

# Supplementary Information: Partisan Group Threat and the Consequences of Cross-partisan Conversation

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## Appendix A Ethical Considerations

The human subjects research conducted for our study adheres to APISA's Principles and Guidance for Human Subjects Research. The study was approved prior to data collection by the authors' institutional review board under expedited review. The IRB deemed: "the criteria for approval are met per 45 CFR 46.111 and/or 21 CFR 56.111 as applicable. Project determined to be minimal risk per 45 CFR 46.102(i) and/or 21 CFR 56.102(i) as applicable." A modification to the study protocol was also approved in which the only change was to increase the number of participants enrolled in the study.

All participants who completed the pre-treatment survey were paid \$0.75. All participants who were invited to return to the conversation/short essay portion of the survey were paid \$2.00 if they returned, completed their task, and completed the post-treatment survey that immediately followed the task. Participants who returned but could not complete the task because their conversation partner did not return were still paid the full \$2.00 compensation. Additionally, participants were incentivized to have "thoughtful, thorough" participation with a bonus of \$1.00 for doing so. Without the bonus, participants earned \$2.75 for approximately 12 minutes of work, resulting in an hourly wage of \$13.75. Finally, participants who completed the study were invited back to a follow-up survey asking one question that took only seconds to answer. All participants who completed this question were paid \$0.50. All participants self-reported living in the United States and were paid above federal minimum wage.

Prior to beginning the pre-treatment survey, all participants read an information sheet to obtain their informed and voluntary consent. Participants had to check a box indicating "I consent" to proceed, which is how we documented consent in an online environment. Participants also had the option to select "I do not consent to participate," which would allow them to leave the survey immediately.

We will share the full consent information sheet upon request. Importantly, among other things, our information sheet shared how long we anticipated the tasks would take, the payment for completing the tasks, and how they would be paid. We also told participants that they could choose to stop participating at any time. We also told participants that we would keep the information they provided confidential, and that we would not be collecting any personally identifying information. Finally, we told participants that anonymized transcripts of their conversation or short essay would be made available for research purposes as described, involving removing any people's names, places, religious or cultural backgrounds, occupations, family relationships, and any other potentially identifying information that they may have disclosed in their conversations (even though they were explicitly not prompted to do so).

Our participants came from Amazon's Mechanical Turk, which has a subject pool broadly demographically diverse within the U.S. population and not comprised mainly of members of groups we should consider vulnerable or marginalized. This research did not differentially harm particular demographic groups.

## Appendix B Sample Sizes and Attrition

3,483 participants completed an initial pre-treatment survey, and of them, 1,801 agreed to return to a follow up task, described as having a conversation with another MTurk Worker or writing a short essay. From those who agreed to return, only 1,032 were randomly assigned to treatment and invited back. Because our design required balance on partisanship, but MTurk has more Democrat workers than Republican workers, the number of participants included in the design was smaller than the number that agreed to participate.

Of the 1,032 participants included in the design, 698 (67%) completed the conversation or short essay and the post-treatment survey. However, because treatment is assigned at the *partnership* level, we pre-registered dropping all cases where the full partnership does not complete the task, resulting in our main analyses having a sample size of 578 participants.

410 of the 578 participants (70.9%) included in the main analyses returned three days later to complete the follow-up to assess durability of treatment effects. We preregistered analyzing all participants that complete this survey item, regardless of whether their partner also completed the item.

We next assess whether rates of attrition differ across treatment and control groups. The first model in Table A.1 shows that, of all partnerships *in the design* (N=516 partnerships), a partnership was no more or less likely to complete the task depending on if they were assigned to treatment or control. This means we didn't have differential attrition for those assigned to have a conversation, a task requiring more coordination than a short essay.

Furthermore, the second model in Table A.1 shows that an individual is just as likely to return to take the follow-up survey whether they were a part of a partnership that was assigned to write short essays or have a conversation. Of the 578 individuals invited back to complete the follow up, 410 did, and it was no more or less likely for an individual to complete this task if they completed a conversation, for example.

Finally, the third model in Table A.1 shows that change in outparty affect, our main outcome of interest, does not explain who returns to take the follow-up survey. Therefore, it is unlikely that the follow-up survey consists of only respondents who had strong treatment effects.

Table A.1: Assessing Differential Rates of Attrition

	<i>Partnership</i> Completed Post-Treatment Survey	<i>Individual</i> Completed Follow-up Survey	
(Intercept)	0.55*	0.71*	0.71*
	(0.03)	(0.03)	(0.03)
Conversation	0.02	-0.01	-0.02
	(0.04)	(0.04)	(0.04)
Change in Outparty Animosity			0.00
			(0.00)
N	516	578	578

Note: \*  $p < 0.05$ . First model tests whether treatment assignment explains whether the partnership finishes the task. The second and third models test whether treatment assignment and effect of treatment explains whether participant returns for follow-up survey three days later.

Furthermore, in Appendix C, we show that we have balance on all pre-treatment observables except one in the sample used for analyses after attrition. We fail to find any strong determinants of attrition from these balance tests.

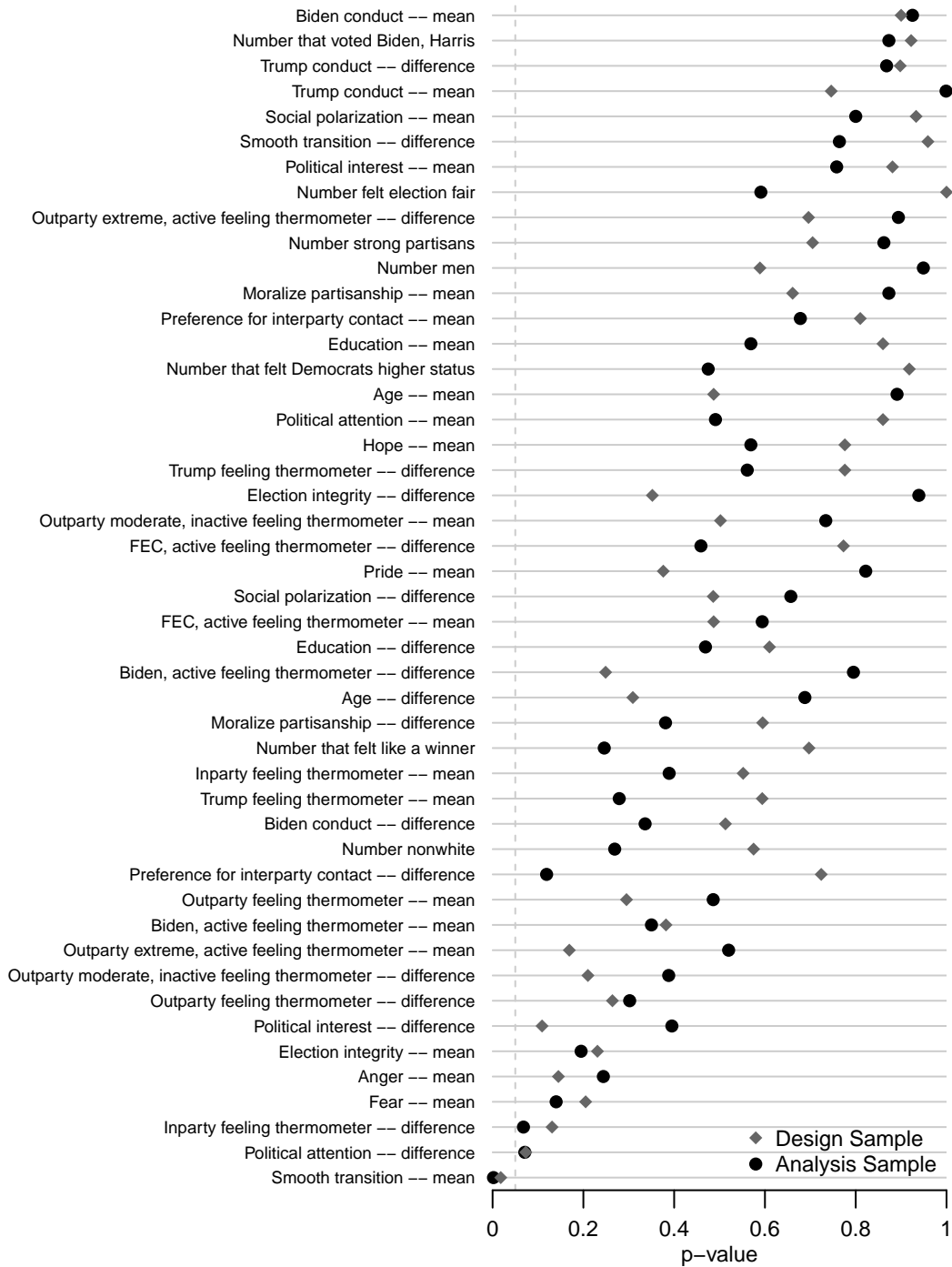
## Appendix C Blocking Strategy

Since treatment is assigned at the partnership level, we sought to maximize partnership-level similarity within each block. For example, if partnership A in a block consisted of a younger Republican and an older Democrat, partnership B ought to have a younger Republican and an older Democrat as well to achieve balance on age at the partnership level across experimental conditions.

We create blocked partnerships using the following covariates from our pre-treatment survey: age, education, non-white, non-male, partisan identity strength, moralization of partisanship, political interest, vote choice, preference for outparty conversation, feelings about the 2020 election (fear, hope, anger, pride), perceptions of Trump and Biden's conduct since the election, attention paid to the 2020 election, political status of parties, feeling like a "winner" after the election, social polarization, perceptions of election integrity, and feeling thermometers toward Trump, Biden, Republicans, and Democrats.

Figure A.1 shows balance on all pre-treatment covariates for the sample consisting of participants included in the experimental design before attrition and the sample used in analysis after attrition to estimate treatment effects. Neither sample was confounded by these pre-treatment variables. The figure plots  $p$ -values from difference-in-means tests between the treatment and control group. The only pre-treatment covariate that had a significant difference between treatment and control groups was attitudes toward whether the transition between the Biden and Trump administrations would be smooth.

Figure A.1: Balance in Partnership-Level Covariates Across Treatment Conditions



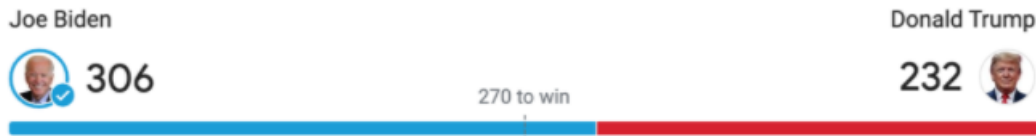
Note: Difference in means p-values assessing balance across treatment conditions for partnership-level pre-treatment covariates. Grey diamonds denote partners included in the design and black squares are partners in the sample in our analyses after attrition.

## Appendix D Experimental Conditions

Figure A.2 displays the instructions participants saw when they entered the chat app if they were assigned to the conversation condition. Figure A.3 shows the instructions those assigned to the short essay condition saw. Note that the conversation instructions differ only in that they mention the partisanship of the participant's partner and ask the participant to have a conversation rather than write a short essay individually. In both conditions, the questions prompting the conversation/short essay were presented in a random order.

Figure A.2: Conversation Instructions (Treatment Condition)

After a long, hard-fought campaign, Joe Biden and Kamala Harris defeated Donald Trump and Mike Pence in the November 2020 election for president and vice president of the United States. While some battleground states were closely contested and legal challenges were raised in several states, the Electoral College formally voted on December 14, 2020 and Joe Biden won 306 Electoral College votes, clearing the 270 votes needed to win. After the Electoral College vote, top Republicans, including Senate Majority Leader Mitch McConnell [congratulated Biden on his victory](#). [Former Vice President Mike Pence declared Joe Biden the winner](#) after Congress formally verified the Electoral College votes on January 6, 2021, despite [challenges raised by some Republican legislators](#) and a disruption in the process when [rioters breached the Capitol building](#). Biden and Harris formally took office upon their inauguration on January 20, 2021.



We've randomly assigned you to have a conversation with someone that belongs to or leans toward the **Republican** party. Please have a conversation about the result of the 2020 United States presidential election between Donald Trump and Joe Biden.

For example, you might talk about some of the following topics.

- How do you feel about the candidates' actions since the election?
- How do you feel about the state of the country these days—fearful, angry, hopeful, proud, or something else?
- Do you think the transition from the Trump Administration to the Biden Administration will go smoothly?
- Do you think that the election in November was administered fairly?

Please have your conversation by sending messages in the chat box below.

With bonus, this HIT is paying above minimum wage. We expect you to provide several comments and to utilize the full 8 minutes without large gaps of time. Participants who do so will receive a \$1.00 bonus.

► Additional instructions about the chat app:

Write your reply...

Send

Done



Figure A.3: Short Essay Instructions (Control Condition)

After a long, hard-fought campaign, Joe Biden and Kamala Harris defeated Donald Trump and Mike Pence in the November 2020 election for president and vice president of the United States. While some battleground states were closely contested and legal challenges were raised in several states, the Electoral College formally voted on December 14, 2020 and Joe Biden won 306 Electoral College votes, clearing the 270 votes needed to win. After the Electoral College vote, top Republicans, including Senate Majority Leader Mitch McConnell [congratulated Biden on his victory](#). [Former Vice President Mike Pence declared Joe Biden the winner](#) after Congress formally verified the Electoral College votes on January 6, 2021, despite [challenges raised by some Republican legislators](#) and a disruption in the process when [rioters breached the Capitol building](#). Biden and Harris formally took office upon their inauguration on January 20, 2021.



We'd like you to share your thoughts on the 2020 United States presidential election between Donald Trump and Joe Biden in an individual short essay.

For example, we'd like you to write about some of the following topics.

- How do you feel about the candidates' actions since the election?
- How do you feel about the state of the country these days—fearful, angry, hopeful, proud, or something else?
- Do you think the transition from the Trump Administration to the Biden Administration will go smoothly?
- Do you think that the election in November was administered fairly?

Please answer these questions by sending messages in the chat box below.

With bonus, this HIT is paying above minimum wage. We expect you to provide several comments and to utilize the full 8 minutes without large gaps of time. Participants who do so will receive a \$1.00 bonus.

► Additional instructions about the chat app:

Write your reply...

Send

Done

## Appendix E Example Conversation and Short Essay

Below is an example of a conversation and short essay in response to the prompts shown in Figure A.2 and Figure A.3. The length of the conversation and engagement between users is representative of the sample of conversations. Moreover, the participants in the sample stay on topic, demonstrated in this example.

### Conversation

user1 Hello  
user2 hello  
user2 I think Trump egging on that the election was stolen was very irresponsible  
user2 because QANON followers really ate that up  
user1 Rather unique election it was.  
user1 Yeah, completely agree on that.  
user1 News nowadays has become a cesspool.  
user2 I dont really think it was stolen even though I am republican  
user1 I believe there was probably fraud (just like in any election ever) but not enough to turn the outcome.  
user2 I think it was done fairly but also from hearing everyone saying there was fraud I cant tell  
user1 Yeah. I believe turnout was a bit higher too though, because of mail-in voting. More people were willing to actually participate because of lockdowns, etc.  
user2 yeah so its kind of hard to tell  
user2 I also think the state of the country right now is pretty bad even with Biden  
user2 it looks pretty bleak  
user1 If it were a Covid-free, usual year, I would say Trump may have likely won a second term.  
user2 I 100 percent agree  
user1 For sure—Biden or not, the country has a ways to go to recover.  
user2 Has the transition been smooth  
user1 I lean democrat, but I'm registered independent.  
user2 hasnt biden overturned a lot of stuff or something  
user1 Yeah, his first week was overturning quite a bit.  
user1 Too much

## Short Essay

I find this topic to be very interesting. Do I think that the transition from the Trump to Biden Administration will go smoothly? No, I do not. I think this is because of the massive difference in changes between Biden and Trump. Some of the things Biden called for right off the bat were rather shocking to some. I think anytime you go from a Republican to Democrat and vs, it is not going to be entirely smooth.

The way I feel about the candidates since the election is actually quite surprising to myself. Yes, I did support and vote Trump, but his reaction to the election results was sickening. I fully think he acted like an immature monster, and not going to Biden's inauguration speaks volumes on his character. I think Biden accepted and began his presidency with grace, and that is something I greatly appreciate.

I am in fear for the state of this country these days, but that has been going on for a year now. I think Covid- 19 has had a detrimental effect for so many people, and my particular state (MI) has suffered. I am hopeful our country will get back to normal, but fearful for some of the things Biden wants done. However, I am not doubting him entirely, I would like to see what happens.

Yes, I think the election in November was mostly administered fairly. My biggest concern is the mail-in voting. It simply makes more sense that voting in person is the safest, most effective way to vote. I do fear with mail in voting that some people voted twice, or a deceased person's information was used. However, given the pandemic, it was necessary to offer especially for our most vulnerable.

## Appendix F Robustness to Different Operationalizations of Feeling Thermometers

We also examined the extent to which our results were robust to different operationalizations of outparty affect. Consistent with recent work (Druckman et al. 2022), we find that outparty affect was slightly warmer for ideologically moderate, politically inactive outpartisans, compared to ideologically extreme, politically active outpartisans, at baseline. However, we find that the average treatment effect of conversation holds across all operationalizations of outparty affect. Table A.2 shows that regardless of whether individuals were thinking about (1) outpartisans across the country, (2) ideologically extreme, politically active outpartisans, or (3) ideologically moderate, politically inactive outpartisans, we find that conversations with outpartisans can increase positive feelings toward each of these characterizations.

Table A.2: Treatment Effects Using Different Operationalizations of Outparty Affect

	<i>Outparty affect considering...</i>					
	Outpartisans (main result)		Moderate, inactive outpartisans		Extreme, active outpartisans	
Conversation	6.22*	7.51*	6.68*	7.04*	4.13*	4.46*
	(1.41)	(1.82)	(1.41)	(2.05)	(1.35)	(1.76)
Partisanship (Rep.)		0.31		1.98		1.42
		(1.35)		(1.77)		(1.49)
Conversation x Partisanship (Rep.)		-2.58		-0.71		-0.67
		(2.11)		(2.75)		(2.35)
N	578	578	578	578	578	578

Note: \*  $p < 0.05$ . The model includes blocked fixed effects to reflect the design’s randomization of treatment. HC2 robust standard errors are clustered at the partnership level for individuals assigned to the conversation condition. Conversation is 1 if the participant engaged in conversation with outparty member and 0 if the participant wrote an individual short essay. Partisanship is 1 if Republican and 0 if Democrat.

## Appendix G Interpreting the Interaction Term

We designed our study to be powered to detect an interaction of size  $-4.00$  based on results from similar studies. Please see our pre-registration for the simulation study we ran to determine the sample size needed for 80% power for this coefficient size. Importantly, we determined our sample size for our analysis would need to be 584 participants, balanced across conditions. Our analysis sample includes 578 participants, with slightly more in the treatment condition than the control, giving us confidence our study was well-powered.

We determined the coefficient size of  $-4.00$  *a priori* through discussion of what would be a substantively interesting difference between partisans based on coefficient sizes from previous studies using feeling thermometer measures. Our discussion of what would be a substantively meaningful difference between partisans also considered that our treatment was particularly intense. We expected partisan competition to be at extremely high levels immediately following Joe Biden taking office and the events of January 2020. Moreover, the topic of conversation was the electoral competition itself (the 2020 election). Despite the intense treatment, our interaction effect was only  $-2.58$  shown in Table A.7 for outparty affect, our main outcome of interest. We were not powered to detect a coefficient this small. Therefore, we encourage future studies to build on this result and consider conditions that may lead to impactful differences in the effectiveness of cross-partisan discussion.

## Appendix H Robustness to Different Operationalization of Partisan Group Threat

Our results are robust to partisan group threat operationalized in ways other than partisanship. From Figure 1 in the main text, we can see that a majority of the sample did perceive threat as we expected. Here, we exclude the observations from our main analysis that did not perceive threat from the election as we theorized. Specifically, we replicate our results when operationalizing partisan group threat via (1) participants’ personal feelings of being a winner or loser in politics after the 2020 election, (2) participants’ perceptions of the parties’ political status at the time of the survey, and (3) vote choice.

First, we replicate our main results, but only with Democrats who felt like a winner and Republicans who felt like a loser after the 2020 election ( $N = 516$ ). Table A.3 shows that all of our results hold, except support for Democratic values now has significant average treatment effect and heterogeneous treatment effect estimates. Democrats’ increase in support for democratic values due to conversation, relative to writing a short essay, may have been due to a more acute reminder the conversation provided about the threats to American democracy surrounding the 2020 election. However, recall this was an exploratory analysis, and this section shows that average treatment effects and heterogeneous treatment effects are *not* a robust finding to other operationalizations of partisan group threat. We caution readers from interpreting the results in Table A.3 as strong evidence that conversation can improve support for democratic norms pending these results can be replicated in future work.

Table A.3: Results Operationalizing Threat via Feeling Like a Winner or Loser in Politics

	Outparty Affect		Social Polarization		Election Integrity		Democratic Values	
Conversation	6.86*	7.97*	0.10*	0.10*	0.00	-0.02	0.15*	0.26*
	(1.52)	(1.94)	(0.03)	(0.04)	(0.04)	(0.05)	(0.07)	(0.09)
Partisanship (Rep.)		-0.12		-0.06		0.01		0.14
		(1.48)		(0.03)		(0.05)		(0.08)
Conversation $\times$ Partisanship (Rep.)		-2.32		0.01		0.05		-0.25*
		(2.30)		(0.05)		(0.07)		(0.12)
Num.Obs.	516	516	516	516	516	516	516	516

Note: \*  $p < 0.05$ . The model includes blocked fixed effects to reflect the design’s randomization of treatment. HC2 robust standard errors are clustered at the partnership level for individuals assigned to the conversation condition.

Second, when analyzing only participants that felt Democrats were of a higher status ( $N = 532$ ), Table A.4 reports that all of our results hold. Conversation has a positive effect on outparty affect and social polarization, and we find no evidence of heterogeneous treatment effects of conversation due to threat operationalized in this way. The only difference from our main results is that our estimate of the average treatment effect of conversation on support for democratic norms is positive and significant via this operationalization.

Third, Table A.5 reports that our results remain consistent when operationalizing electoral threat via vote choice. We analyze partnerships where the Republican reported voting for Donald Trump and the Democrat reported voting for Joe Biden ( $N = 568$ ). Note that only five partnerships in the sample do not fit this pattern. When operationalizing electoral threat in this way, the treatment effect of conversation remains positive for outparty affect and social polarization, and we again find no evidence of heterogeneous treatment effects of conversation stemming threat.

Table A.4: Results Operationalizing Threat via Perceptions of Parties' Political Status

	Outparty Affect		Social Polarization		Election Integrity		Democratic Values	
Conversation	6.92*	8.13*	0.11*	0.11*	0.01	0.00	0.11	0.21*
	(1.45)	(1.91)	(0.03)	(0.04)	(0.03)	(0.05)	(0.07)	(0.10)
Partisanship (Rep.)		0.72		-0.04		0.02		0.14
		(1.47)		(0.03)		(0.05)		(0.08)
Conversation × Partisanship (Rep.)		-2.40		-0.01		0.03		-0.18
		(2.29)		(0.05)		(0.07)		(0.11)
Num.Obs.	532	532	532	532	532	532	532	532

Note: \*  $p < 0.05$ . The model includes blocked fixed effects to reflect the design's randomization of treatment. HC2 robust standard errors are clustered at the partnership level for individuals assigned to the conversation condition.

Table A.5: Results Operationalizing Threat via Vote Choice

	Outparty Affect		Social Polarization		Election Integrity		Democratic Values	
Conversation	6.31*	7.77*	0.09*	0.10*	0.01	-0.01	0.11	0.19*
	(1.43)	(1.84)	(0.03)	(0.04)	(0.03)	(0.05)	(0.07)	(0.09)
Partisanship (Rep.)		0.57		-0.05		0.01		0.08
		(1.35)		(0.03)		(0.04)		(0.08)
Conversation × Partisanship (Rep.)		-2.91		-0.03		0.03		-0.17
		(2.13)		(0.05)		(0.07)		(0.11)
Num.Obs.	568	568	568	568	568	568	568	568

Note: \*  $p < 0.05$ . The model includes blocked fixed effects to reflect the design's randomization of treatment. HC2 robust standard errors are clustered at the partnership level for individuals assigned to the conversation condition.

## Appendix I Mechanisms

We examine several possible mechanisms, which we pre-registered as exploratory mechanism checks. Our experiment revealed that even in highly competitive contexts, conversation can increase outparty affect, regardless of partisanship and feelings of partisan group threat. There are many possible mechanisms through which conversation can work to reduce outgroup hostility, which we broadly group as the depth and quality of the connection from the conversation experience.

### Depth of Connection

Previous research on intergroup contact suggests that contact is effective because it allows people to connect with one another despite their differences. The first dimension of connection we consider is the depth of this connection, and one way to consider the depth of connection is how much participants actually engaged in conversation with each other. The more individuals discussed, the more we should expect them to have opportunities to connect and subsequently reduce their animosity toward the outparty. We tested this by analyzing the transcripts of the conversations and short essays. The conversations occurred via an online chat, allowing us to easily measure the number of times each participant "spoke" in the conversation. In the control condition, we broke down the short essays into sentences. We regressed the change in outparty affect on the interaction between treatment assignment and the number of "turns" (sentences for the control group; speaking turns for the conversation group) taken by each participant. We found that in the treatment group, each additional turn taken by a participant was associated with a .46 point increase in outparty affect. But, there was no statistically significant association between the number of sentences written and outparty affect in the control group. Moreover, we did not find that Republicans and Democrats participated at different rates. In all, while both groups varied in how much they engaged with our treatment, it was only in the interactive, conversation group where more engagement was associated with improved outparty affect.

The second way we consider depth of connection is whether individuals shared personal information about themselves (self-disclosure), see things from their partner's point of view (empathy), and learn something new about the outgroup (learning). We measured each of these variables on a 5-point likert scale.<sup>3</sup> These questions were asked post-treatment and only of participants in the conversation group. We are very cautious in interpreting the results from this analysis. We do not intend them to be interpreted causally in any way; but rather, we interpret them as correlations that are suggestive as possible mechanisms future research could explore.

Figure A.4 shows the coefficients from simple bivariate linear regressions where the independent variable is the mechanism of interest and the dependent variable is the change in outparty affect. For each mechanism, self-disclosure, empathy, and learning, we observe a positive, statistically significant association with outparty affect. That is, the more someone shared personal information with their partner, felt that they could see things from their partner's point of view, and learned something about the outparty, the more they also improved their attitudes toward the outparty.

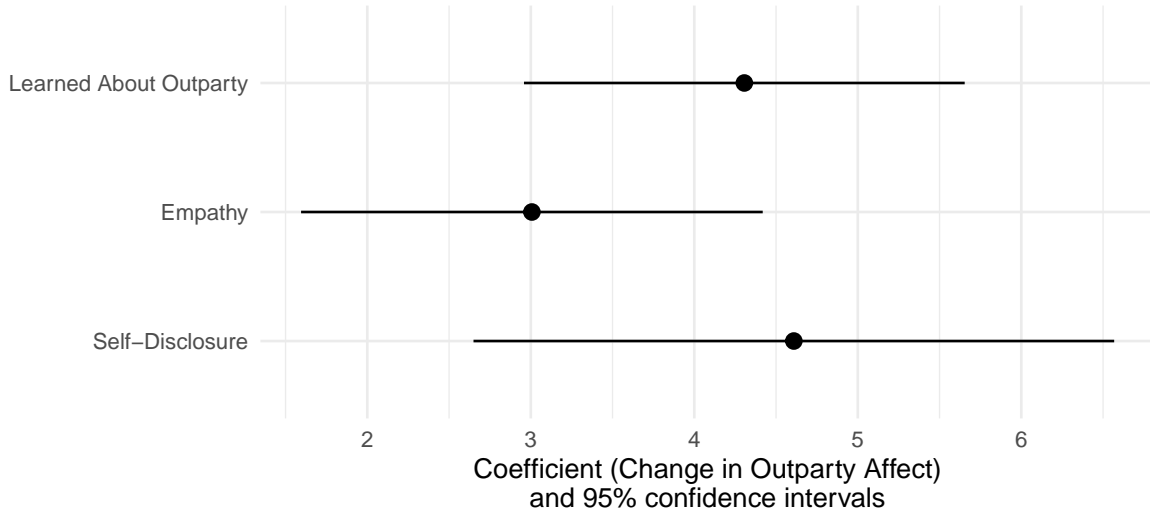
As with our experimental findings, we did not find any evidence to suggest that partisanship conditioned reports of connection and improved outparty attitudes. We interacted each mechanism with partisanship, as we were interested in whether Democrats who reported learning, empathizing,

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<sup>3</sup>We measured self-disclosure using a question adapted from Laurenceau, Barrett, and Pietromonaco (1998): "How personal was the information you disclosed to your partner?" (1=not personal at all; 5=extremely personal). We measured empathy using a question adapted from Davis (1983): "How easy was it for you to see things from the point of view of your conversation partner?" (1=extremely difficult, 5=extremely easy). We measured learning by asking respondents to report how strongly they agreed (1=strongly disagree, 5=strongly agree) with the statement "I learned something about [Democrats/Republicans] from the conversation."



Figure A.4: Exploratory Mechanisms for Improved Outparty Affect



or self-disclosing improved outparty affect more than Republicans who reported these conversation experiences. We find no evidence that self-reporting these dimensions of connection had differential effects for Democrats and Republicans; however, we again interpret these results as exploratory and warranting future research.

### Quality of Connection

Closely related to the depth of connection between the participants is simply the manner in which they connected—was it a positive or negative experience? Recent evidence suggests that the anticipated tone of a conversation, as civil or heated, affects individuals’ willingness to engage in a political discussion even more than simple disagreement (Connors and Howell 2022). Moreover, previous research on intergroup contact suggests that its effectiveness is conditioned on whether the experience is civil and positively valenced. To test this, we return to both the self-reported data on the discussion experience and the transcripts.

First, we asked respondents to simply report whether they had a positive or negative experience in the conversation, following a method used by (Barlow et al. 2012), and expecting that those with positive contact experiences would have greater improvement in outparty attitudes. Respondents’ blunt assessments of whether they had a positive or negative experience was strongly associated with change in outparty affect. The vast majority of respondents reported that they had a positive experience with their partner. Those who reported having a positive experience improved their outparty affect by 10.7 points, compared to those who had a negative experience, who decreased their outparty affect by 2.4 points. The change in affect among those who had a negative experience is not statistically distinguishable from zero. This suggests that negative conversation experiences are not associated with polarized attitudes, while positive experiences are associated with improvements in attitudes. We again caution readers against interpreting these results causally.

When we analyze the free-response descriptions of how participants felt over the course of the conversation, we find - once again - that the positive experiences were associated with more positive outparty affect. We used a sentiment dictionary to estimate the positive and negative sentiment of these descriptions (Young and Soroka 2012). We found that 37% of the descriptions were more

positive than negative when describing how they felt at the *start* of the conversation, but when describing how they felt at the *end* of a conversation, 67% of the responses were more positive than negative. Here too, the more positive the change in feelings, the more people improved their outparty affect. These results are correlational, so we encourage future research to experimentally investigate these concepts as potential mechanisms of politically-charged interparty contact.

With a bit more precision, we return to the full transcripts of the conversations and short essays. We estimated the sentiment of the transcripts at the room-level by aggregating the positive sentiment words of the two participants. In contrast to the self-reported experiences, we find no evidence that the positive or negative tone of the conversation or essay was associated with outparty affect. Once the number of turns - which we analyzed as part of the "depth" of connection - was included in the models, the relationship between sentiment and outparty affect disappeared. This suggests that the civility of the language used is perhaps less important than the amount of language used in shaping outparty affect.

## Appendix J Durability

70.9% of participants returned for our follow-up survey at least three days after the experiment. Table A.6 reports the details for our findings that treatment effects for the main outcome of interest—change in outparty affect for at least three days after treatment. We did not ask about our other two main outcomes of interest (social polarization and perceptions of election integrity). We find a positive and significant treatment effect persists (Cohen’s  $d = .24$ ), and we again fail to find any evidence of a heterogeneous treatment effect across partisans.

Table A.6: Treatment Effects of Conversation Persist for At Least Three Days

	Outparty Affect	
Conversation	4.54*	5.31*
	(1.78)	(2.30)
Partisanship (Rep.)		2.57
		(1.83)
Conversation x Partisanship (Rep.)		-1.56
		(2.79)
Num.Obs.	410	410

Note: \*  $p < 0.05$ . Models assess durability of treatment effects for main outparty affect feeling thermometer outcome. The model includes blocked fixed effects to reflect the design’s randomization of treatment. HC2 robust standard errors are clustered at the partnership level for individuals assigned to the conversation condition.

## Appendix K Figure 2 Results

Figure 2 in the main text displays estimates of sample average treatment effects (SATEs), conditional average treatment effects by partisanship (CATEs), and heterogeneous treatment effects (the difference between the CATEs) for each of our three main outcomes of interest and one exploratory outcome of interest. Full tables of these results are in Table A.7 and Table A.8.

Table A.7: Sample Average Treatment Effects and Heterogeneous Treatment Effects by Partisanship

	Outparty Affect		Social Polarization		Election Integrity		Democratic Values	
Conversation	6.22*	7.51*	0.09*	0.09*	0.01	0.00	0.10	0.19*
	(1.41)	(1.82)	(0.03)	(0.04)	(0.03)	(0.05)	(0.07)	(0.09)
Partisanship (Rep.)		0.31		-0.05		0.02		0.08
		(1.35)		(0.03)		(0.04)		(0.08)
Conversation $\times$ Partisanship (Rep.)		-2.58		-0.02		0.01		-0.18
		(2.11)		(0.05)		(0.07)		(0.11)
Num.Obs.	578	578	578	578	578	578	578	578

Note: \*  $p < 0.05$ . The model includes blocked fixed effects to reflect the design's randomization of treatment. HC2 robust standard errors are clustered at the partnership level for individuals assigned to the conversation condition.

Table A.8: Conditional Average Treatment Effects by Partisanship

	Outparty Affect		Social Polarization		Election Integrity		Democratic Values	
	Dem.	Rep.	Dem.	Rep.	Dem.	Rep.	Dem.	Rep.
Conversation	6.85*	5.59*	0.12*	0.05	0.03	-0.01	0.24*	-0.03
	(2.10)	(1.85)	(0.05)	(0.06)	(0.05)	(0.04)	(0.11)	(0.09)
Num.Obs.	289	289	289	289	289	289	289	289

Note: \*  $p < 0.05$ . Conditional average treatment effects (CATEs) by partisanship. The model includes blocked fixed effects to reflect the design's randomization of treatment. Because these results are conditional on partisanship, there are no clustered standard errors (each participant's partner is an out-partisan and therefore not included in CATE estimation).

## Appendix L Willingness to Engage in Cross-partisan Conversation as a Moderator

We asked participants in the pre-treatment survey: “If you were talking with a typical member of the [Democratic/Republican] party, would you be willing to have a conversation with them about the 2020 presidential election?” The response options were definitely yes, probably yes, probably not, and definitely not. We binarized this variable into a yes or no response. In our sample, 241 (41%) said they would *not* be willing to have this kind of conversation, while 337 (59%) said they would. This is representative of our full recruitment sample where 44% indicated they would not be willing to have an interparty conversation and 56% said they would.

We assessed whether having a preference for or against interparty conversation moderated our treatment effects (a pre-registered analysis) in Table A.9. We do not find that having a preference for cross-partisan conversation moderates our main treatment effect findings.

Table A.9: Pre-Treatment Willingness to Engage in Cross-Partisan Conversation

	Outparty Affect
Conversation	6.15* (2.13)
Preference for Conversation	0.51 (1.87)
Conversation $\times$ Preference for Conversation	0.17 (2.59)
Num.Obs.	578

Note: \*  $p < 0.05$ . The model includes blocked fixed effects to reflect the design’s randomization of treatment. HC2 robust standard errors are clustered at the partnership level for individuals assigned to the conversation condition.

## Appendix M Willingness to Engage in Cross-partisan Conversation as an Outcome

We asked respondents, both pre-treatment and post-treatment, three questions about their willingness to have future cross-partisan conversations.

First, we asked about their willingness to have a conversation with an outpartisan about the 2020 election—the same topic as the treatment. Specifically, we asked: "If you were talking with a typical member of the [Democratic/Republican] party, would you be willing to have a conversation with them about the 2020 presidential election?" The response options were a four-point Likert scale from 0 (definitely not) to 3 (definitely yes).

Second, our social polarization scale asked respondents to report their willingness to talk about "politics" with an outpartisan. The response options were on a four-point Likert scale from 0 (very unlikely) to 3 (very likely). Third, also on the social polarization scale, we asked respondents to report their willingness to talk about "sports or pop culture" with an outpartisan. The response options were on a four-point Likert scale from 0 (very unlikely) to 3 (very likely).

For all three topics, we use the change score as our outcome and estimate average treatment effects of conversation and heterogeneous treatment effects by partisanship. Table A.10 shows that conversation causes an increase in respondents' willingness to interact with an out-partisan for all three topics. Interestingly, as we abstract away from the experiment's specific topic (the 2020 election) estimated average treatment effect sizes wane, but all remain positive and significant. Consistent with our findings in the main text, we find no evidence of heterogeneous treatment effects by partisanship. We find no evidence that partisan group threat conditions the ability of conversation to decrease one's self-reported likelihood of having future cross-partisan conversations.

Table A.10: Treatment Effects of Conversation on Future Cross-Partisan Conversation

	2020 Election		Politics		Sports or Pop Culture	
Conversation	0.21*	0.23*	0.17*	0.16*	0.12*	0.19*
	(0.07)	(0.08)	(0.06)	(0.08)	(0.06)	(0.08)
Partisanship (Rep.)		0.08		0.07		0.00
		(0.06)		(0.06)		(0.05)
Conversation × Partisanship (Rep.)		-0.04		0.02		-0.13
		(0.10)		(0.10)		(0.09)
Num.Obs.	578	578	578	578	578	578

Note: \*  $p < 0.05$ . The model includes blocked fixed effects to reflect the design's randomization of treatment. HC2 robust standard errors are clustered at the partnership level for individuals assigned to the conversation condition.

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