number of views of films on its platform.[32] In the 6-month period between July and December 2023, Netflix hosted 9,395 films. The lowest number of views for any film in that 6-month period, as well as the modal number, was 100,000. Approximately 40% of films had 100,000 views. A few documentaries analogous in length and quality of production (though not subject matter) to the one we study here had between 200,000 and 300,000 views.[33]

Equally important, this type of mass distribution of films highlighting vicarious contact across partisan lines also has very recent precedent. For example, in 2021, a documentary featuring vicarious contact, called the *Reunited States*, was nationally distributed on multiple major platforms.[34] Other content, such as a long-running CNN series called *The United Shades of America*, has also regularly featured footage of positive interactions across the partisan divide. Distribution of *Reunited States* was conducted with support from our partner, Braver Angels, and community screenings were funded by private foundations. A similar approach—leveraging horizontal networks such as Braver Angels' local chapters (which exist in all 50 states)—could be used to disseminate the documentary we study here, which has already been viewed 27,000 times on the Braver Angels YouTube channel without any mass distribution or advertising at all.

[31]These estimates are based on our own conversations with representatives from Google Ads in November 2024.

[32]The data is publicly available at `https://about.netflix.com/en/news/what-we-watched-the-second-half-of-2023`.

[33]One potentially comparable film is *Waking the Titanic*, a 2012 documentary about Irish emigrants on the Titanic. It is 52 minutes long and has 200,000 views on Netflix. It is also available for free on YouTube, where it has 258,000 views (`https://www.youtube.com/watch?v=x5GjYR2Wk4g`). A second potentially comparable film is *Lee Kuan Yew: In His Own Words*, a 46-minute 2023 documentary about the first prime minister of Singapore that is also available on YouTube for free (`https://www.youtube.com/watch?v=whRN-CJZDr0`). It has 300,000 views on Netflix and 1̃.5 million on YouTube. A third potentially comparable film is *Costco: Is It Really Worth It?*, a 43-minute film about whether a Costco membership is worth the cost. It has 300,000 views on Netflix.

[34]More information on the film is available at `https://reunitedstates.tv/`.

At the same time, any type of scaling is not without barriers. While some political and media elites may express interest in depolarization or bipartisanship, many elites are motivated to polarize the electorate for political or financial gain. They therefore may be uninterested in (or even opposed to) the distribution of depolarizing content, and in the goal of depolarization more broadly. Depolarizing content may also provoke some amount of backlash as it reaches larger and larger swaths of the highly polarized American public. Of course, these headwinds are likely to hinder any depolarization effort conducted at scale.

Another barrier to scale is simply the length of the documentary and its ability to hold the attention of someone with minimal interest in depolarization. It was for this reason that we developed 5-minute versions of the documentary. While we cannot make robust comparisons given differential attrition between the films of different lengths, we nevertheless find no evidence that the long documentary was more effective than the short ones in reducing affective polarization or generating commitment to depolarization (as measured by donations). The results are presented in Figure SI.14. We do find, however, that the long documentary was more effective at generating interest in depolarization (as measured by newsletter signups); at strengthening belief in the efficacy of dialogue; and (more weakly) at increasing optimism about the prospects for restoring civility across partisan lines. But again, we interpret these results cautiously given the extent of differential attrition between these experimental conditions.[35]

Conclusion

In sum, our findings suggest that mass media interventions featuring vicarious intergroup contact could serve as a relatively cheap and scalable tool to promote depolarization in the United States, at least in the short term. These interventions can reduce affective polarization at scale, promoting optimism and highlighting the potential for dialogue to bridge divides. By reducing affective polarization and generating interest in future depolarization activities, vicarious contact may also serve as a gateway

[35] As a more exploratory exercise, we compare both short films to the empty control condition using the non-nationally representative sample (where we do not observe differential attrition, see SI G). We find that the short films had no effect on affective polarization but that they did increase interest in depolarization. Table SI.32 presents the results.

to other forms of exposure to, and dialogue with, members of the outparty. We encourage future researchers to continue exploring the promise and boundaries of vicarious contact depolarization interventions.

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Competing interests

The authors declare no competing interests.

Ethics and data availability

The study was approved by the Brown University IRB (protocol #2022003227). All participants provided informed consent and were compensated for their participation in the study. Data files and analysis code for the study will be made publicly available upon publication.

Acknowledgments and declaration of interest

We are grateful to Peter Yarrow for creating the Braver Angels documentary, as well as for his insights and partnership in carrying out this research. Peter passed away on January 7th, 2025, before the publication of this article. Peter was a visionary individual, who pushed us to make academic rigor useful to the public good and humbled us with his wisdom. This article would not have been possible without him and we dedicate it to his memory.

We also thank Braver Angels for allowing their work to be filmed and rigorously evaluated. Funding for this research was provided by Operation Respect. We also thank Unify America for their collaboration. We thank Roman Gallardo for research assistance and Niyoshi Parekh for video editing. The authors declare that they have no competing interests.

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ONLINE SUPPLEMENTARY INFORMATION

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SI A Previous studies of vicarious cross-partisan contact

To our knowledge, four previously published studies have experimentally evaluated the impact of vicarious cross-partisan contact interventions. Two of these studies focus on contact between elites. Huddy and Yair (2021) test the effects of a mock news story describing an interaction between then-Senate majority leader Mitch McConnell, a Republican, and then-Senate minority leader Chuck Schumer, a Democrat, in a Washington, DC restaurant. The nature of the interaction is randomized to be either "warm" or "hostile." The sample is non-representative, and consists of MTurk workers. The study focuses on attitudinal outcomes only, all of them related to perceptions of and affect towards in-partisans and out-partisans. Outcomes were measured immediately after treatment was administered.

Voelkel et al. (2023) test the effects of a video depicting the friendship between Joe Biden, a Democrat, and John McCain, a Republican. The sample consists of Lucid respondents who were quota-matched to be similar on observables to the US adult population. The study appears to include both attitudinal and behavioral measures, but does not report the effects of the video on the behavioral measures. The attitudinal measures capture affect towards out-partisans, support for partisan violence, support for undemocratic politicians, and willingness to prioritize partisan ends over democratic means. The behavioral measures consist of dictator games and joy-of-destruction games with endowments of 50 cents per game. Outcomes were measured immediately after treatment was administered.

Two other studies focus on contact between citizens. Wojcieszak and Warner (2020) test the effects of a mock Yahoo News article describing a cooking competition at a local restaurant to raise money for a community swimming pool. The nature of the competition is randomized such that out-partisans either (a) share the cooking space and enjoy one another's company, (b) work cooperatively to win the competition, or (c) verbally attack each other and have to be separated. The sample consists of Dynasta respondents who were quota-stratified to be similar on observables to the US adult population. The study focuses on attitudinal outcomes only, most of them related to perceptions of and affect towards in-partisans and out-partisans. Other outcomes include feelings of anxiety and empathy while reading the article. Outcomes were measured immediately after treatment was administered.

Finally, Voelkel et al. (2024) test the effects of a 4-minute Heineken ad depicting interactions between pairs

of British citizens with disparate political and ideological views. (The ad does not specify their party affiliation.) The sample consists of Bovitz respondents who were quota-stratified to be similar on observables to the US adult population. The study includes both attitudinal and behavioral measures. The attitudinal measures capture perceptions of and affect towards out-partisans, support for partisan violence, support for undemocratic politicians, support for undemocratic practices, opposition to bipartisan cooperation, and biased evaluation of politicized facts. The behavioral measure consists of a dictator game with an endowment of 50 cents. Outcomes were measured immediately after treatment was administered, then again two weeks later.

SI B Intervention

The present study consists of five arms: a primary treatment arm, two secondary treatment arms, a placebo condition, and a pure control group.

B.1 Full-Length Braver Angels Documentary

Our primary treatment arm is a 50-minute documentary film showcasing a real-life case of intergroup contact—a dialogue workshop conducted by the American depolarization organization Braver Angels, a non-profit which aims to reduce partisan polarization in the United States. The workshop convened a small group of Republicans and Democrats in an Ohio town following the 2016 presidential election. The film includes edited footage from the discussions and activities used in the workshop, post-workshop interviews with the facilitators and participants, as well as scenes showing the joint actions taken by the participants following the workshop. The workshop itself is called a Red-Blue workshop and has since been replicated across the country. Red-Blue workshops bring together a small, evenly divided group of conservatives and liberals, or "reds" and "blues," for a series of exercises designed to help participants clarify disagreements, reduce stereotyped thinking, and discover common values. The workshop is moderated by two trained facilitators.

The full film is freely available for viewing at `https://vimeo.com/565943983` and at `https://www.youtube.com/watch?v=u6kZpN5T3lU`.

B.2 Short Films

Vicarious contact: The first of our secondary treatment arms is an approximately 5-minute short documentary film, using only footage from the full documentary to highlight the intergroup contact that took place during the workshop. It intentionally downplays instances of partisan misperception correction. The film can be viewed here: `https://youtu.be/I1JH60xBPdA`[1].

Vicarious contact + partisan misperception correction: The second short video is an approximately 5-minute short documentary film, again using only footage from the full documentary. It highlights the fact that participants' partisanship-based misperceptions were corrected via the workshop's exercises.

Link: `https://youtu.be/z9XIotkpGvI`.

B.3 Pure Control and Placebo Documentary

Pure control: In addition to the three treatment arms, we also included a pure control group. Participants assigned to this condition went directly from answering demographic questions to the survey debrief.

Placebo documentary: We also included a placebo arm consisting of a 50-minute publicly available nature documentary. We sought to include a film that has roughly equivalent length and production value relative to the Braver Angels documentary. The film focuses on the migration of the wildebeest and has no discernible connection to politics or the United States. The video is available at `https://www.youtube.com/watch?v=zEp5H12vGm4`.

B.4 Nudges

In addition, at the end of each film we added three different kinds of behavioral "nudges." This meant that after participants were randomized into one of the three treatment arms, they were then randomly assigned one of three versions of the film—each with a different "nudge" ending.

Our logic was that in a highly polarized environment like the present-day US, citizens may be skeptical of depolarization interventions. The nudges are designed to address participants' potential reservations about

[1]Note that this version includes the social proofing nudge, see below for full description

these activities. Specifically, they address three possible barriers to participation: (1) uncertainty about the extent to which depolarization is a normative and widespread activity (social proofing); (2) doubts about the effectiveness of depolarization workshops; and (3) exaggerated beliefs about the degree to which outparty members dislike/hate one's own group (meta-affective polarization). Each nudge ends with an encouragement to sign up to participate in a depolarization activity. Nudges appeared as text at the end of the treatment videos, as follows:

Social proofing: Braver Angels and other depolarization organizations are active in all 50 states, including your state. Sign up to participate in a depolarization activity!

Efficacy: Research shows that depolarization workshops, like the one shown in the film, successfully reduce hostility between Republicans and Democrats. Sign up to participate in a depolarization activity!

Meta-affective polarization: Research shows that Republicans and Democrats overestimate how much members of the opposite party hate them. Sign up to participate in a depolarization activity!

These nudges were exploratory components of our study. We marginalize over them when reporting our treatment effect estimates for the Braver Angels documentary.

SI C Sampling Strategy

Our main results in the paper are based on a nationally representative sample recruited from YouGov's online panel. YouGov uses a sample matching methodology to create a nationally representative sample. Sample selection using the matching methodology is a two-stage process. First, a random sample is drawn from the target population (the "target sample"). This is a true probability sample and thus representative of the frame from which it was drawn. Second, for each member of the target sample, one or more matching members are selected from the pool of opt-in respondents (the "matched sample"). Matching is accomplished using a large set of variables that are available in consumer and voter databases for both the target population and the opt-in panel. The purpose of matching is to find an available opt-in respondent who is as similar as possible to the selected member of the target sample. The result is a sample of opt-in respondents who have the same measured characteristics as the target sample.

YouGov uses the proximity matching method to construct the matched sample. For each variable used for matching, they define a distance function, $d(x, y)$, which describes how "close" the values x and y are on a particular attribute. The overall distance between a member of the target sample and a member of the panel is a weighted sum of the individual distance functions on each attribute. For our study, over 150,000 respondents to YouGov's Internet surveys were used for the pool from which to construct the matches for the final sample. The YouGov sampling frame is based upon the 2019 American Community Survey (ACS) public use microdata file, with additional modeled voting behavior variables using data from public voter file records, the 2020 Current Population Survey (CPS) Voting and Registration supplements, the 2020 National Election Pool (NEP) exit poll, and the 2020 CES surveys, including demographics and 2020 presidential vote.

The matched cases were weighted to the sampling frame using propensity scores. The propensity score function included age, gender, race/ethnicity, years of education, and region. Propensity scores were grouped into deciles and post-stratified, such that the sample was weighted to match the characteristics of the population across each decile. The weights were then post-stratified on 2016 and 2020 presidential vote choice, and a four-way stratification of gender, age (four categories), race (four categories), and education (4-categories), to produce the final weight. This final weight is a composite adjustment that combines the initial propensity score decile adjustments, aligns the sample to the known distributions of 2016 and 2020 presidential vote choice, and then adjusts for the joint distribution of gender, age, race, and education. The matched sample for our study consists of 2,000 opt-in respondents in wave 1, 1,600 in wave 2, and 1,360 in wave 3. We discuss attrition between waves in detail in Section G below.

SI D Descriptive Statistics

YouGov reports interviewing 2,630 participants in three waves, recruited from YouGov's nationally representative panel. Of those, 3 participants are totally missing. The initial, full sample is composed of 2,627 participants who were randomly assigned to five different treatment groups (full-length Braver Angels documentary, placebo documentary, short video-PMC, short video-VC, and pure control). After eliminating duplicate participants, the actual number of participants assigned to treatment is 2,573.

Wave 1 was fielded between June 27, 2022 and July 15, 2022. Participants were then recontacted 72 hours later for participation in wave 2. Wave 3 was fielded approximately one month following the completion of

wave 2, between August 16, 2022 and August 22, 2022.

Separate weights for a nationally representative sample are available for wave 1 (N = 2,000), wave 2 (N = 1,600), and wave 3 (N = 1,360). Wave 1 and 2 weights are "matched down." All participants assigned to treatment were recontacted at waves 2 and 3, irrespective of their compliance with the treatment and completion of previous surveys.

In the tables below, "Matched sample" refers to the nationally representative sample, which is used in the main specifications presented in our manuscript and is the sample we pre-registered. "Full sample" is used to describe all participants assigned to treatment. We include descriptive statistics for the full sample as well.

Table SI.1: Descriptive statistics

	Matched Sample			**Full Sample**		
	N	Mean	SD	N	Mean	SD
Republican	812	0.41	0.49	1059	0.41	0.49
Democrat	1188	0.59	0.49	1514	0.59	0.49
Age	2000	50	18	2573	48	19
Male	929	0.46	0.5	1168	0.45	0.5
Female	1071	0.54	0.5	1405	0.55	0.5
White	1418	0.71	0.45	1786	0.69	0.46
Black	212	0.11	0.31	284	0.11	0.31
Hispanic	189	0.095	0.29	254	0.099	0.3
Asian	52	0.026	0.16	65	0.025	0.16
College	679	0.34	0.47	866	0.34	0.47

† Descriptive statistics are based on pretreatment demographic information collected at wave 1. Matched sample refers to the nationally representative sample and is based on the participants assigned sampling weights ($n = 2,000$). Full sample is based on all participants assigned to treatment ($n = 2,573$).

Table SI.2: Descriptive statistics: dependent variables

	Matched Sample			**Full Sample**		
	Mean	SD	N	Mean	SD	N
Affective Polarization	0.015	0.64	1826	-0.0016	0.63	2105
Affective Polarization (Outparty Animus)	0.016	0.65	1826	-0.0015	0.65	2105
BA Newsletter Clicks	0.088	0.28	1622	0.091	0.29	1827
All Donations	14	33	1814	17	36	2090
Anti-Democratic Attitudes	-0.031	0.67	1819	-4.2e-05	0.68	2094
Positive Outparty Stereotypes	4	1.8	1816	3.9	1.8	2091
Negative Outparty Stereotypes	3.2	2.4	1816	3.3	2.4	2091
Mass Perceptions: Abortion	34	44	1618	32	44	1825
Mass Perceptions: Marriage	31	42	1594	30	42	1796
Mass Perceptions: Parental Leave	26	35	1527	25	35	1733
Optimism: survival of democratic institutions	2	1.2	1818	2	1.2	2094
Non-violent change is possible	2.4	1.2	1819	2.4	1.1	2094
Optimism: restoring civility	2	1.2	1819	2	1.2	2095
Dialogue as effective tool for change	55	26	1676	55	26	1889

† Descriptive statistics of outcomes are based on outcome data collected at wave 2. Matched sample refers to the nationally representative sample and is based on the participants assigned sampling weights ($n = 2,000$). Full sample is based on all participants assigned to treatment ($n = 2,573$). Affective polarization, affective polarization (outparty animus), and anti-democratic attitudes are all indexed. All other outcomes are not indexed or standardized.

D.1 Participants by Condition at each Wave

D.1.1 Nationally Representative Sample

Table SI.3: Participant Count at Each Wave of the Study: Nationally Representative Sample

Treatment Arm	N	Wave 2 Attriters	N Wave 2	Wave 3 Attriters	N Wave 3
Empty Control	458	(99)	359	(156)	302
Placebo Control	380	(82)	298	(111)	269
Treatment Full Film	351	(65)	286	(112)	239
Treatment Short PMC	405	(78)	327	(133)	272
Treatment Short VC	406	(76)	330	(128)	278

† Attriters in the nationally representative sample are those participants who did not respond to our affective polarization questions or who were not assigned sampling weights by YouGov

D.1.2 Full Sample

Table SI.4: Participant Count at Each Wave of the Study: Full Sample

Treatment Arm	N	Wave 2 Attriters	N Wave 2	Wave 3 Attriters	N Wave 3
Empty Control	506	(60)	446	(169)	337
Placebo Control	514	(113)	401	(188)	326
Treatment Full Film	524	(146)	378	(235)	289
Treatment Short PMC	514	(83)	431	(196)	318
Treatment Short VC	515	(66)	449	(173)	342

† Attriters in the full sample are those participants who do not have responses to affective polarization index

D.2 Affective Polarization Index Correlation Tables

FT refers to Feeling Thermometer. Friends, Marriage, and Neighbors all refer to variables asking participants how much discomfort they would feel if members of the outparty would be their [Friends/Best friend's spouse/Neighbors]. Negative_Par refers to the negative partisanship indicator.

Table SI.5: Correlations Between Items of Affective Polarization Index, Wave 2

	Trust	FT	Friends	Marriage	Neighbors	Threat	Negative_Par
Trust	1.00	0.67	0.24	0.24	0.23	0.41	-0.07
FT	0.67	1.00	0.29	0.27	0.29	0.40	-0.09
Friends	0.24	0.29	1.00	0.74	0.73	0.27	0.04
Marriage	0.24	0.27	0.74	1.00	0.70	0.28	0.05
Neighbors	0.23	0.29	0.73	0.70	1.00	0.28	0.05
Threat	0.41	0.40	0.27	0.28	0.28	1.00	0.13
Negative_Par	-0.07	-0.09	0.04	0.05	0.05	0.13	1.00

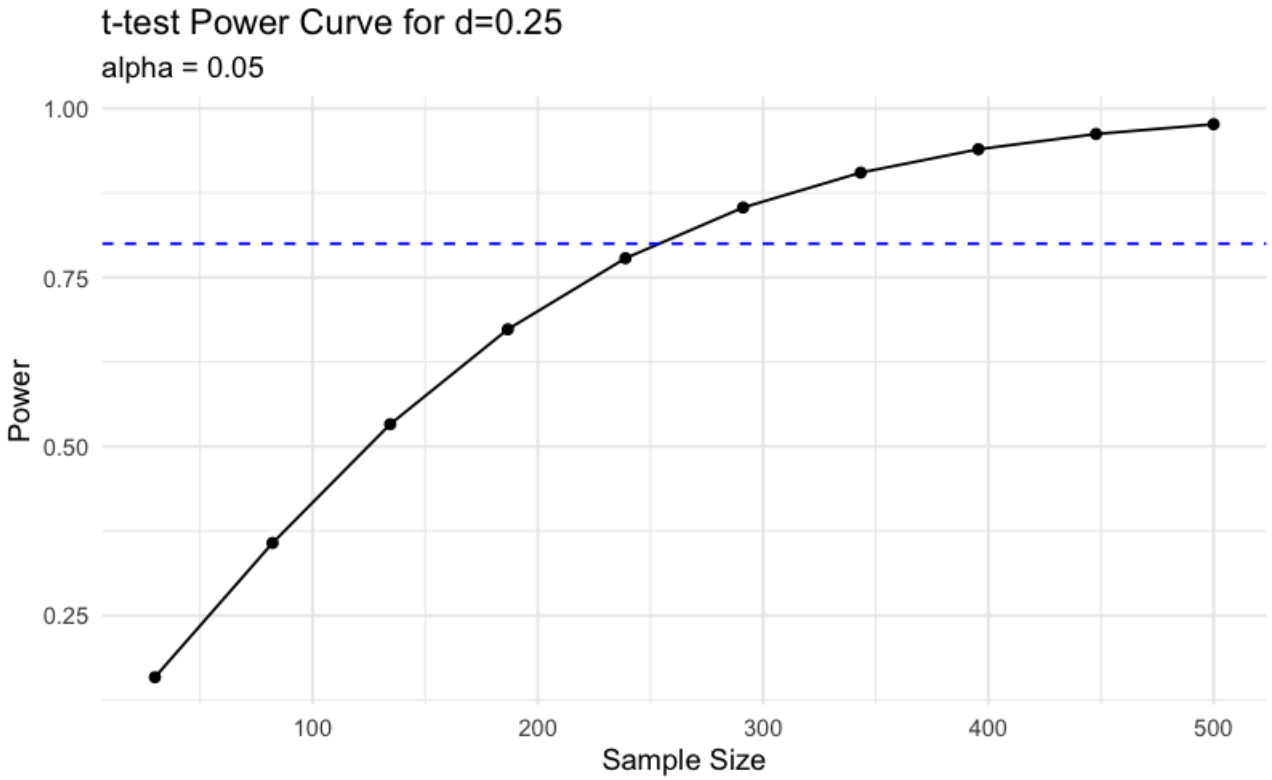
Table SI.6: Correlations Between Items of Affective Polarization Index (outparty only), Wave 2

	Trust (out)	FT (out)	Friends	Marriage	Neighbors	Threat	Negative_Par
Trust (out)	1.00	0.57	0.30	0.30	0.28	0.44	0.11
FT (out)	0.57	1.00	0.32	0.28	0.29	0.41	0.10
Friends	0.30	0.32	1.00	0.74	0.73	0.27	0.04
Marriage	0.30	0.28	0.74	1.00	0.70	0.28	0.05
Neighbors	0.28	0.29	0.73	0.70	1.00	0.28	0.05
Threat	0.44	0.41	0.27	0.28	0.28	1.00	0.13
Negative_Par	0.11	0.10	0.04	0.05	0.05	0.13	1.00

SI E Power Calculations

The sample size was calculated using R's pwr package. For our primary hypotheses, H1.1 and H1.2, we conducted a power analysis based on a two-sided t-test, assuming a minimum detectable effect (MDE) of Cohen's $d = 0.25$, with $\alpha = 0.05$, at a conventional power of 0.8. Given these parameters, our study requires a sample of 253 participants per condition. A more conservative MDE of $d = 0.2$ requires a sample of 393 per condition. Given concerns about attrition we opted to increase our initial sample size to 400 per condition. See Figure SI.7.

Figure SI.7: Power calculations for testing primary hypotheses



We can also re-estimate our minimum detectable effects retrospectively using the observed standard errors on our ITT estimates. We do this in several ways. First, we simply multiply the standard error on our ITT estimates in columns 1 and 2 of Table SI.12 (s=0.07) by 2.8—the rule of thumb proposed by Gelman and Hill 2006. Using this procedure, we find that we have 80% power to detect an effect of $d = 0.2$. This is very similar to the results of the power calculations in our PAP.

Next, we use the "post-hoc" approach proposed by Gelman and Carlin (2014). The most important challenge

with this approach is to identify a plausible hypothesized effect size. We consider several possibilities. First, we use the conservative hypothesized effect size from our PAP ($d = 0.2$). Again, given the standard error on our ITT estimates in columns 1 and 2 of Table SI.12 ($s = 0.07$), we find that we have 81.5% power to detect an MDE of $d = 0.2$ using the Gelman and Carlin (2014) procedure.[2]

Second, we use the overall meta-analytic effect size estimate from the 73 highest-powered studies in Paluck et al.'s (2021) review of prejudice reduction research ($d = 0.19$). This is a hard and possibly inappropriate benchmark, given that Paluck et al. (2021, 537) explicitly exclude studies of partisan animosity from their review, and given that d=0.19 is one of the smallest meta-analytic effect sizes that they report. Nonetheless, using Paluck et al. (2021) as a benchmark, we find that we have 77% power to detect an MDE of $d = 0.19$, which is very close to the conventional 80% threshold.

Finally, we use as benchmarks the ITT estimates in Voelkel et al.'s (2024) "megastudy" of "light-touch" depolarization interventions. As noted above, we are powered at 81.5% to detect an effect of $d = 0.2$, which is roughly equivalent in magnitude to the effect of the 14th most successful intervention in Voelkel et al. (2024). Given that these interventions were all 8 minutes long (or shorter), it seems reasonable to assume that a 50-minute documentary would have an effect at least as large as the 14th most successful intervention in Voelkel et al. (2024). Based on these estimates, we conclude that the power calculations in our PAP were reasonable, and that our study was adequately powered to detect effects within the range of plausible MDEs based on the existing prejudice reduction and depolarization literatures.

[2]We implement this procedure using the retrodesign package in R.

SI F Balance Tests

Balance tests report coefficients from OLS regressions in which assignment to treatment is the dependent variable and several pretreatment covariates are the independent variables for wave 1 of the study. Other than a binary variable indicating whether participants have a 4-year college degree in the documentary vs. placebo nationally representative sample (table SI.7), none of the covariates reach statistical significance in predicting treatment assignment. F-statistics for all four models do not reach statistical significance.

F.1 Nationally Representative Sample

Table SI.7: Balance Test using Nationally Representative Sample

	Dependent variable: Assignment to Treatment	
	Full Film vs. Placebo	Any Treatment vs. Any Control
Party ID	−0.022	0.033
	(0.044)	(0.026)
Age	0.001	0.0001
	(0.001)	(0.001)
White	−0.019	−0.008
	(0.074)	(0.046)
Black	0.079	0.009
	(0.092)	(0.056)
Asian	0.131	−0.069
	(0.144)	(0.082)
Hispanic	0.029	0.008
	(0.092)	(0.057)
Female	−0.043	−0.030
	(0.037)	(0.022)
4-year College	0.079**	0.021
	(0.040)	(0.024)
Ideology	0.022	−0.003
	(0.016)	(0.010)
Observations	731	2,000
R^2	0.016	0.003
Adjusted R^2	0.003	−0.002
Residual Std. Error	0.499 (df = 721)	0.494 (df = 1990)
F Statistic	1.276 (df = 9; 721)	0.572 (df = 9; 1990)
Coefficients from OLS regressions		*p<0.1; **p<0.05; ***p<0.01

F.2 Full Sample

Table SI.8: Balance Test using Full Sample

	Dependent variable: Assignment to Treatment	
	Full Film vs. Placebo	Any Treatment vs. Any Control
Party ID	−0.012	0.021
	(0.036)	(0.023)
Age	0.001	0.0002
	(0.001)	(0.001)
White	−0.001	−0.029
	(0.062)	(0.038)
Black	0.051	−0.024
	(0.076)	(0.047)
Asian	0.111	−0.106
	(0.120)	(0.071)
Hispanic	0.053	−0.020
	(0.076)	(0.048)
Female	−0.050	−0.022
	(0.031)	(0.020)
4-year College	0.040	0.005
	(0.033)	(0.021)
Ideology	0.020	0.0001
	(0.013)	(0.008)
Observations	1,038	2,573
R^2	0.010	0.002
Adjusted R^2	0.001	−0.002
Residual Std. Error	0.500 (df = 1028)	0.490 (df = 2563)
F Statistic	1.140 (df = 9; 1028)	0.570 (df = 9; 2563)
Coefficients from OLS regressions		*p<0.1; **p<0.05; ***p<0.01

SI G Attrition

The main results we report in the manuscript are based on our pre-registered comparison of the full documentary film to a placebo documentary using a nationally representative sample (the aforementioned "matched" sample). As shown in Tables SI.9 and SI.11, we do not observe statistically significant differential attrition in this specification,[3] which allows us to more reliably estimate our main quantity of interest: the causal effect of vicarious contact (in the form of the Braver Angels documentary) on affective polarization among the American public. As can be seen in Table SI.11, there is a 3.4 percentage point gap in the likelihood of attrition between the Braver Angels and the placebo documentaries during wave 2, which is a substantively small and statistically insignificant difference. As we mention in the main text, while we initially planned to compare the Braver Angels film to the placebo and control together, we believe comparing the Braver Angels film to the placebo alone (a comparison that we also pre-registered and in which we do not observe differential attrition) is the more reliable approach to estimating the effects of the former, not least because participants assigned to placebo and Braver Angels documentaries received the same compensation to watch a video of similar length, making them more directly comparable to one another.

Nevertheless, given the length of the treatment administered, we carefully evaluate the possibility of bias induced by differential attrition across treatment conditions. Our concern is that differential attrition may bias our treatment effect estimates if attriters are systematically different from non-attriters (for instance, if attrition was caused by aversion to content related to polarization or interactions with the outparty). We define participants as attriters if they did not answer enough survey questions for us to construct an affective polarization index. For the nationally representative sample, we define participants as attriters if they did not answer enough survey questions for us to construct an affective polarization index *or* if they do not have weights for the nationally representative sample assigned by YouGov (i.e., they were not matched).

Out of the full sample of 2,573 participants, 468 attrited by the end of wave 2, yielding an effective sample of 2,105 (18% attrition rate). By wave 3, there were 959 participants who attrited, yielding an effective sample of 1,614 participants (37% attrition rate). For the matched-down samples, all participants who were

[3] As we show in Tables D.1.1 and D.1.2, overall, a slightly larger fraction of respondents attrited in the matched sample (20%) than in the full sample (18%). However, the *distribution* of attriters across treatment conditions was more equal in the matched sample than in the full sample. This allows us to estimate the effects of the Braver Angels documentary in the nationally representative sample with less risk of bias while still adhering to our PAP.

assigned weights answered enough survey questions for us to construct an affective polarization index. From the original sample of 2,000 wave 1 matched participants, 1,600 remained at wave 2 (20% attrition) and 1,360 remained at wave 3 (32% attrition).

Our attrition analyses suggest that differential attrition was primarily caused by the length of the treatment. The more time participants were asked to devote to the study, the likelier they were to drop out of it. Most other pretreatment covariates were not strong predictors of attrition, as we show in the attrition tables below (see Section G.1). Unsurprisingly, participants in the full Braver Angels and placebo documentary conditions (each approximately 50 minutes long) attrited more than those assigned to one of the two short videos (each roughly 5 minutes long), while participants assigned to the empty control condition attrited even less. This interpretation is further borne out by the fact that we find no evidence of differential attrition by treatment assignment if we compare the full Braver Angels documentary group to the placebo group (both of which watched films that were nearly identical in length), or if we compare the two short video groups (also nearly identical in length) to one another. If attrition were driven by aversion to content related to polarization, or by some other factor that is correlated with polarization, then we would expect to observe a higher rate of attrition in the full Braver Angels documentary group relative to the placebo group. But we do not.

Some attrition also resulted from YouGov's standard quality control procedures, but this too seems unlikely to bias our results. Before creating the matched sample, YouGov attempted to identify and remove duplicates, bots, respondents who experienced technical difficulties completing the survey, and "professional" survey takers (i.e., respondents who maximize compensation by completing as many YouGov surveys as possible as quickly as possible, without actually reading or responding meaningfully to them). YouGov identified very few duplicates or professional survey takers in our sample, and those that it did identify were roughly evenly distributed across treatment conditions.[4] YouGov identified more bots and respondents who experienced technical difficulties in our sample, but—with the exception of the pure control condition—these respondents were again roughly evenly distributed across treatment groups.[5] In all other cases, attrition resulted from

[4] A total of 15 respondents were flagged as duplicates, with a maximum of four and a minimum of two per treatment condition. A total of 25 respondents were flagged as professional survey takers because of the number of questions they skipped, with a maximum of seven and a minimum of four per treatment condition. A total of 30 respondents were flagged because of the speed at which they completed the survey, with exactly six in each treatment condition. These three categories are not mutually exclusive; for example, some respondents were flagged as both "skippers" and "speeders."

[5] A total of 200 respondents were flagged as bots, with a maximum of 47 and a minimum of 39 in all treatment groups except the pure control condition. 26 respondents were flagged as bots in pure control. This discrepancy is likely due to the fact that pure control respondents were not asked to watch a video or answer questions about the videos

respondents simply neglecting to complete the survey, or from YouGov failing to find a suitable match for them.

Given these patterns, it seems unlikely that there is some systematic difference between attriters and non-attriters that will bias our main results. Rather, most of the differential attrition we observe is likely a result of idiosyncratic factors that made some participants less willing to devote time to watching a video and filling out a survey. While we believe differential attrition is unlikely to bias our treatment effect estimates, and while we are able to use a wealth of pretreatment covariates to correct for differential attrition using weights, we nonetheless focus our main analyses on comparisons in which we do not observe statistically significant differential attrition. We also interpret estimates based on specifications in which we do observe statistically significant differential attrition with caution. These include the longitudinal (wave 3) results as well as an ancillary analysis that compares the effects of the full Braver Angels film and the shorter videos (see Table SI.9). We believe this is a more prudent and conservative approach.

The pre-treatment covariates we used to predict attrition are:

1. partyID = Republican (1) or Democrat (0)
2. ideology = ideology, very liberal (1) to very conservative (5); with 0 being not sure.
3. age = participant age
4. sex = male (0) or female (1)
5. white = dummy for whether participant identifies as white or not
6. college = dummy for whether participant has at least 4-year college degree or not
7. marriage = dummy for whether participant is married/in domestic partnership or not

they watched, thus generating less data that YouGov could use to identify bots. Because we focus on the comparison between the Braver Angels and placebo documentaries in the body of the paper, the smaller number of bots in the pure control condition is irrelevant to our main results.

A total of 77 respondents could not complete the survey due to technical difficulties, with a maximum of 21 and a minimum of 16 in all treatment groups except the pure control condition. No respondents experienced technical difficulties in the pure control condition. This is because the technical difficulties related in all cases to problems loading the videos respondents were asked to watch in the three treatment groups and the placebo group. Again, because we focus on the comparison between the Braver Angels and placebo documentaries in the body of the paper, the smaller number of respondents who experienced technical difficulties in the pure control condition is irrelevant to our main results.

8. christian = dummy for whether participant identifies as Christian
9. child = dummy for whether participant has at least one child at home
10. turnout_2020 = dummy for whether participant voted in 2020 election
11. job = dummy variable for employment status of participant (1= full or part-time employed, all else is 0)
12. region_2 = one of 4 geographic regions indicating where the participant is from
13. vote_2020 = who participant voted for in 2020 Presidential election. Where 0 indicates Biden, 1 indicates Trump, and 2 anyone else/did not vote
14. vote_2016 = who participant voted for in 2016 Presidential election. Where 0 indicates Clinton, 1 indicates Trump, and 2 anyone else/did not vote

G.1 Attrition Tables

We present 3 attrition tables below. First, Table SI.9, which summarizes the differential attrition between the study's different conditions for waves 2 and 3, for both the nationally representative and the full samples. The table only includes pre-registered comparisons. Each cell presents the differential attrition in percentage points between the two conditions (i.e., Full Film vs. Placebo) as well as the p-value indicating whether the difference in attrition rates is statistically significant at conventional levels. So a value of .098 suggests a 9.8 percentage point difference in attrition between one condition and another. As the table shows, with the exception of the comparison between the full film and the placebo, as well as the mechanism comparison between the two short videos, we observe statistically significant differential attrition across most conditions in the nationally representative sample.

Second, a table comparing non-attriters and attriters along key pre-treatment covariates for our main comparison: the full film vs. placebo in the nationally representative sample during wave 2 (Table SI.10). There are only two standardized mean differences (SMDs) greater than 0.1: attriters were slightly younger than non-attriters (by 2.49 years, SMD = 0.14) and slightly less likely to have voted in the 2020 election (by 5 percentage points, SMD = 0.14). Attriters and non-attriters are comparable along party ID, ideology, sex, educational attainment, race, religion, employment, and parental and marital status.

Third, Table SI.11 examines differential attrition when including covariates for our main comparison. Similar to Table SI.10, the table highlights that differential attrition in our main comparison is not reliably predicted by covariates. Taken together, these models suggest that differential attrition in our study was largely a function of treatment length, as well as some routine technical difficulties associated with video treatments administered via web surveys.

Table SI.9: Summary of Attrition Across Conditions and Waves

Conditions	**Wave 2 (Matched)**	**Wave 3 (Matched)**	**Wave 2 (Full Sample)**	**Wave 3 (Full Sample)**
Full Film vs. Placebo	0.034 (p=0.270)	0.067 (p=0.030)*	0.059 (p=0.029)*	0.083 (p=0.007)**
Full Film vs. Placebo + Control	0.098 (p=0.000)***	0.104 (p=0.000)***	0.109 (p=0.000)***	0.098 (p=0.000)***
Full Film vs. Both Short Videos	0.093 (p=0.000)***	0.078 (p=0.003)**	0.134 (p=0.000)***	0.090 (p=0.001)***
Both Short Videos vs. Empty Control	0.071 (p=0.006)**	0.062 (p=0.021)*	0.026 (p=0.159)	0.025 (p=0.342)
Short VC vs. Short PMC	0.005 (p=0.878)	0.011 (p=0.733)	0.033 (p=0.129)	0.045 (p=0.129)

Notes: Stars denote significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates are the percentage point differences in attrition rates between our pre-specified comparisons. The p-values are derived from OLS estimates of the likelihood of attrition based on treatment assignment. Matched sample indicates the nationally representative sample.

Table SI.10: Covariates of Attriters vs. Non-Attriters, full film vs. placebo, wave 2 nationally representative sample

Variable	**Non-Attriter**	**Attriter**	**Difference**	**SMD**	**Explanation**
Count	584	454	—	—	Total count of participants
Party ID	0.38	0.41	-0.03	-0.07	0 is Democrat, 1 is Republican
Ideology	2.66	2.62	0.04	0.03	1 is very liberal; 5 is very conservative
Age	49.12	46.63	2.49	0.14	Participant age
Sex	0.52	0.56	-0.04	-0.08	0 is male; 1 is female
College	0.33	0.35	-0.02	-0.05	Binary variable whether participants have a 4-yr college degree
White	0.71	0.69	0.02	0.05	0 is non-white, 1 is white
Christian	0.49	0.46	0.03	0.06	0 is non-Christian, 1 is Christian
Child	0.22	0.25	-0.03	-0.06	0 means participant does not have a child under 18 years of age at home; 1 means participant does
Job	0.48	0.48	0.00	-0.01	Whether participant is employed full-time/part-time (1) or unemployed (0)
Marriage	0.51	0.55	-0.04	-0.09	Dummy for whether participant is married/in domestic partnership (1) or NOT (0)
Turnout 2020	0.82	0.77	0.05	0.14	Whether participant voted in 2020

Table SI.11: Likelihood of Wave 2 Attrition in Nationally Representative Sample (Full Film vs. Placebo Documentary)

	Attriter		
	(Full Film vs. Placebo Documentary)		
	(1)	(2)	(3)
treatment	0.034 (0.031)	0.034 (0.031)	−0.005 (0.136)
partyID		0.039 (0.037)	0.014 (0.051)
ideology		0.005 (0.014)	0.0004 (0.019)
age		−0.001 (0.001)	−0.003* (0.001)
sex		0.027 (0.032)	0.125*** (0.045)
college		0.043 (0.035)	0.095* (0.049)
white		−0.013 (0.037)	−0.016 (0.052)
region_21		−0.034 (0.046)	−0.066 (0.065)
region_22		−0.033 (0.049)	−0.024 (0.068)
region_23		−0.006 (0.042)	0.008 (0.058)
christian		−0.035 (0.033)	−0.070 (0.047)
child		−0.002 (0.040)	0.009 (0.057)
job		−0.012 (0.034)	−0.101** (0.049)
marriage		0.057* (0.033)	0.109** (0.047)
turnout2020		−0.079* (0.044)	−0.030 (0.059)
treatment:partyID			0.064 (0.074)
treatment:ideology			0.005 (0.027)
treatment:age			0.003 (0.002)
treatment:sex			−0.204*** (0.064)
treatment:college			−0.108 (0.069)
treatment:white			−0.007 (0.073)
treatment:region_21			0.080 (0.092)
treatment:region_22			−0.006 (0.099)
treatment:region_23			−0.025 (0.083)
treatment:christian			0.068 (0.066)
treatment:child			−0.012 (0.080)
treatment:job			0.181*** (0.069)
treatment:marriage			−0.108 (0.067)
treatment:turnout2020			−0.092 (0.088)
Constant	0.420*** (0.022)	0.485*** (0.069)	0.494*** (0.095)
Observations	1,038	1,009	1,009
R^2	0.001	0.015	0.040
Adjusted R^2	0.0002	0.0002	0.012
Residual Std. Error	0.496 (df = 1036)	0.494 (df = 993)	0.491 (df = 979)
F Statistic	1.216 (df = 1; 1036)	1.014 (df = 15; 993)	1.408* (df = 29; 979)

Note: *p<0.1; **p<0.05; ***p<0.01

G.2 Inverse Probability Weights

Inverse probability weights (IPWs) were constructed using the following procedure. We first used a logit model to regress a reverse-coded attrition indicator on treatment assignment, pre-treatment covariates, and the interaction between treatment assignment and pretreatment covariates. We then calculated fitted values from this model and inverted them to construct IPWs for the full sample. For the nationally representative matched sample, we multiplied the sampling weights provided by YouGov by the inverted fitted values described above.

This procedure yielded four types of IPWs: IPWs for wave 2 and wave 3 for the nationally representative sample and IPWs for wave 2 and wave 3 for the full, unmatched sample.

The covariates used were the following:

1. partyID = Republican (1) or Democrat (0)
2. ideology = ideology, very liberal (1) to very conservative (5); with 0 being not sure.
3. age = participant age
4. sex = male (0) or female (1)
5. white = dummy for whether participant identifies as white or not
6. college = dummy for whether participant has at least 4-year college degree or not
7. marriage = dummy for whether participant is married/in domestic partnership or not
8. christian = dummy for whether participant identifies as Christian
9. child = dummy for whether participant has at least one child at home
10. turnout_2020 = dummy for whether participant voted in 2020 election
11. job = dummy for employment status of participant (1= full or part-time employed)
12. region = region where participant is from

SI H Deviations from Pre-Analysis Plan

During wave 2, we diverged from our pre-analysis plan in two primary ways. First, due to technical difficulties we had to move our main behavioral outcome—sign-ups for the Unify America challenge—to wave 3 rather than wave 2. We also omitted from both waves 2 and 3 a question that would allow us to measure expressed interest in participating in future Braver Angels activities—a behavioral outcome we pre-registered, but one that relied on self-reports, and which therefore seemed less useful than our other behavioral measure. Second, participants were supposed to complete wave 2 at least 72 hours after completing wave 1. However, YouGov began contacting participants 24 hours after completing wave 1. On average, participants were contacted 5 days or 120 hours after completing wave 1. In wave 3, we planned on re-contacting participants 1 month following treatment. However, YouGov contacted participants an average of 1.5 months (approximately 50 days) following treatment.

In addition, we pre-registered testing heterogeneous treatment effects based on dispositional empathy. However, this set of questions was ultimately removed from the survey instrument.

We deviated from our analysis in four ways:

1. For our primary hypothesis, we pre-registered the full video vs. placebo+control. We observed statistically significant differential attrition in this comparison. Instead, we used the full video vs. placebo (which we pre-specified as robustness hypothesis 2) as the main comparison to test our hypotheses regarding vicarious contact's effects on affective polarization, behavioral commitment to depolarization, and anti-democratic attitudes. While this choice was by necessity due to differential attrition, we also reasoned it is a more appropriate comparison since it compares participants who watched videos of similar lengths and received the same compensation.

2. We analyzed our main outcomes using inverse probability weights despite pre-registering the use of IPWs only for those comparisons in which we observe differential attrition. The procedure for their construction is detailed in the previous section. All results are robust to dropping these IPWs in our analyses.

3. Our pre-specified analysis of the stereotyping measure called for subtracting ingroup stereotypes from outgroup stereotypes. Because we also found that the outparty-only version of the affective polarization

index better reflected changes in attitudes towards the outgroup (since the film appeared to also improve views of the ingroup), we opted to present changes in stereotypes of the outgroup only in the main body of the paper. We present the pre-specified results in the section below.

4. Our pre-analysis plan described some outcomes variously as "mechanisms," "mediators," "secondary outcomes," and "exploratory items." For completeness, we present results for all pre-registered outcomes here and in the body of the paper. However, for the sake of clarity, we relabel some secondary outcomes in a way that increases theoretical coherence while still maintaining fidelity to our pre-analysis plan. This deviation is conceptual and semantic; it does not affect the construction of the variables themselves.

SI I Analyses

I.1 Main Analysis

The tables below present the full regression tables of all analyses presented in the main paper, first with inverse probability weights (as presented in the paper) and then without them. We present results for both the nationally representative sample (which we pre-registered presented in the paper) and for the full sample (which we did not pre-register and include here as a robustness check). However, We interpret results in the full sample somewhat cautiously due to differential attrition. In all cases, we compare the full film to the placebo condition only.

Results are unchanged when removing the inverse probability weights. Effect sizes for the full sample are smaller, and with the exception of the affective polarization index, retain the same statistical significance.

I.1.1 Main Outcomes

Table SI.12: Main Analysis: Nationally Representative Sample (Long vs. Placebo)

	Affective Polarization	Outparty Only	BA Newsletter	Any Donation
Full Film	-0.14^{*}	-0.16^{*}	0.08^{*}	-0.04
	(0.07)	(0.07)	(0.04)	(0.04)
R^2	0.01	0.02	0.01	0.00
Adj. R^2	0.01	0.01	0.01	0.00
Num. obs.	584	584	509	583
RMSE	0.85	0.86	0.41	0.46

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Inverse probability weights and robust standard errors are used.

Table SI.13: Main Analysis without IPWs: Nationally Representative Sample (Long vs. Placebo; NO IPWs)

	Affective Polarization	Outparty Only	BA Newsletter	Any Donation
Full Film	-0.13^{*}	-0.15^{*}	0.09^{*}	-0.02
	(0.07)	(0.07)	(0.04)	(0.04)
R^2	0.01	0.01	0.02	0.00
Adj. R^2	0.01	0.01	0.02	-0.00
Num. obs.	584	584	509	583
RMSE	0.64	0.65	0.31	0.34

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used. Inverse Probability weights are NOT used

FULL SAMPLE

Table SI.14: Main Analysis: Full Sample (Long vs. Placebo)

	Affective Polarization	Outparty Only	BA Newsletter	Any Donation
Full Film	−0.08	−0.10*	0.05*	−0.03
	(0.05)	(0.05)	(0.02)	(0.03)
R^2	0.00	0.01	0.01	0.00
Adj. R^2	0.00	0.00	0.01	0.00
Num. obs.	779	779	667	776
RMSE	0.72	0.74	0.35	0.43

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Inverse probability weights and robust standard errors are used.

NO INVERSE PROBABILITY WEIGHTS

Table SI.15: Main Analysis: Full Sample (Long vs. Placebo; NO IPWs)

	Affective Polarization	Outparty Only	BA Newsletter	Any Donation
Full Film	−0.08	−0.10*	0.06*	−0.03
	(0.05)	(0.05)	(0.02)	(0.03)
R^2	0.00	0.01	0.01	0.00
Adj. R^2	0.00	0.00	0.01	−0.00
Num. obs.	779	779	667	776
RMSE	0.63	0.65	0.31	0.37

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used. Inverse probability weights are NOT used

All Donations

Table SI.16: Proportion Donated by Depolarization Organization

	Braver Angels	AllSides	LivingRoom Conversations	Total
Full Film	−0.52	−2.43	−1.31	−4.26
	(1.28)	(1.24)	(1.57)	(3.83)
R^2	0.00	0.01	0.00	0.00
Adj. R^2	−0.00	0.01	0.00	0.00
Num. obs.	583	583	583	583
RMSE	14.92	14.28	16.80	40.44

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used.

Table SI.17: All Donations by Depolarization Organization (NO IPWs)

	Braver Angels	AllSides	LivingRoom Conversations	Total
Full Film	−0.16	−2.03	−0.92	−3.12
	(1.29)	(1.27)	(1.56)	(3.89)
R^2	0.00	0.01	0.00	0.00
Adj. R^2	−0.00	0.01	−0.00	0.00
Num. obs.	583	583	583	583
RMSE	11.29	10.98	12.54	30.63

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used.

Any Donation by Organization

Table SI.18: Any Donations by Depolarization Organization

	Braver Angels	AllSides	Living Room Conversations
Full Film	−0.02	−0.06	−0.04
	(0.04)	(0.04)	(0.04)
R^2	0.00	0.01	0.00
Adj. R^2	−0.00	0.01	0.00
Num. obs.	583	583	583
RMSE	0.44	0.43	0.44

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors and inverse probability weights are used

I.1.2 Secondary Outcomes, Mechanisms, and Mediators

Secondary outcomes and mechanisms are presented as follows: four models are presented for each outcome measure.

1. Matched sample, referring to the nationally representative sample with inverse probability weights (the specification presented in the paper)
2. Matched sample without inverse probability weights.
3. The full sample with inverse probability weights
4. The full sample without inverse probability weights

Optimism

Survival of Democratic Institutions

Table SI.19: Main Analysis: Optimism about Survival of Democratic Institutions Wave 2 (Full Film vs. Placebo)

	Matched (IPWs)	Matched (No IPWs)	Full (IPWs)	Full (No IPWs)
Full Film	0.32**	0.30**	0.18*	0.18*
	(0.10)	(0.10)	(0.07)	(0.07)
R^2	0.03	0.02	0.01	0.01
Adj. R^2	0.03	0.02	0.01	0.01
Num. obs.	583	583	776	776
RMSE	1.26	0.96	1.13	0.99

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used.

Restoring Civility

Table SI.20: Main Analysis: Optimism about Restoring Civility Wave 2 (Full Film vs. Placebo)

	Matched (IPWs)	Matched (No IPWs)	Full (IPWs)	Full (No IPWs)
Full Film	0.43***	0.39***	0.16*	0.15*
	(0.10)	(0.10)	(0.07)	(0.07)
R^2	0.05	0.04	0.01	0.01
Adj. R^2	0.04	0.04	0.00	0.00
Num. obs.	583	583	776	776
RMSE	1.29	0.98	1.15	1.01

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used.

Dialogue

Table SI.21: Main Analysis: Dialogue as Effective Tool For Change Wave 2 (Full Film vs. Placebo)

	Matched (IPWs)	Matched (No IPWs)	Full (IPWs)	Full (No IPWs)
Full Film	0.46^{***}	0.45^{***}	0.28^{***}	0.29^{***}
	(0.11)	(0.10)	(0.07)	(0.07)
R^2	0.05	0.05	0.02	0.02
Adj. R^2	0.05	0.05	0.02	0.02
Num. obs.	528	528	682	682
RMSE	1.27	0.96	1.10	0.97

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used.

Anti-Democratic Attitudes

Results of estimating vicarious contact's effects on anti-democratic attitudes are robust to different specifications.

Anti-democratic attitudes are measured through a series of hypothetical scenarios. Respondents will be asked how likely they would be to support each of six anti-democratic attitudes on a scale from 0-3. The six items are below are standardized (mean 0, SD 1) and then averaged into one anti-democratic attitudes score. For compactness we present question wording for Republican respondents; the questions are identical for Democratic respondents, but with the party IDs flipped.

- Imagine a right-leaning officeholder in your jurisdiction sought to restrict or ban rallies by far-left groups, on the grounds that even peaceful far-left rallies have the potential to turn violent. To what extent would you support restrictions of this sort?
- Imagine a Republican official from your state proposed a law that would make it easier for Republicans to vote and harder for Democrats, on the grounds that Democrats would do the same if they were in power. To what extent would you support a law of this sort?
- Imagine a social media platform that you use tried to monitor or shut down far-left online chat groups, on the grounds that they promote extremism. To what extent would you support restrictions of this sort?

- Imagine a right-leaning officeholder in your jurisdiction proposed a law that would make it easier to disqualify far-left candidates for public office, on the grounds that far-left candidates support policies that are dangerous for the country. To what extent would you support a law of this sort?

- Imagine Republican officials from your state tried to cause gridlock in Congress at a time when Democrats are in the majority, on the grounds that gridlock prevents liberal policies that are bad for the country, even if those policies are supported by most Americans. To what extent would you support actions of this sort?

- Imagine Republican officials in a nearby state decided to draw congressional districts that maximize Republican seats in Congress, even in places where the share of Republican voters is declining, on the grounds that Democrats are doing the same thing. To what extent would you support actions of this sort?

Table SI.22: Main Analysis: Anti-Democratic Attitudes Wave 2 (Full Film vs. Placebo)

	Matched	Matched (No IPWs)	Full	Full (No IPWs)
Full Film	−0.08	−0.10	−0.06	−0.05
	(0.07)	(0.07)	(0.05)	(0.05)
R^2	0.00	0.01	0.00	0.00
Adj. R^2	0.00	0.00	0.00	0.00
Num. obs.	583	583	776	776
RMSE	0.89	0.67	0.77	0.67

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used.

I.1.3 Mechanisms

Figure SI.8 below presents the effect of the documentary on six possible mediators. All outcomes have been standardized for ease of interpretation.

- Stereotyping. Broken down into a standardized index of five positive stereotypes about the outparty: patriotic, intelligent, honest, open-minded, and generous ($\beta = 0.201$, $p = 0.062$, $N = 581$). And a

standardized index of three negative stereotypes: hypocritical, selfish, and mean (β = -0.202 , $p = 0.043$, $N = 581$).

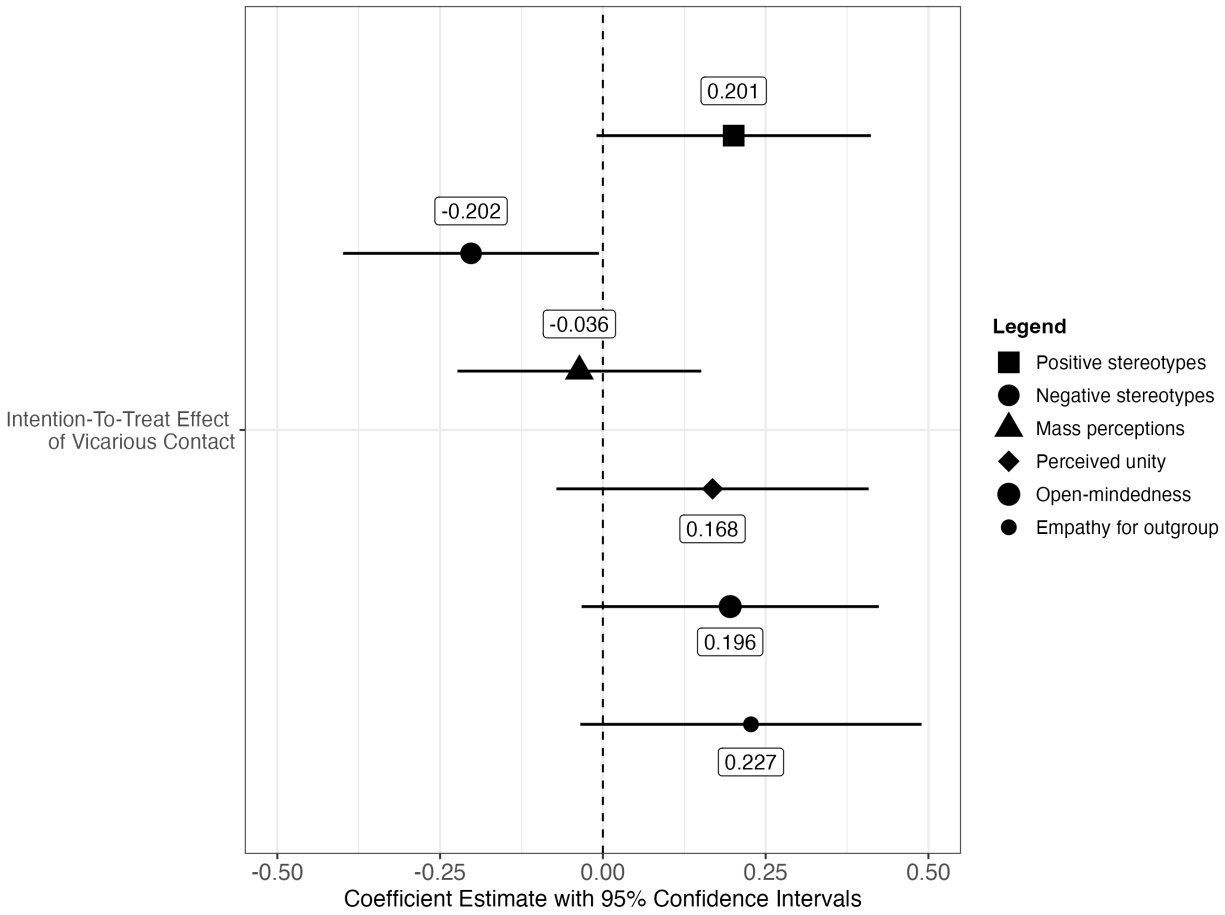
- Mass perceptions index (β = -0.036 , $p = 0.550$, $N = 536$). A standardized index of perceived agreement on three policy issues: abortion, same-sex marriage, and paid child leave.

- Perceived division ($\beta = 0.168$, $p = 0.169$, $N = 545$). In your view, how divided or united are ordinary Americans in their political views? With 0 being totally divided and 100 being totally united. For ease of interpretation, this outcome is presented as perceived unity in Figure SI.8.

- Open-mindedness ($\beta = 0.196$, $p = 0.093$, $N = 488$). A standardized composite of two measures: (1) I sometimes find it difficult to see things from OUTPARTYs' point of view (reverse-coded); and (2) I sometimes try to understand OUTPARTYs better by imagining how things look from their perspective.

- Empathy for the outgroup ($\beta = 0.227$, $p = 0.089$, $N = 489$). A standardized composite of two measures: (1) It is not worth my time trying to listen to OUTPARTYs talk about politics (reverse-coded); and (2) People should listen to OUTPARTYs' concerns.

Figure SI.8: The effect of vicarious contact on possible mediators



Notes: Intention-to-treat estimates from weighted OLS regressions comparing the effect of the Braver Angels documentary to the effect of a placebo nature documentary among a nationally representative sample of Americans. Symbols denote coefficients; lines denote 95% confidence intervals. Observations are weighted by the product of the inverse probability of non-attrition and the sample weights provided by YouGov.

I.1.4 Extensions

Extension 1: Heterogeneous Treatment Effects by Party ID

Table SI.23: Main Analysis: Affective Polarization HTE by Party ID Wave 2 (Full Film vs. Placebo)

	Matched (IPWs)	Matched (No IPWs)	Full (IPWs)	Full (No IPWs)
Full Film	-0.19^{*}	-0.17	-0.07	-0.08
	(0.10)	(0.09)	(0.06)	(0.06)
partyID	-0.14	-0.13	-0.17^{**}	-0.18^{**}
	(0.08)	(0.08)	(0.06)	(0.06)
Full Film:partyID	0.12	0.09	-0.03	-0.02
	(0.13)	(0.13)	(0.09)	(0.09)
R^2	0.02	0.02	0.03	0.03
Adj. R^2	0.01	0.01	0.02	0.02
Num. obs.	584	584	779	779
RMSE	0.85	0.64	0.71	0.63

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used. Binary partyID variable where 0 is Democrat, 1 is Republican

Table SI.24: Main Analysis: Affective Polarization HTE by Ideology Wave 2 (Full Film vs. Placebo)

	Matched (IPWs)	Matched (No IPWs)	Full (IPWs)	Full (No IPWs)
Full Film	-0.56^{**}	-0.49^{**}	-0.26^{*}	-0.26^{*}
	(0.20)	(0.19)	(0.13)	(0.12)
Ideology	-0.12^{***}	-0.12^{***}	-0.13^{***}	-0.13^{***}
	(0.03)	(0.03)	(0.03)	(0.03)
Full Film x Ideology	0.13^{*}	0.10	0.06	0.06
	(0.06)	(0.06)	(0.04)	(0.04)
R^2	0.05	0.04	0.04	0.04
Adj. R^2	0.04	0.04	0.04	0.04
Num. obs.	538	538	714	714
RMSE	0.82	0.62	0.71	0.62

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used. Lower numbers indicate ideology is more liberal. Higher numbers indicate more conservative.

Media Index Construction

Index constructed by subtracting (Fox News + WSJ) from (NYT + NPR) and standardizing the variable. So a higher score indicates greater belief in the truthfulness of conservative-leaning media compared with liberal media. If media index is equal to or greater than the median (0.0604), then a value of 1 is assigned. Meaning

greater trust in conservative media. If media index is less than median, then 0 is assigned. Meaning greater trust in liberal media.

Table SI.25: Main Analysis: Affective Polarization HTE by Relative Confidence in Media Wave 2 (Full Film vs. Placebo)

	Matched (IPWs)	Matched (No IPWs)	Full (IPWs)	Full (No IPWs)
Full Film	-0.27^{*}	-0.23^{*}	-0.24^{**}	-0.23^{**}
	(0.11)	(0.10)	(0.08)	(0.08)
Media Trust	-0.32^{***}	-0.31^{***}	-0.36^{***}	-0.36^{***}
	(0.09)	(0.09)	(0.07)	(0.07)
Full Film x Media Trust	0.19	0.16	0.27^{*}	0.25^{*}
	(0.15)	(0.14)	(0.11)	(0.11)
R^2	0.05	0.05	0.05	0.05
Adj. R^2	0.05	0.05	0.05	0.05
Num. obs.	422	422	558	558
RMSE	0.82	0.62	0.71	0.62

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used. Lower values of media trust indicate greater confidence in liberal as opposed to conservative media

Table SI.26: Main Analysis: Affective Polarization Wave 3 (Full Film vs. Placebo)

	Matched (IPWs)	Matched (No IPWs)	Full (IPWs)	Full (No IPWs)
Full Film	-0.11	-0.10	-0.03	-0.04
	(0.07)	(0.07)	(0.05)	(0.05)
R^2	0.01	0.01	0.00	0.00
Adj. R^2	0.01	0.00	-0.00	-0.00
Num. obs.	506	508	613	615
RMSE	0.91	0.63	0.81	0.63

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used. Note that these comparisons have differential attrition

Table SI.27: Main Analysis: Affective Polarization Outparty Only Wave 3 (Full Film vs. Placebo)

	Matched (IPWs)	Matched (No IPWs)	Full (IPWs)	Full (No IPWs)
Full Film	−0.12	−0.11	−0.04	−0.03
	(0.08)	(0.07)	(0.05)	(0.05)
R^2	0.01	0.01	0.00	0.00
Adj. R^2	0.01	0.01	−0.00	−0.00
Num. obs.	506	508	613	615
RMSE	0.95	0.64	0.82	0.64

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used.

I.2 Pre-Registered Analyses not Featured in Paper

I.2.1 Full Film vs. Control + Placebo

Includes affective polarization index, outparty only, BA newsletter clicks, and any donations: the main specified outcomes. We only include a binary indicator for donations for brevity. Results remain unchanged for specific donated amounts. Termed H1.1 and H1.2 in pre-analysis plan.

Table SI.28: Excluded Pre-Specified Analysis: Nationally Representative Sample Wave 2 (Full Film vs. Placebo+Control)

	Affective Polarization	Outparty Only	BA Newsletter	Any Donation
Full Film	−0.10	-0.13^{*}	0.09^{**}	−0.04
	(0.06)	(0.06)	(0.03)	(0.04)
R^2	0.00	0.01	0.02	0.00
Adj. R^2	0.00	0.01	0.02	0.00
Num. obs.	943	943	836	941
RMSE	0.83	0.83	0.36	0.46

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used.

Table SI.29: Excluded Pre-Specified Analysis: Full Sample Wave 2 (Full Film vs. Placebo+Control)

	Affective Polarization	Outparty Only	BA Newsletter	Any Donation
Full Film	−0.07	−0.09*	0.07**	−0.04
	(0.04)	(0.04)	(0.02)	(0.02)
R^2	0.00	0.00	0.01	0.00
Adj. R^2	0.00	0.00	0.01	0.00
Num. obs.	1225	1225	1070	1218
RMSE	0.70	0.71	0.32	0.43

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Inverse probability weights and robust standard errors are used.

I.2.2 Short films vs. Full film

Table SI.30: Excluded Pre-Specified Analysis: Nationally Representative Sample Wave 2 (Full Film vs. Short Videos)

	Affective Polarization	Outparty Only	BA Newsletter	Any Donation
Short PMC	−0.02	−0.05	0.02	−0.04
	(0.06)	(0.06)	(0.02)	(0.04)
Short VC	0.01	−0.02	0.05*	0.01
	(0.07)	(0.07)	(0.02)	(0.04)
Full Film	−0.06	−0.10	0.10**	−0.05
	(0.07)	(0.07)	(0.03)	(0.04)
R^2	0.00	0.00	0.02	0.00
Adj. R^2	−0.00	0.00	0.02	0.00
Num. obs.	1302	1302	1151	1301
RMSE	0.81	0.82	0.36	0.45

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors and inverse probability weights are used. Empty control is the reference category

Table SI.31: Excluded Pre-Specified Analysis: Full Sample Wave 2 (Full Film vs. Short Videos)

	Affective Polarization	Outparty Only	BA Newsletter	Any Donation
Short PMC	−0.06	−0.07	0.03	−0.01
	(0.04)	(0.04)	(0.02)	(0.03)
Short VC	−0.04	−0.07	0.06**	0.02
	(0.04)	(0.04)	(0.02)	(0.03)
Full Film	−0.06	−0.09*	0.09***	−0.04
	(0.04)	(0.05)	(0.02)	(0.03)
R^2	0.00	0.00	0.01	0.00
Adj. R^2	−0.00	0.00	0.01	0.00
Num. obs.	1704	1704	1479	1691
RMSE	0.69	0.71	0.32	0.43

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used and inverse probability weights used. Empty control is the reference category

Since the comparison using the nationally representative sample using both short videos pooled compared with the empty control had differential attrition, we are using the full sample instead, which did not have differential attrition. With the exception of BA newsletter clicks, none of the main outcomes reached statistical significance.

Table SI.32: Excluded Pre-Specified Analysis: Short Films Main Outcomes Wave 2 using Full Sample (Pooled short videos vs. Empty Control)

	Affective Polarization	Outparty Only	BA Newsletter Clicks	Any Donation
Short Videos	−0.05	−0.07	0.05**	0.00
	(0.04)	(0.04)	(0.02)	(0.02)
R^2	0.00	0.00	0.01	0.00
Adj. R^2	0.00	0.00	0.01	−0.00
Num. obs.	1326	1326	1160	1314
RMSE	0.67	0.69	0.29	0.43

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used.

I.2.3 Longitudinal Outcomes

Behavioral Commitment to Depolarization: Unify America Clicks: Wave 3

Table SI.33: Excluded Pre-Specified Analysis: Unify America Sign-Ups Wave 3 (Full Film vs. Placebo)

	Matched (IPWs)	Matched (No IPWs)	Full (IPWs)	Full (No IPWs)
Full Film	−0.01	0.00	−0.01	−0.01
	(0.05)	(0.05)	(0.03)	(0.04)
R^2	0.00	0.00	0.00	0.00
Adj. R^2	−0.00	−0.00	−0.00	−0.00
Num. obs.	506	508	615	617
RMSE	0.60	0.44	0.55	0.44

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used.

Anti-Democratic Attitudes Wave 3

Table SI.34: Excluded Pre-Specified Analysis: Anti-Democratic Attitudes Wave 3 (Full Film vs. Placebo)

	Matched (IPWs)	Matched (No IPWs)	Full (IPWs)	Full (No IPWs)
Full Film	−0.13	−0.11	−0.06	−0.04
	(0.09)	(0.08)	(0.06)	(0.06)
R^2	0.01	0.01	0.00	0.00
Adj. R^2	0.01	0.00	0.00	−0.00
Num. obs.	506	508	612	614
RMSE	1.03	0.71	0.89	0.70

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used.

I.3 Ancillary Analyses

I.3.1 Breakdown of Affective Polarization Index Components

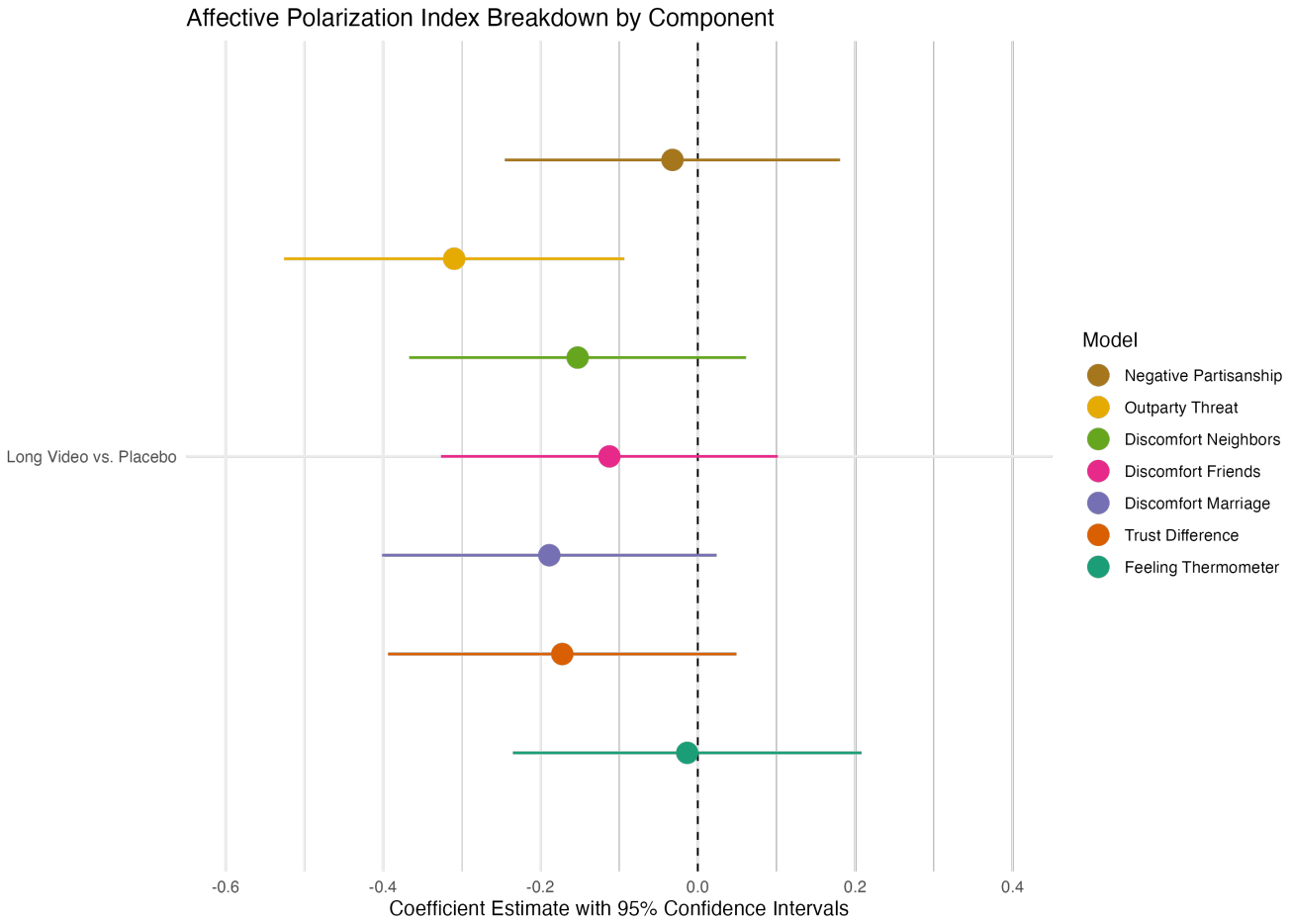


Figure SI.9: Affective polarization index for nationally representative sample wave 2: breakdown by component. Robust standard errors and inverse probability weights used.

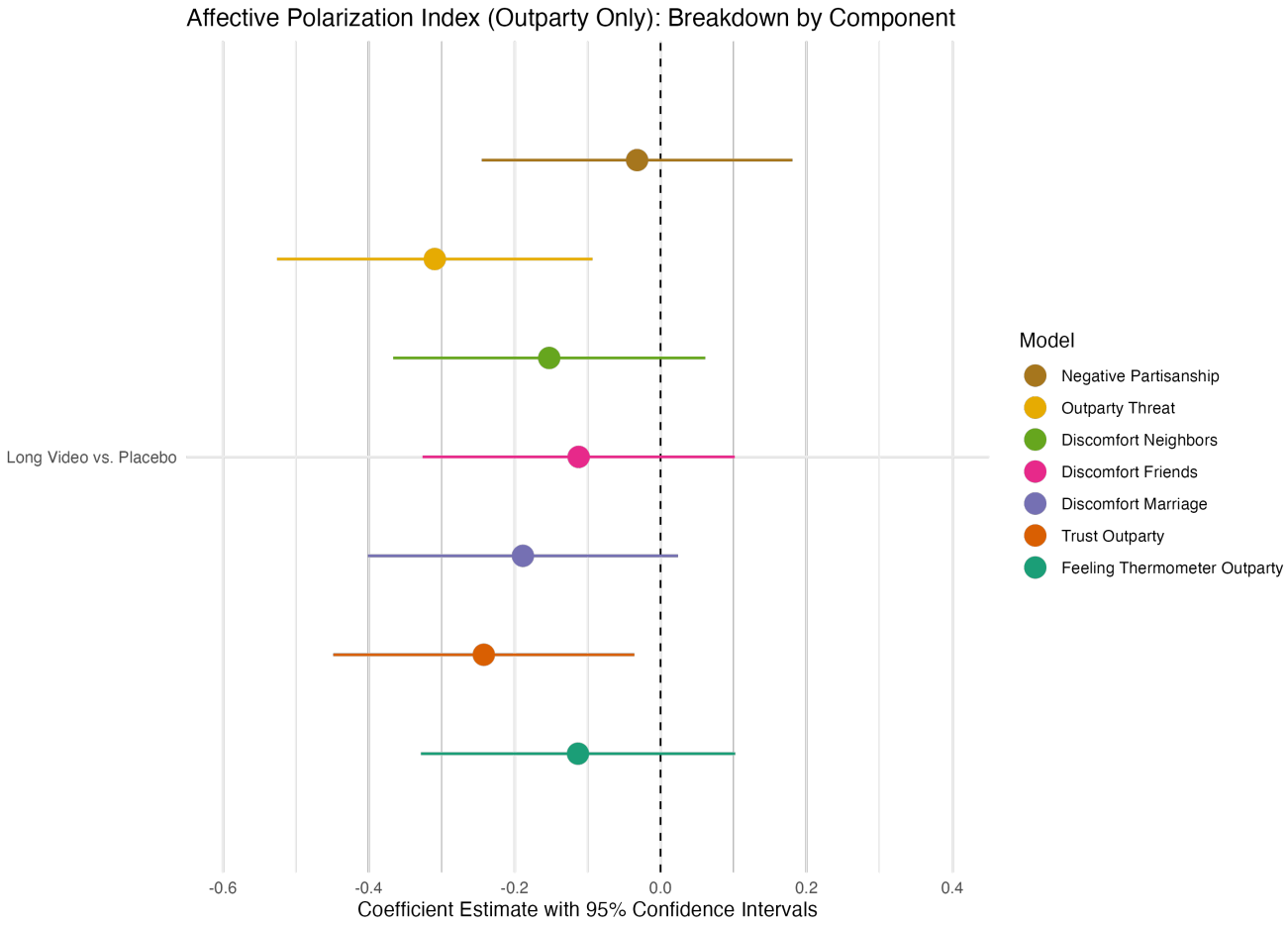


Figure SI.10: Affective polarization index for outparty only, nationally representative sample wave 2: breakdown by component. Robust standard errors and inverse probability weights used. Outparty trust variable reverse-coded for ease of interpretation

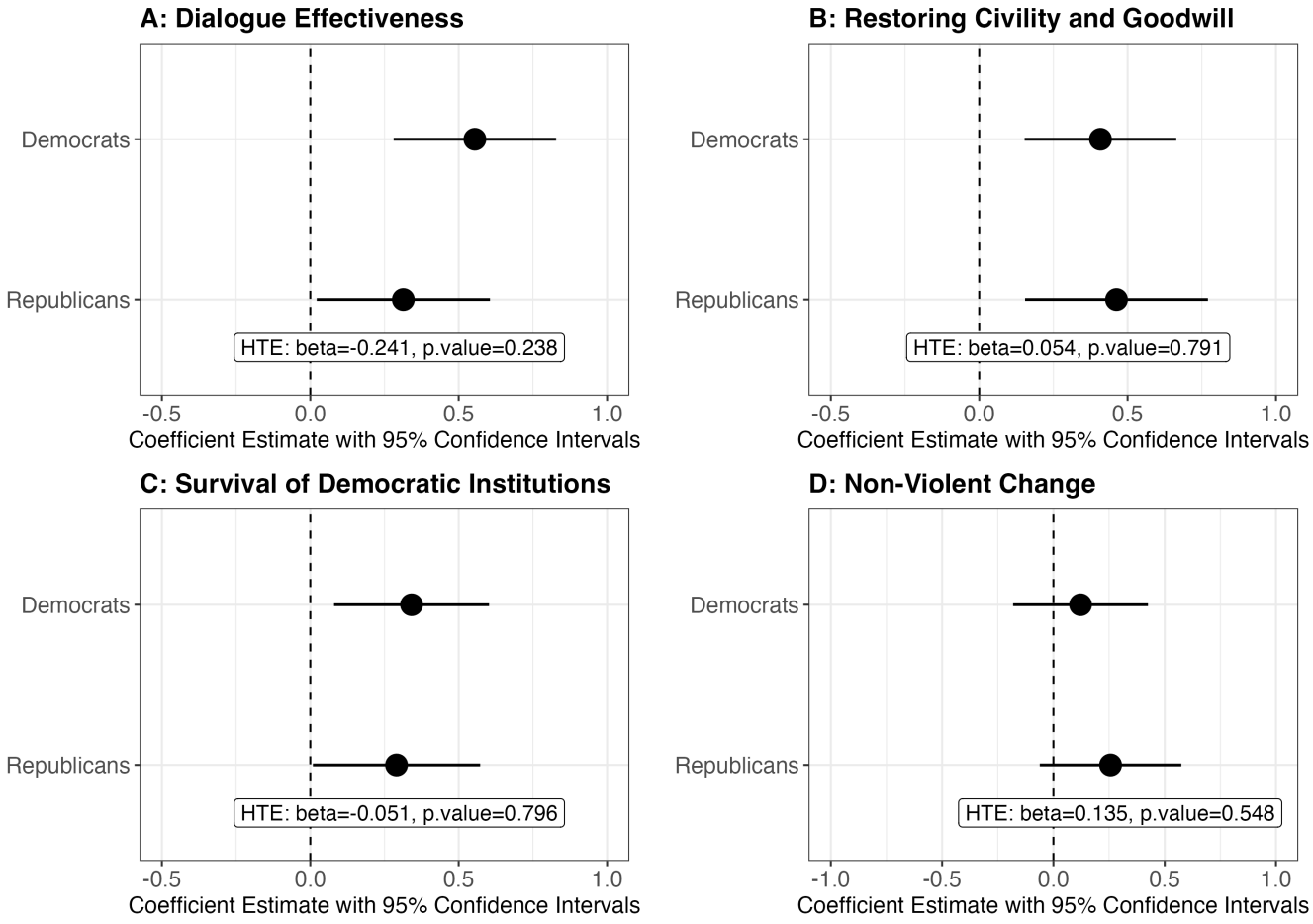
Table SI.35: Effect of Documentary on Ingroup Affective Polarization Measures

	Ingroup Trust	Ingroup Warmth
Full Documentary	−0.01	0.10
	(0.11)	(0.10)
R^2	0.00	0.00
Adj. R^2	−0.00	0.00
Num. obs.	583	563
RMSE	1.33	1.27

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors and inverse probability weights are used. Nationally representative sample, wave 2

I.3.2 Heterogeneous Treatment Effects: Secondary Outcomes

Figure SI.11: Conditional average treatment effects on optimism outcomes, by party ID.



Notes: Nationally representative sample wave 2. IPWs and robust standard errors used.

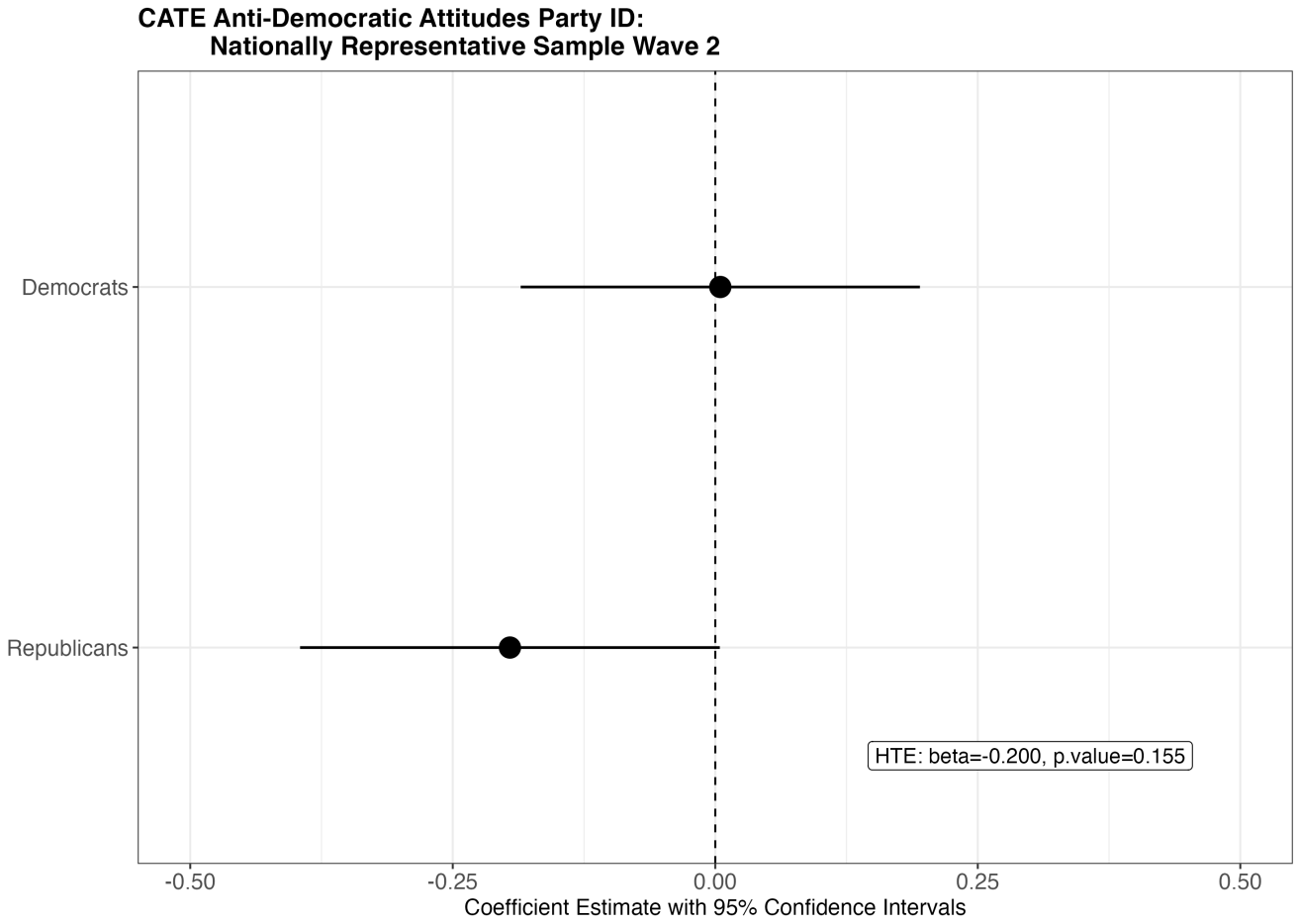


Figure SI.12: Anti-democratic index conditional average treatment effect plot by Party ID. Nationally representative sample wave 2. IPWs and robust standard errors used.

I.3.3 Survey response time and treatment decay

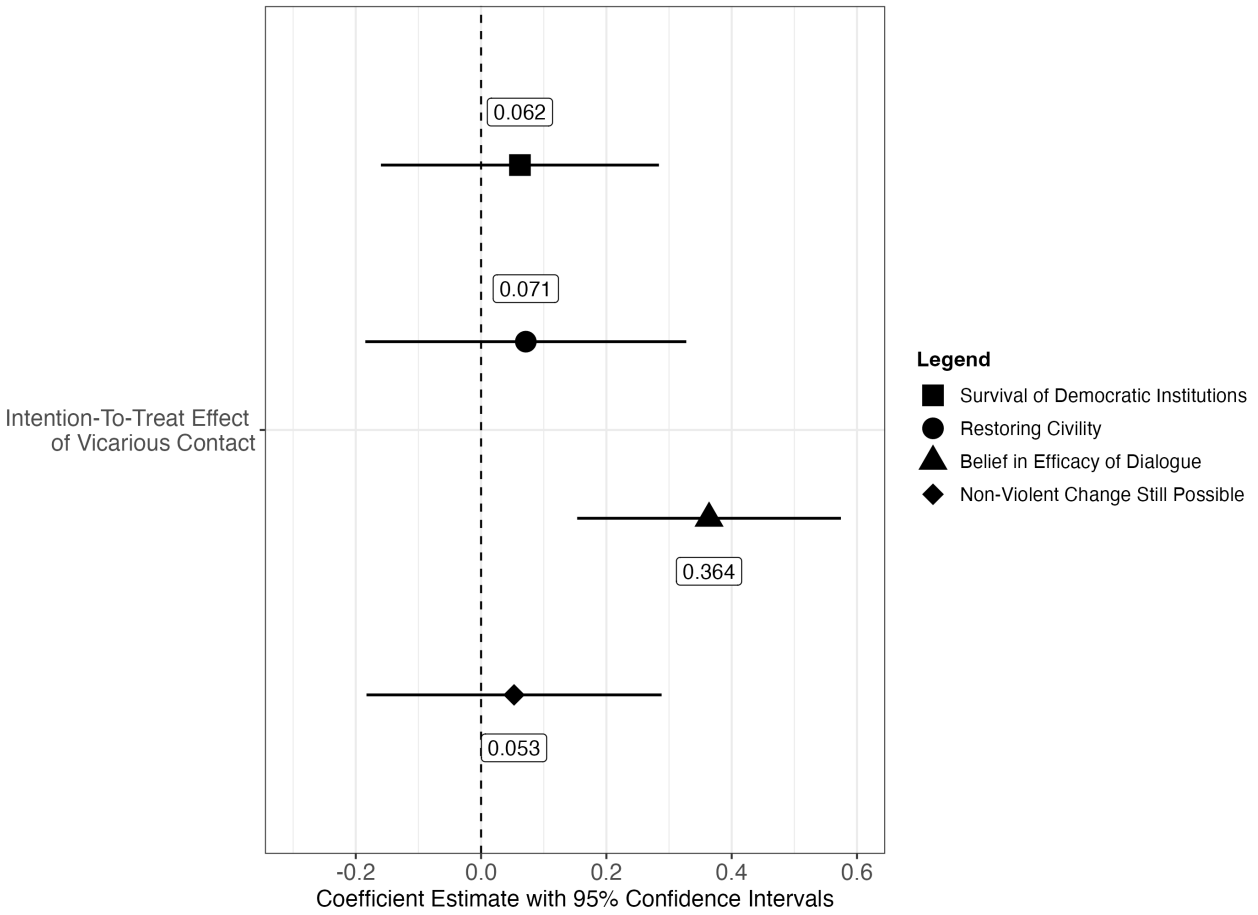
Table SI.36: Changes in affective polarization conditional on time between treatment and wave 2

	Affective Polarization	Outparty Only
Full documentary	−0.30	−0.31
	(0.18)	(0.19)
Response Time (in days)	−0.01	−0.01
	(0.02)	(0.02)
Interaction	0.04	0.03
	(0.03)	(0.03)
R^2	0.02	0.02
Adj. R^2	0.01	0.01
Num. obs.	584	584
RMSE	0.85	0.85

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors and inverse probability weights are used.

I.3.4 Longitudinal results: secondary outcomes

Figure SI.13: Vicarious contact's effect on secondary outcomes. Nationally representative sample wave 3



Notes: Intention-to-treat estimates from weighted OLS regressions comparing the effect of the Braver Angels documentary to the effect of a placebo nature documentary among a nationally representative sample of Americans. Symbols denote coefficients; lines denote 95% confidence intervals. The dependent variables are standardized measures of participants' optimism about (1) the survival of democratic institutions; (2) the restoration of civility and goodwill between Democrats and Republicans; (3) participants' belief in the efficacy of dialogue; and (4) belief that non-violent change is still possible. Observations are weighted by the product of the inverse probability of non-attrition and the sample weights provided by YouGov.

I.3.5 Heterogeneous Treatment Effects with Covariates

Below we present conditional treatment effects by party ID with covariate adjustment in Table SI.37. In general, the results are substantively comparable to hetereogenous treatment effect analyses without covariate adjustment.

Table SI.37: Heterogeneous treatment effects by party ID with covariate adjustment

	Affective polarization	Outparty only
Full documentary	-0.22^{**}	-0.22^{*}
	(0.08)	(0.09)
Party ID	-0.01	0.02
	(0.10)	(0.10)
Age	-0.00	-0.00
	(0.00)	(0.00)
Sex	0.00	0.01
	(0.07)	(0.07)
College	0.05	0.06
	(0.07)	(0.07)
White	-0.04	-0.05
	(0.07)	(0.07)
Christian	-0.09	-0.12
	(0.07)	(0.07)
Child	-0.14	-0.19^{*}
	(0.08)	(0.09)
Job	-0.03	-0.03
	(0.08)	(0.08)
Marriage	-0.08	-0.08
	(0.07)	(0.07)
2020 Turnout	0.31^{**}	0.24^{*}
	(0.11)	(0.11)
Ideology	-0.10^{*}	-0.12^{*}
	(0.04)	(0.05)
Full documentary x Party ID	0.13	0.14
	(0.13)	(0.13)
R^2	0.10	0.11
Adj. R^2	0.07	0.08
Num. obs.	538	538
RMSE	0.80	0.82

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Robust standard errors and inverse probability weights are used. Party ID variable denotes whether participants identify as Democrats (0) or Republicans (1). Covariates are: age, sex, college/no-college, white/non-white, christian/non-christian, have a child at home, employed/unemployed, married/not-married, 2020 election turnout, and ideology.

I.3.6 Additional secondary outcomes that were not pre-registered

Optimism about Overcoming Polarization

Table SI.38: Main Analysis: Optimism about Overcoming Polarization Wave 2 (Full Film vs. Placebo)

	Matched (IPWs)	Matched (No IPWs)	Full (IPWs)	Full (No IPWs)
Full Film	0.37***	0.34**	0.25***	0.25***
	(0.11)	(0.11)	(0.07)	(0.07)
R^2	0.04	0.03	0.02	0.02
Adj. R^2	0.03	0.03	0.01	0.01
Num. obs.	583	583	776	776
RMSE	1.27	0.96	1.12	0.98

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used.

Time to Trust

Table SI.39: Main Analysis: Time It Would Take to Rebuild Trust (reverse-coded) Wave 2 (Full Film vs. Placebo)

	Matched (IPWs)	Matched (No IPWs)	Full (IPWs)	Full (No IPWs)
Full Film	0.33**	0.31**	0.20**	0.19**
	(0.10)	(0.10)	(0.07)	(0.07)
R^2	0.03	0.02	0.01	0.01
Adj. R^2	0.03	0.02	0.01	0.01
Num. obs.	583	583	776	776
RMSE	1.28	0.97	1.12	0.99

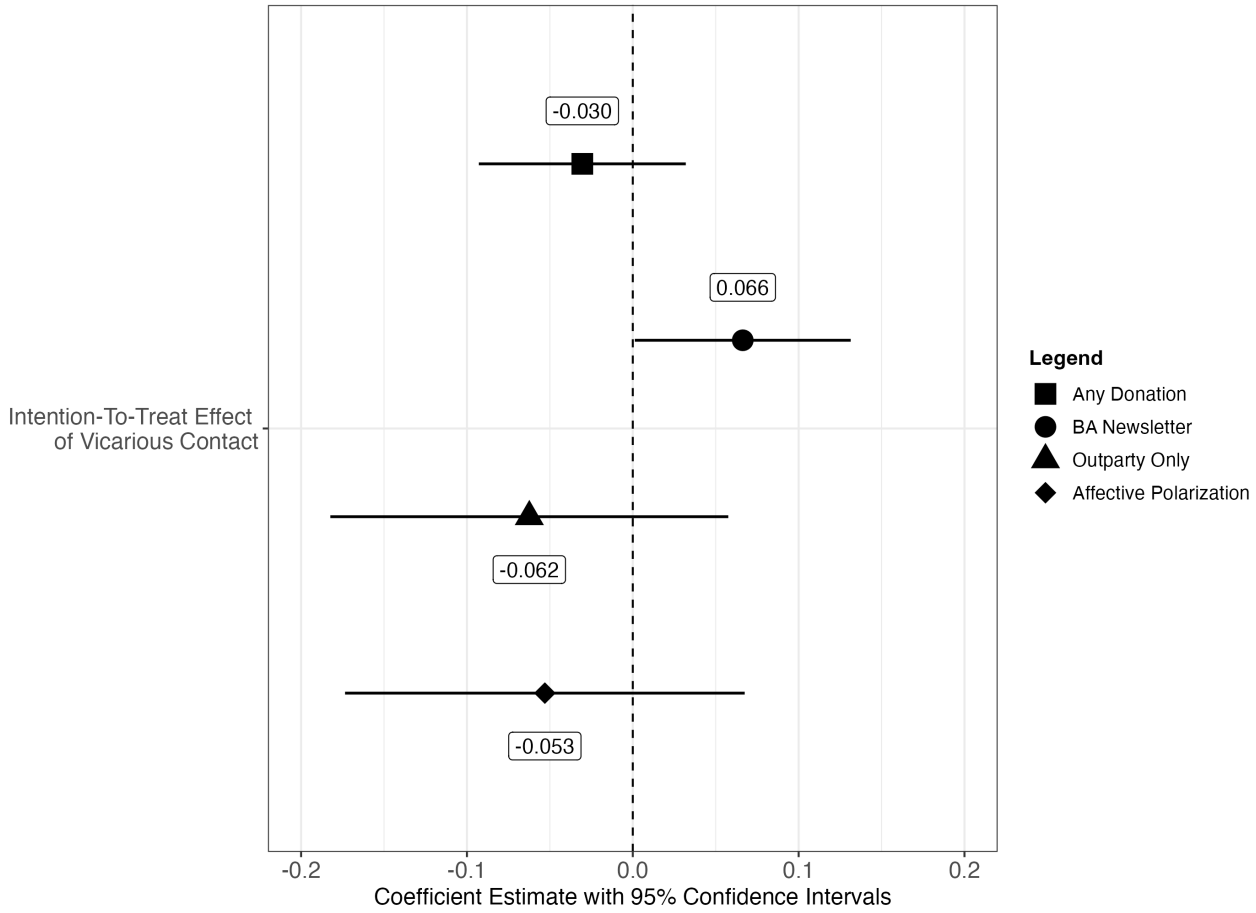
$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
Robust standard errors are used.

I.3.7 Comparison between short videos and full documentary

One hope of our study is to test the efficacy of the full documentary against shorter videos that are likelier to be watched by more people due to their brevity. Here, we report results that compare the treatment

effect of the full documentary relative to both short video conditions. However, these comparisons should be interpreted cautiously since we find statistically significant and substantial differential attrition in both the full ($\beta = 0.134$, $p = 0.000$, $N = 1553$) and nationally representative samples ($\beta = 0.093$, $p = 0.000$, $N = 1553$). With this caveat in mind, we report results in Figure SI.14 below for all four primary outcomes: Affective polarization (β = -0.053 , $p = 0.388$, $N = 943$); Outparty only (β = -0.062 , $p = 0.308$, $N = 943$); Newsletter clicks ($\beta = 0.066$, $p = 0.046$, $N = 824$); and Donation (β = -0.030, $p = 0.339$, $p = 943$). With the exception of the Braver Angels newsletter clicks, there is no statistically significant difference between the short video conditions and the full documentary. Although the full film does appear to have a greater effect on affective polarization in general. These results might suggest that the short videos could achieve similar reductions in outparty animus, although not for behavioral interest in depolarization. The implication is that it is possible to shift attitudes using shorter videos but that changing behavior requires a more involved intervention.

Figure SI.14: Comparison of short videos and full documentary. Nationally representative sample, wave 2



Notes: Intention-to-treat estimates from weighted OLS regressions comparing the effect of the full Braver Angels documentary to the effect of the two short Braver Angels videos among a nationally representative sample of Americans (N = 943 for all except for the BA Newsletter model, where the number of observations is N=824). Symbols denote coefficients; lines denote 95% confidence intervals constructed using robust standard errors. Observations are weighted by the product of the inverse probability of non-attrition and the sample weights provided by YouGov.

SI J Benchmarking against Baron et al. (2025)

In the paper we benchmark the effects of the Braver Angels documentary against the effects of a Braver Angels workshop. Baron et al. (2025) report the ITT of an 8-hour, in-person "Red/Blue" workshop implemented on four college campuses. We report the ITT of a 50-minute online documentary featuring footage from a similar (albeit earlier) Red/Blue workshop. Since we and Baron et al. measure affective polarization in very similar ways, this benchmarking exercise allows us to compare the effects of vicarious to direct intergroup contact

while holding many potentially confounding factors (e.g. implementing partner, nature of the intervention, measurement strategy, etc.) constant.

The comparison is imperfect, since Baron et al. sampled college students, measured outcomes on average 14 days after treatment, and estimate treatment effects by comparing a treatment group to a pure control. In contrast, we sampled a nationally representative sample of Americans, measured outcomes on average five days after treatment, and estimate treatment effects by comparing a treatment group to a placebo. Baron et al.'s empirical strategy also differs slightly from our own: their affective polarization index does not include a measure of negative partisanship (ours does); their index is standardized to its baseline values (ours is not); and they estimate their ITTs as marginal effects in a difference-in-differences framework (we estimate our ITTs in a single cross-section).

To make this comparison more direct, we re-estimate the ITT from Baron et al. using an empirical specification that is as similar to ours as possible: we add negative partisanship to their affective polarization index, do not standardize their index to its baseline values, and estimate the ITT in a single cross-section using their midline survey.[6] With these adjustments, we find that the 8-hour, in-person Red/Blue workshop reduced scores on an affective polarization index by 0.184 standard deviations, compared to 0.140 standard deviations for the 50-minute online documentary. The ITT of the documentary on affective polarization is thus roughly 76% the magnitude of the ITT of the workshop.

We can also make the comparison more direct by using waves 2 and 3 of our survey—collected roughly five and 50 days after treatment, respectively—to estimate what the ITT of the documentary *might have been* after 14 days, which is equivalent to the gap between treatment and outcome measurement in Baron et al. Assuming a linear rate of decay, the effect of the documentary on our pre-specified affective polarization index would have been roughly -0.135 standard deviations after 14 days. (The results are nearly identical if we assume an exponential rate of decay instead.) Using this extrapolation to account for the different gaps between treatment and outcome measurement across the two studies, we find that the effect of the documentary is roughly 73% the magnitude of the effect of the workshop.

[6]In principle we could make the benchmarking exercise even more direct by comparing the Braver Angels documentary to a pure control group, as in Baron et al. But given significant differential attrition between the Braver Angels documentary and the pure control group in our study, we focus instead on the comparison between the Braver Angels and placebo documentaries. This comparison does not suffer from differential attrition and therefore is at lower risk of bias.