CROSS-IDEOLOGICAL COMMUNICATION: THE IMPACT OF REAL CONVERSATIONS COMPARED TO IMAGINED ONES

by

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DISSERTATION ABSTRACT

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Political polarization has visibly increased in the last few years. A sense of divisiveness has been exacerbated by a surge in social media communication about contentious issues which has been replacing face-to-face conversations about these topics. Evidence shows that people avoid discussing hot-button topics face-to-face and hold pessimistic expectations about how these interactions will go. However, research has shown that these conversations tend to go better than expected. Intergroup Contact Theory suggests that interacting with those in other groups can reduce intergroup conflict. This opens the question of whether there are benefits of having people engage in face-to-face cross-ideological conversations. The present dissertation aims to answer this through an experimental study conducted online via video calls. In one condition, pairs of people with opposing views on a moral issue were instructed to have a short conversation about that issue. In the other condition, people imagined such conversations instead. Outcomes from the actual conversations were compared to expectations about the imagined ones.

Using a broad sample of adults from Argentina (n = 170) with polarized opinions, this study measured A) whether an agreement on the topic was reached or expected; B) participants'

assessments about the quality (real or imagined) of the conversation and their partner; C) participants' willingness to engage in future cross-ideological conversations; and D) change in participants' opinion on the issue after conversation or imagination. Contrary to predictions, there were no significant differences in the proportion of participants reaching agreement between those who had conversations and those who imagined them. Also contrary to predictions, participants' opinions on the issue did not change. However, consistent with hypotheses, those who engaged in an actual conversation rated the experience more positively than those who imagined one, regardless of whether an agreement (actual or expected) was reached. Finally, participants who had actual conversations reported greater willingness to engage in future cross-ideological communication than those who merely imagined them. This study demonstrates the benefits of face-to-face dialogue in communication about contentious ideological issues and offers a practical paradigm for future studies.

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CHAPTER I: INTRODUCTION

In recent years, the United States, Britain, Brazil, and many other countries have experienced intense political polarization (Boxell et al., 2020; Finkel et al., 2020). This polarization has become a topic of great interest in social psychology, among other disciplines. The rise in polarization and its stark visibility can be attributed to many factors, among which are the divisive ways of presenting ideas and identities in social media (Rogers & Jones, 2021; Sunstein, 2018) and the shift of society to the use of digital and online platforms as a main form of communication, especially since the COVID19 pandemic (Barnes, 2020; Parlak Sert & Başkale, 2022). This shift has left people to discuss important and potentially polarizing topics such as freedom of expression, abortion laws, and vaccine trust on digital platforms rather than face-to-face (Persily & Tucker, 2020; Sunstein, 2018; Wagner, 2019). Moreover, some studies show that people avoid or have negative expectations about real face-to-face conversations, particularly those with strangers or about heated topics (Chen & Rohla, 2018; Dunn, 2019; Epley & Schroeder, 2014).

However, debates around political and moral disagreements are a key element for healthy democracies (Gutmann & Thompson, 1998). As a matter of fact, research suggests that live, face-to-face interactions not only go better than people expect in certain contexts (Binnquist et al., 2022; Epley & Schroeder, 2014) but that these interactions also have the potential to reduce prejudice and resolve conflict (Drolet & Morris, 2000; Pettigrew & Tropp, 2006). This raises some crucial questions: Are there benefits of engaging in face-to-face cross-ideological conversations? If so, why do people have pessimistic expectations about them? And could these conversations change people's attitudes and willingness to engage in such dialogues? Answering

these questions will shed light on how to design future interventions aimed at promoting communication and consensus across ideological divides.

The present dissertation aims to answer some of these questions by presenting an experimental study with two conditions. One condition paired up people with opposing views on a moral issue, instructing them to have a short conversation about the issue. In the other condition, people were asked to imagine such a conversation instead. One of this study's aims was to examine how outcomes from these conversations compare to people's expectations of outcomes when they imagine having a conversation. More importantly, this dissertation intends to look at the effects of engaging in a short cross-ideological face-to-face dialogue (as opposed to merely imagining it) on people's assessment of interactions with others holding different views, people's willingness to engage in these kinds of interactions, and people's individual opinions on the issue being targeted.

In this first chapter, I will overview the relevant literature to set the ground for a study of cross-ideological conversations and people's expectations after imagining them. First, I will review some of the political and affective polarization research – particularly the role of social media (one of the main venues for communication these days – Barnes, 2020; Parlak Sert & Başkale, 2022). I will also look at research on people's perceptions of polarization and how these perceptions may affect their attitudes toward the outgroup. Then I will dive into understanding the already known benefits of contact between different groups through the lens of Intergroup Contact Theory (Allport, 1954) and highlight the gaps in the literature, which need to be filled to help understand the effects of cross-ideological contact through conversations.

In the next section of this introductory chapter, I will quickly overview research on the factors that improve or worsen communication, especially communication about difficult topics

such as moral or political issues. This will include a review of existing evidence of people's negative forecasts, avoidance of difficult conversations, and hypotheses about the effects such attitudes could have. This section will also cover previous research that has tried to test the differences between forecasts and the effects of actually engaging in such conversations, highlighting its relevance and limitations. Then, I will quickly discuss the potential role of intellectual humility as a correlate of the outcomes of cross-ideological conversations. Finally, I will go over the findings from a series of preliminary studies that preceded the dissertation and introduce the present study and its predictions before moving on to the next chapter, which will describe the methodology of the study.

Polarization

We can define polarization as the process by which two entities move towards opposite extremes on a continuum of opinions or viewpoints (Wu et al., 2022). When communication on heated moral or political topics mainly takes place on social media, polarization is exacerbated (Persily & Tucker, 2020; Sunstein, 2018) and accompanied by other negative consequences, such as political violence (Van Dijcke & Wright, 2021). Social media have been criticized for sorting their users into "echo chambers" so that users are selectively exposed to content that aligns with their own beliefs (Barberá et al., 2015; Madsen et al., 2018; Pariser, 2011). However, a recent growing body of work suggests that the idea of people being sorted into like-minded bubbles might have been exaggerated (Bakshy et al., 2015; Eady et al., 2019), and other work suggests that polarization is high even among those who do not use social media (Boxell et al., 2017). Although it might seem that the role of social media in polarization remains an open debate, recent work from Rathje et al. (2021) provides strong evidence that outgroup animosity is highly successful at generating engagement on Facebook and Twitter. By analyzing millions

of posts from news media and US congressional members, Rathje et al. (2021) found that outgroup language in these platforms' posts consistently emerged as the strongest predictor of shares and retweets.

Another important question is how polarization affects people's perceptions of those in the outgroup. A recent study found that polarization can trigger counter-projection of traits (seeing the opposite of oneself in others) onto those in the outgroup across a political divide (Denning & Hodges, 2022). Counter-projection, in turn, could contribute to intergroup conflict by intensifying a divisive perspective and hindering the chances of cooperation. Moreover, a new line of work studying the perception of polarization has found what is now defined as "misperceived polarization": people consistently over-estimate the extremity of those in the outgroup, and this has been found to drive actual polarization (Lees & Cikara, 2021), meaning that people's incorrect views about those in the outgroup have intensified the division between them.

Nevertheless, some new studies have found that correcting people's over-estimations of polarization can significantly adjust their beliefs and even influence their attitudes toward polarizing actions by reducing said attitudes, such as lowering their willingness to engage in partisan violence (Lees & Cikara, 2021; Mernyk et al., 2022). This suggests that interventions that drive people to learn the actual perspectives of those in the outgroup could help prevent increasingly negative attitudes toward them. Within this context, one important question is whether face-to-face communication between opposing sides could help people to accurately learn more about outgroup members' actual perspectives.

Intergroup contact

Reviewing previous research on the effects of inducing interaction between people across a divide is a relevant step to addressing what is already known and where there are still unanswered questions. Social Psychology has a long history of studying the potential benefits of interaction between people from different groups. Intergroup Contact Theory is the idea that increasing interaction between outgroup and ingroup members reduces prejudice towards the outgroup that is exhibited by the ingroup (Allport, 1954; Pettigrew, 1998). Even though much of the research on Intergroup Contact theory has addressed encounters between advantaged and disadvantaged racial or ethnic groups, an important and extensive meta-analysis shows that these findings can also be extended to other sets of group encounters (Pettigrew & Tropp, 2006).

Early on in the research in this area, Allport outlined the necessary conditions for Intergroup Contact Theory to work (Allport, 1954). Allport's optimal conditions for the encounter between two groups have been stated as: "1) Equal group status within the situation, 2) Common goals, 3) Intergroup cooperation, and 4) Authority support " (Pettigrew, 1998, p. 66). An example of an intervention following such ideal conditions would be to mix people from different groups on the same team (equal group status, intergroup cooperation) for a sports competition (common goals, authority support), such as in a field experiment in Iraq that showed that having Muslim teammates reduced Christian soccer players' prejudice towards Muslims (Mousa, 2020; Paluck & Clark, 2020). However, a thorough metanalysis has shown that these conditions are sufficient but not essential to see effects on prejudice reduction (Pettigrew & Tropp, 2006).

When studying the effects of dialogue across ideological divides, it is also important to consider how contact may improve intergroup relations in ways beyond reducing prejudice and

increasing tolerance (Tropp & Mallett, 2011). Research in social psychology is more limited and also conflicted in these areas (Brown et al., 2007). Some studies have shown that contact correlates positively with political solidarity expressed by advantaged groups towards disadvantaged groups (for example, members of ethnic majorities) or with cis-heterosexuals supporting empowering policies for minority groups (Dixon et al., 2007; Hässler et al., 2020; Kamberi et al., 2017; Selvanathan et al., 2018). However, a recent metanalysis found limitations in these results as the within-subject effects on solidarity did not hold in the long term. In contrast, the differences between subjects *did* hold in the long term, suggesting that other factors influencing people's solidarity might have acted as confounding variables in the reported results from previous research (Sengupta et al., 2022). Some studies have shown that intergroup contact can help promote social change or at least people's attitudes towards it. However, other studies have found the influence of contact on people's openness to social change to have opposite effects when it comes to advantage or disadvantaged groups (Al Ramiah & Hewstone, 2013; Hässler et al., 2020). Further controlled studies are needed to understand the extent to which intergroup contact can promote tolerance and reduce conflict in measures that go beyond prejudice reduction.

It is also crucial to consider the type of contact taking place when studying the effects of intergroup contact. Previous research has shown that for intergroup contact to reduce conflict and prejudice, the interaction must be a positive one (Pettigrew, 1998; Pettigrew & Tropp, 2006; Tropp, 2012). Researchers found that negative intergroup contact makes group membership categories salient, which in turn facilitates contact generalization, therefore biasing future intergroup contact experiences (Paolini et al., 2010), as well as increasing liking for the ingroup

(Friehs et al., 2022; Meleady & Forder, 2019). However, in real-life interactions, there is no guarantee that a positive intergroup contact experience will take place.

Therefore, in order to understand the benefits of contact between groups in scenarios that resemble "real life," an important step in this area of research would be to study interactions that are not primed to be positive or negative and instead just provide the space for the interaction to occur. Studies like these have people discuss a moral or political topic with another person or a small group, and under conditions where the participants in the conversation hold opposing opinions (Binnquist et al., 2022; Navajas et al., 2019; Niella et al., 2021). However, there have only been a small number of these studies given how challenging it is to run such experiments in the lab, especially since the beginning of the COVID-19 pandemic, which has put limitations on researchers' ability to run in-person studies and created difficulties in finding participants with extreme opinions and scheduling multiple participants at the same time.

Moreover, research on intergroup contact theory is often based on the idea that there is an advantaged and a disadvantaged group (e.g., ethnic or religious groups, where the group in the minority is disadvantaged; Di Bernardo et al., 2022; Pettigrew & Tropp, 2006; Sengupta et al., 2022). When we think of polarization on political or moral issues, whether one group is more advantaged than the other can be less obvious. If people have differing opinions on a moral issue, then context and individual differences can influence whether one group is disadvantaged and the other not.

A lot of the research on polarization, including the present project, does not focus on advantaged versus disadvantaged groups but rather on people with extreme opinions on a topic, most often moral or political (Binnquist et al., 2022; Navajas et al., 2019; Rathje et al., 2021). Only a handful of studies have aimed to study intergroup contact under such characteristics: One

was done in a crowd with the goal of understanding the process by which people in small groups reached or did not reach a consensus on polarizing moral topics (Navajas et al., 2019). Only one other study that I know of has tested the effects of cross-ideological conversations on people's ideological attitudes (Binnquist et al., 2022). Thus, further research is needed to understand the extent to which contact is beneficial for intergroup relations by measuring outcomes beyond prejudice reduction in controlled studies. There is a shortage of studies measuring how cross-ideological conversations as a form of contact can promote tolerance and reduce conflict, probably in large part due to how challenging it is for researchers to run them in the lab.

Face-to-face conversations

When considering contact between opposing moral or political views in a setting closer to real life, an obvious type of encounter would be face-to-face conversations. How would a face-to-face, one-on-one conversation about a heated topic between people with opposite opinions unfold? Findings from previous research may suggest that discussing moral or political topics can lead to polarizing identities, as people align themselves more strongly with their ingroup and against the outgroup, which may create conflict and hinder constructive dialogue (Bail et al., 2018; Geiger, 2014; Martel et al., 2021). However, face-to-face communication comes with unique characteristics that might overcome the potential divisive identities that come up with moral or political topics. Real-time exchange, immediate feedback and clarification, the presence of non-verbal cues, and the opportunity to adjust communication to fit the situation or needs of the conversation partner have been shown to be beneficial for increasing persuasion and conflict resolution (Campellone & Kring, 2013; Cialdini & Goldstein, 2004; Hargie, 2011; Patterson, 2018; Weger et al., 2014).

However, in current times, people lean on different digital platforms for communication and exchange of ideas, many of which lack a lot of the features that characterize face-to-face conversations. Furthermore, these platforms have other features that have been shown to contribute towards more animosity and divisive speech (Rathje et al., 2021; Sunstein, 2018). Therefore, when presented with a heated moral topic, one could expect people to believe that interacting with someone from the outgroup might not go well (Binnquist et al., 2022; Dunn, 2019). Nevertheless, when inducing people to discuss these topics in a one-on-one face-to-face format, we expect that the factors of face-to-face interaction mentioned before may overcome these expectations and therefore enhance chances of finding common ground, even between people holding opposite opinions (Navajas et al., 2019). For instance, having a chance for dialogue without an audience (as opposed to what happens in a lot of social media interactions) frees participants from being representatives of their ingroup. This may help promote a calmer and less defensive conversation (Binnquist et al., 2022) and may also give conversants a chance to listen and respond to a different ideology.

Another key factor when understanding why face-to-face interaction can be beneficial for cross-ideological communication is people's tendency to be more polite and conform to opinions different than their own when engaging in face-to-face conversations. This can be explained by the existence of social norms that influence human cooperation (Fehr & Fischbacher, 2004). These norms may be powerful in face-to-face interaction and drive cooperative behavior (Turner, 2002). In terms of conversations, and in particular those about polarizing issues, people often restrain from showing their divergent or challenging views and engage in self-censorship to maintain harmony and avoid social rejection (Carlson & Settle, 2016; Mutz, 2002; Neubaum & Krämer, 2018). This may be especially true between strangers (Mourad, 2021; Ye, 2019). Such

behavior is in sharp contrast to the more hostile and divisive speech that takes place on social media, where the cost of reducing harmony and risks of social rejection are lower (Neubaum & Krämer, 2018; Persily & Tucker, 2020; Rathje et al., 2021; Sunstein, 2018). Moreover, one-on-one dialogue offers opportunities to make the exchange of contentious ideas more harmonious and inviting, such as when conversation partners show conversation receptiveness (i.e., use language to communicate one's willingness to engage with opposing views thoughtfully). Researchers have found conversation receptiveness to have an important positive impact on how people relate, improving how they perceive each other, including during polarized political discussions (Yeomans et al., 2020).

The present study will test face-to-face communication in a virtual meeting setting (i.e., Zoom), which has been successfully used in previous research with similar purposes (Binnquist et al., 2022). Thus, the communication in this study will not be "true" face-to-face conversations and it is important to consider how online virtual meetings may differ from that true face-to-face communication. For example, platforms like Zoom may negatively affect people's conversational behavior relative to "true" face-to-face conversations (Balters et al., 2023). On the other hand, Zoom conversations might have the advantage of providing a greater sense of distance from the interlocutor, which in turn might help participants feel more comfortable with disagreement when talking about polarizing topics than if they were talking in person (in the same physical space). Despite these differences, virtual meetings provide people with many of the aspects that define face-to-face communication: the opportunity to see each other, listen to each other, and take turns to speak. Virtual meetings on the platforms like Zoom mimic a lot of the beneficial aspects of face-to-face conversation, especially in comparison to communication on social media, where communication between two people is often not synchronous, it does not

provide non-verbal cues or allow for perceived changes in voice, and it is often framed in terms of conversation between people representing opposing teams, all which might hinder a peaceful conversation about a heated topic (Binnquist et al., 2022; Sunstein, 2018).

Forecasts and avoidance

Regardless of the potential benefits of face-to-face interaction when it comes to divisive topics, people seem reluctant to have such interactions as they choose avoidance over conflict in these contexts (Dunn, 2019). This has been shown to affect social gatherings such as family holiday dinners, where people may be conversing with close others who hold different political views (Chen & Rohla, 2018). People also report feeling uncomfortable or awkward when having to think about engaging in any kind of conversation with a stranger, not just an ideologically polarized one (Epley & Schroeder, 2014). But evidence shows that when people are induced to have these conversations (at least about non-polarizing topics), they find them more enjoyable than they imagined (Epley & Schroeder, 2014; Sandstrom & Dunn, 2014).

The faults in people's expectations about face-to-face conversations can be explained through affective forecasting – people's tendency to overestimate the intensity and duration of their emotional reactions to events (Ayton et al., 2007; Wilson & Gilbert, 2005). But do affective forecasting errors apply to cross-ideological conversations? Evidence suggests so. A recent study by Binnquist et al. (2022) tested this by having people report how they expected a conversation would go with someone who held an opposite opinion on a political issue. For this, participants were asked to rate different aspects of the hypothetical conversation (e.g., how stressful it would be, enjoyable it would be, etc.) as well as how they expected to feel about their potential conversation partner (e.g., how much they would like them, feel judged by them, etc.). Then, the participants actually had to have these conversations and were asked to rate these aspects after

the fact (e.g., how stressful it was, how much they liked their conversation partner, etc.). The researchers found participants' forecasts about the conversation and their conversation partners to be significantly more negative than their ratings after experiencing them.

This opens the question of how these incorrect predictions might, in turn, negatively affect people's attitudes toward the outgroup. A recent body of work has looked at Imagined Intergroup Contact Theory (Crisp & Turner, 2009), where researchers found that when people are induced to imagine positive contact experiences with the outgroup, the effects on prejudice reduction are equivalent to those in real intergroup contact studies (Miles & Crisp, 2014). However, this outcome has so far only been found for imagined scenarios primed to be positive. In many real-life situations, when we imagine a scenario involving a conversation with someone whose position differs from our own, this might not be the case. And the results for negative intergroup contact experiences suggest that imagining negative situations may, in turn, negatively affect people's attitudes toward those with opposing ideologies. Therefore, an important step would be to contrast the effects of imagining a cross-ideological conversation with experiencing a real one without priming a positive scenario – in order to see whether such imagined conversations still have positive outcomes.

Binnquist et al.'s (2022) study came close to testing this question, with some limitations. Participants were only indirectly asked to imagine a contentious conversation, as they were told they might have a conversation with someone who had an opinion opposite to their own, and then asked to forecast how they expected the conversation to go by rating it on a series of dimensions. Moreover, the study was run using a within-subject design, which might limit the conclusions of contrasting the outcomes of real versus forecasted conversations: it is important to understand the effects of having these conversations without having first imagined them. Finally,

Binnquist et al. (2022) tested questions about the outcomes of real versus imagined conversations in an exploratory manner. The main focus of their study was something different: to answer whether the presence of an ingroup audience affected the course of the conversation. Therefore, they tested the effects of other factors in the same study, leading to the presence of potential confounds, such as the fact that all participants had a conversation with an ingroup member before the polarized conversation, which might have affected the way these cross-ideological dialogues unfolded. To better isolate how expectations and actual conversation differ, a between-subjects design without testing other factors may be more adequate, as it allows one to attribute the differences between conditions solely to the presence or absence of a conversation.

As Binnquist et al. (2022) suggest, people tend to have negative or pessimistic expectations about how conversations on polarizing topics will go. However, we do not know whether these expectations remain after engaging in thoughtful imagination about the conversation and how said expectations compare to being tasked with actually having such a conversation. Therefore, in the present research project, I use a design where people either get to talk to somebody else whose position on a contentious issue is opposite from their own for a short period of time or are asked to imagine the conversation with this person instead. The only difference between conditions is whether the two participants in a dyad get a chance to talk to each other or imagine talking instead.

Intellectual Humility

A relevant individual difference trait in interpersonal relations, especially when it comes to exchanges about polarized ideological topics, is Intellectual Humility. Intellectual Humility has received various definitions (Porter, Baldwin, et al., 2022), but it can generally be understood as a measure of willingness to recognize and appreciate the limits of one's knowledge, an

openness to new ideas and perspectives, and the ability to revise one's beliefs in light of new evidence (Krumrei-Mancuso & Rouse, 2016; Leary et al., 2017; McElroy et al., 2014; Porter & Schumann, 2018). Evidence shows that certain measures of Intellectual Humility have strong negative correlations with extremism and affective and political polarization (Bowes et al., 2020, 2022; Mellers et al., 2019). Moreover, there is common agreement that fostering Intellectual Humility is desirable to combat polarization and increase civility and collaboration in society (Bowes et al., 2020; Porter, Elnakouri, et al., 2022; Porter & Schumann, 2018; Stanley et al., 2020). However, the ideal framework and methods to approach and measure Intellectual Humility are still being developed (Porter, Baldwin, et al., 2022). Existing measures are better suited as measures of stable individual trait differences rather than as an outcome variable to measure state-level changes (Bowes et al., 2020). The present study will include exploratory analyses to examine how Intellectual Humility correlates with participants' assessments of real or imagined cross-ideological conversations.

Present goals

I aim to study whether face-to-face cross-ideological short conversations can help people on opposing sides communicate and learn from each other's perspectives. And more importantly, I seek to compare the outcomes of these conversations with the expected outcomes from people who imagine these conversations. When tasked to have the conversation, participants will also be instructed to try to reach an agreement on the ideologically polarizing topic as a mechanism to ensure attention and focus on the task. Those who imagine the conversation will be given the same instructions, only in their case, they will be asked to imagine trying to reach an agreement. Given the current climate of polarization and social media communication, we expect that people imagining a conversation with someone across the ideological divide will predict reaching an

agreement with that person less often; will predict reporting a more negative rating of the interaction and the interaction partner; and will be less willing to engage in such interactions in the future, compared to the outcomes for those who hold an actual cross-ideological conversation.

Preliminary studies

My collaborators and I have tested some of these ideas in a series of preliminary studies and found promising results. One exploratory study analyzed results from a big crowd experiment (a 10,000-person crowd of attendees at a TEDx event, plus hundreds of virtual attendees who participated simultaneously) conducted in Argentina (Navajas et al., 2019). This study compared the proportion of small independent groups that reached an agreement when discussing a polarizing moral issue, when the groups were composed of at least two participants with extreme opposite opinions about the issue, with how often people online expected imaginary groups with the same composition to behave. We found that the real group conversations resulted in agreement significantly more often than people online predicted they would (Niella et al., 2021).

Secondly, we ran a study at the University of Oregon comparing student participants assigned to one of two conditions, using students enrolled in undergraduate classes from different majors (Niella, 2019; Niella et al., 2021). In one condition, the whole classroom was divided into dyads. The dyads were instructed to discuss a moral issue and to try to reach a point of agreement on the issue. In the other condition, each participant in the classroom received information about the position on the moral issue from a previous participant and was asked to imagine having a conversation with that participant and then evaluate whether they would expect to reach an agreement with the other participant or not. Once more, we found that the dyads who

had real interactions reached agreement significantly more often than those who imagined the conversation expected they would.

While these results are promising, the first study was an exploratory analysis, and the second study allowed for distance between opinions on the issue to vary naturally – participants were not necessarily on opposite sides of an issue. Furthermore, participants in the second study either had or imagined conversations about two moral issues. A significant difference between imagined and actual results was only found for the first of the two moral issues tested. The order of the moral issues was not manipulated so that the same issue always came first. The first moral issue was related to freedom of speech; this was the one that produced a significant result. Cyber privacy was always the second issue and yielded non-significant results. However, it is unclear whether the first issue led to significant results because of the particular topic used (freedom of speech) or simply because participants worked on it first (perhaps getting fatigued, jaded, or less motivated by the time they got to the second issue). The present project has participants discussing just one issue, but that one issue was selected from a variety of six possible issues, specifically one that the two participants had opposite extreme positions on.

Present study

In this study, I aim to compare the agreement rates between individuals with opposing moral views engaging in actual conversations versus those imagining such discussions. Participants will converse or envision dialogues while participating via a video call platform (Zoom) which has been shown to effectively facilitate controlled cross-ideological conversations (Binnquist et al., 2022). Pre- and post-conversation questionnaires will be used to examine participants' opinions, perceptions, and future willingness to engage in similar discussions.

My pre-registered[1] predictions are the following:

1. The percentage of people who imagine the conversation and expect to reach an agreement will be significantly less than the percentage of actual dyads who reach an agreement during a real conversation.
2. When asked about qualitative aspects of the conversation and the conversation partners, ratings from participants who have the actual conversation will be significantly more positive than those from participants who imagined the conversation.
3. People's willingness to engage in such conversations in the future will be greater among those who had real conversations versus imagined ones.
4. People's personal opinions on the moral topic will change more after real conversations than imagined ones.

Moreover, I propose a few exploratory analyses. I will explore how people's assessments of the conversation quality, their reported willingness to engage in future conversations, and certain characteristics of their individual opinion (such as how informed they felt about the topic) may affect how people's personal opinions on the moral topic changed after imagining or having the conversation. I will also look at how self-reported Intellectual Humility (Leary et al., 2017) correlates with the reports of conversation quality and willingness to engage in future conversations, as well as with changes in people's personal opinions.

This study provides a more controlled and potentially friendlier setting for contentious conversations, with the benefit of the conversation partners being strangers (they might tend to be more polite than with somebody they know well; Mourad, 2021; Ye, 2019). However, if the outcomes are as predicted, we would still show that agreement and other positive conversation

[1] The study design and its predictions have been pre-registered in AsPredicted.org, pre-registration #112587

outcomes and evaluations between people holding opposite opinions are possible. This is relevant when there is evidence that most people think these positive outcomes are impossible, especially when imagining these kinds of conversations (Binnquist et al., 2022; Dunn, 2019; Niella et al., 2021). Testing my predictions could provide insights into how such induced short conversations could work as an intervention to help diminish the heated and divisive aspects of polarization.

CHAPTER II: METHODOLOGY

Overview

Using dyads of people with opposing opinions, this study compared rates of agreement reached after actual conversations with rates of agreement expected by people who imagined these conversations. For the real conversations, people were instructed to try to reach an agreement on the topic during a 5-minute conversation on Zoom, a video call platform, which has been demonstrated as an effective way to test cross-ideological conversations in a more controlled setting (Binnquist et al., 2022). For imagined conversations, people also participating on Zoom were instructed to imagine having such a conversation.

Participants responded to different questionnaires before and after the conversation (imagined or real) that measured their opinion on a moral topic, their perceptions of the conversation and their conversation partners, and their willingness to have such conversations in the future.

Participants and recruitment

Participants in this study were drawn from a subject pool in a collaborating lab at Universidad Torcuato Di Tella in Buenos Aires, Argentina. This subject pool is composed of adults who have previously participated in other experiments with the lab and/or of people who have stated interest in participating in future studies. The lab recruits people for their participant pool beyond undergraduate students (see demographics of my sample below) by reaching out to the community through various platforms: social media, news articles, classes, and newsletters at various universities in the country. Using information from previous studies, I reached out to potential participants by selecting those who already stated extreme positions on various moral issues but who had not discussed them with someone else in a previous experiment (people in

this pool who participated in a previous similar study and previously discussed the same moral topics used in the current study were filtered out from participating). Moreover, the lab continuously recruits people for their subject pool on a regular basis, collecting data on moral issues, and these participants were invited to participate in this study as well.

Our anticipated target sample was between 175 and 250 participants. Using the Power package in R, we ran a power analysis for Chi-square tests (for whether agreement is reached or not) and calculated that a sample of 174 participants (58 dyads – and thus 116 participants for the Interaction Condition; 58 individuals for the Imagine Condition) would give us 90% power (using an alpha of 0.05) to detect a medium effect size of w= 0.3. Adjusting the effect size to the one obtained in a previous study similar to this one with an equivalent population (Niella et al., 2021), we calculated that a sample of 216 participants (72 dyads and 72 individuals) would give us 90% power (with an alpha of 0.05) to detect an effect size of w=.27. Our sample size was determined by targeting a number roughly between these two estimates.

In the end, the study included 222 participants. From that total, 52 participants were removed for various reasons: 36 were removed because they were in sessions where they were the only participant attending and the other person did not show up or canceled at last minute; another 16 participants were removed because they did not complete key stages of the experiment, such as the pre or post manipulation forms, due to various reasons. The final sample was of 170 participants. This final sample included 14 ‘half-dyads’ where the data from only one participant in the dyad is considered (9 in the Interaction Condition and 5 in the Imagine Condition). This is because the other participant in the dyad was removed – there was missing data in the forms they filled in individually (their answers were not recorded in the online forms). In these ‘half-dyads’, however, the dyadic activity, whether it was the conversation (Interaction

Condition) or just being present in the Zoom session (Imagine Condition), was completed successfully; therefore, I kept the data from the participant for whom I have completed data in the forms. Nevertheless, the participants who were part of a 'half-dyad' in the Interaction Condition were not considered for analyses that needed a response from both participants (see Chapter 3, Results). In total, the final sample had 92 dyads. A total of 78 out of the 92 dyads were 'full-dyads' (i.e., 156 participants), of which 28 belonged to the Imagine Condition (i.e., 56 participants) and 50 to the Interaction Condition (i.e., 100 participants). A total of 14 out of the 92 dyads were 'half-dyads' (i.e., 14 participants; 9 in the Interaction Condition and 5 in the Imagine Condition).

In our sample of 170 participants, the mean age was of 29.15 years (SD = 11.45), and 65.29% identified as Female, 31.18% as Male, 2.35% identified as Gender Fluid or Non-Binary, and 1.18% preferred not to provide gender information. In terms of education, 6.47% completed a graduate-level degree, 44.70% completed at least some college, 48.24% completed up to high school education, and the remaining 0.59% completed up to elementary school education. When asked about their religion, 34.70% chose Catholicism, 14.12% chose Agnosticism, 10.00% chose Atheism, 32.94% chose "None in particular", 4.12% chose Evangelicalism, and the remaining 4.12% were distributed among the other options (Jehovah's Witnesses, Judaism, Mormonism, Islam, Protestantism, or Other). In terms of Political Party, 25.88% chose "Frente de todos" (center-left wing party in Argentina), 19.41% chose "Juntos por el cambio" (center-right wing party in Argentina), and the remaining 54.71% chose Other or Prefer not to say. In terms of Political Orientation, 12.35% chose Left, 10.59% chose Right, 34.71% chose Center-Left, 17.06% chose Center-Right, and the remaining 25.29% chose "I don't know/Prefer not to answer."

Design

Using a between-subjects design, participants were assigned to one of two conditions, intended to be as similar as possible except for the central manipulation, which was whether participants got a chance to talk to each other (Interaction Condition) or merely imagined the conversation instead (Imagine Condition).

Interaction Condition

A dyad of participants who held opposite opinions about a statement on a moral topic were instructed to have a five-minute conversation to try to reach a level of agreement on that statement. After the conversation, each participant reported individually whether they reached an agreement or not, and if an agreement was reached, what position they agreed on.

Imagine Condition

A dyad of participants who held opposite opinions about a statement on a moral topic were notified of each other's positions and were instructed to individually imagine a five-minute conversation during which they would have to try to reach a level of agreement on that statement. Then they answered individually whether they thought they would be able to reach agreement or not, and if so, what position they would expect to agree on. Then they were given five minutes to write a paragraph describing how they thought the conversation would go.

Procedure

This study had four stages:

Stage 1 (Pre-screening)

In an online survey, all participants were presented with a series of six statements about different moral dilemmas (e.g., "there should be absolutely no limits to freedom of speech"; for a complete list of the statements, please refer to Appendix A). Participants were asked to answer

how much they agreed or disagreed with the statement using an 11-point Likert scale (0 = Completely disagree, 10 = Completely agree). Once this information was collected, we looked for participants who gave extreme answers (i.e., either 0, 1, or 2; or 8, 9, or 10) for at least one of the statements. Then, these participants were invited to sign up for the experiment by filling out an online form. We then paired these participants up based on the following criteria: A) The two participants' answers were at opposite extremes for one of the dilemmas; this dilemma (issue) was the one they were assigned to discuss or imagine in the experiment (if more than one issue met the criteria, we picked one at random using an online random number generator); and B) The two participants' time availability provided on the sign-up form made it possible for them to both participate in the same study session.

Once we had invited participants in the dyad to the online study session, each dyad was randomly assigned to one of the two conditions, using the following proportion: for every two dyads assigned to the Interaction Condition, one dyad was assigned to the Imagine Condition. The reason for this proportion is that in Imagine Condition we obtained two data points per dyad (each participant imagined the conversation separately, so that two independent imagined conversations took place), whereas for the Interaction Condition, for some variables, we only obtained one data point per dyad (only one real conversation took place).

Stage 2

One dyad participated at a time in each session. Each participant joined an online Zoom meeting with an experimenter present. When participants joined, their video was visible, but the microphones on their computers were turned off and only enabled when participants were allowed by the experimenter to talk. Moreover, participants' real names were not visible – instead, their names on the screen were changed to "Participant A" or "Participant Z."

The experimenter guided the session and gave audio instructions and shared their screen with slides that showed the same instructions. This helped to reduce confusion if any minor sound issues occurred due to connection problems. First, the experimenter read the moral issue statement that the participants would be working with (the experimenter presented it on the screen as well). Then, each participant received a link to the Pre-measure Form in the chat. This form was an online survey where participants individually reported their level of agreement with the statement using a Likert scale from 0 to 10. They also answered how sure they felt about their answer, how much they cared about the issue, and how informed they felt about it, using 7-point Likert scales to respond. Participants stayed on Zoom the entire time, they completed the form privately, and when they finished, they returned to Zoom and let the experimenter know using the chat. Other aspects of Stage 2 differed depending on what condition participants were in, and these are described next.

Interaction Condition

After completing the Pre-measure Form, the experimenter moved on to the next set of instructions, where participants were told they would have five minutes to discuss the issue and try to reach an agreement on the statement together, meaning that they would need to agree on a number on the Likert scale. They were told that if they reached an agreement, they would report that point of agreement later; they were also told that if they did not reach an agreement, that was ok, too. The experimenter reminded the participants about the issue and the scale to be used on the chat.

The experimenter let participants know when they could turn their mics on and start talking. The experimenter's camera and mic were turned off while participants discussed the issue and during this time the experimenter did not share their screen. Participants were informed

when there was one minute left in the conversation. When time was up, the experimenter's camera and mic were turned on again, and the experimenter told the participants that the discussion was over.

The experimenter next shared their screen with instructions again. Participants were then given a link to the Post-manipulation Form in the chat. This online survey asked them whether they reached agreement or not and if they did, what number on the Likert scale represented their point of agreement.

For quality check purposes, while participants were having the conversation (or immediately right after, either while participants completed the Post-manipulation Form or right after the study session ended), the experimenter entered information related to what they observed in the conversation using an online form[2] (for more details, refer to the "Measures" section).

Imagine Condition

After both participants completed the Pre-measure Form, the experimenter presented each participant with the other participant's position on the moral issue (taken from the pre-screening survey) by sending separate private messages on the Zoom chat (i.e., "Participant A responded X" to Participant Z, and vice versa).

Then the experimenter moved on to the next set of instructions that appeared on screen. Participants were asked to imagine they had five minutes to discuss the issue with the other participant and to try to reach a point of agreement with the statement on the Likert scale. The experimenter asked the participants to imagine (separately) the conversation and sent them the

[2] This experimenter-coding form was added to the study about a month after data collection started; therefore we do not have answers on it for all the dyads that participated in the study.

link to complete the Post-manipulation Form. In this online survey, they answered the following questions: "Do you think you would be able to reach an agreement with this participant?" (Yes or No), and if they answered "Yes," then "What do you think the agreed upon number would be?" (number on the Likert-scale). Once the participant answered, they went to the next page in the survey where they were asked: "*When you imagine this conversation, how do you think it would go? Describe it in a couple sentences, in the time provided. (You have 5 minutes, please use the entire time to think and write your answer – when the time is up, the form will go to the next page automatically)*". The online survey had a timer showing how much time participants had left to write. In addition, they were unable to move to the previous or next page until five minutes had elapsed. When five minutes had passed, the survey automatically moved to the next page.

While participants completed the Post-manipulation Form, the experimenter entered information related to the study session using an online form[3] (for more details, refer to the "Measures" section).

Stage 3

In both conditions, when participants moved to the next page of the Post-manipulation Form, they answered a series of brief questionnaires aimed at interpreting their perceptions of the conversation and their conversation partner, as well as to measure how willing they would be to have a future conversation like the one they had just had (or had just imagined) in the future. (For a more detailed description, please refer to the "Measures" section below.) After these sets of questions, participants answered a series of demographic questions. Then they answered a set of questions they had previously answered on the Pre-measure Form again: their level of

[3] This experimenter-coding form was added to the study about a month after data collection started, therefore we do not have answers on it for all the dyads that participated in the study.

agreement with the moral issue statement, how sure they felt about their answer, how much they cared about the issue, and how informed they felt about the issue. Last, participants rated their level of agreement with a series of statements aimed at measuring Intellectual Humility (Leary et al., 2017). After completing the Post-manipulation Form, participants let the experimenter know in the chat and at this point, they were free to leave the session.

Stage 4

One week after the study session, participants received an email inviting them to fill out one last survey[4]. This survey asked them to answer the questions from the Pre-measure Form one last time: their level of agreement with the moral issue statement, how sure they felt about their opinion, how much they cared about the issue, and how informed they felt about it.

Measures

For detailed versions of the measures described below, refer to Appendix A.

Individual Opinion

This measure was used to establish a participant's position on the moral issue that was discussed, which would then determine whether they held an extreme ideological position or not. For Stage 1, in an online survey, participants were presented with a set of six moral issue statements on different topics: freedom of speech, vegetarianism, womb surrogacy, genetic editing, immortality, and sex work. For each statement they were asked to rate how much they agreed or disagreed using an 11-point Likert scale (0 = "Absolutely disagree", 5 = "Neither agree nor disagree", 10 = "Absolutely agree").

[4] Response rates for this questionnaire were low, and it did not bring new insights to the results from analyzing opinion change from pre- to post-manipulation forms (see Chapter 3). Nevertheless, a brief analysis of this data can be found in Appendix B.

These answers were used in our pre-screening to determine 1) whether two participants were at opposite extremes for at least one of the issues and 2) who their study partner would be (i.e., someone at the opposite extreme for one of the issues). Whichever issue met this criterion was the one both participants worked with throughout the study (if more than one issue met the criteria, one of those issues was picked at random). For a summary of how many participants worked with each issue for each condition, refer to Table 1 below. For the complete text for the statements of each issue, refer to Appendix A.

Table 1

Counts of participants per issue discussed in each condition.

Issue	Imagine Condition	Interaction Condition	Total
Freedom of Speech	20	23	43
Genetic Editing	7	8	15
Immortality	3	9	12
Womb Surrogacy	16	18	34
Vegetarianism	2	18	20
Sex Work	13	33	46
Total	61	109	170

During the study, we measured Individual Opinions again at Stages 2, 3 and 4. At these stages, participants answered four questions related to the moral issue statement they were assigned: how much they agreed with the statement ("Individual Opinion"); how sure they felt

about their answer ("Sure")[5]; how much they cared about the issue ("Care"); and how informed they felt about the issue ("Informed"). These four questions resembled those used in other studies to measure people's standpoints on moral or political topics before and after engaging in a dyadic or group discussion about it (Binnquist et al., 2022; Navajas et al., 2019).

Agreement

Depending on which condition they were assigned to, participants received one of the two following sets of instructions to complete the experimental manipulation.

Interaction Condition

"For this next task, you will have 5 minutes to discuss how much you two agree or disagree with the statement. Within that time, you should try to agree on an answer together, using the scale from 0 to 10. You must use the entire time to discuss your answer, you cannot finish early. When the 5 minutes are over, you must stop speaking. You will be notified when there is 1 minute left. If you are unable to agree on a number, that is ok too."

Imagine Condition

"For this next task, imagine that you have 5 minutes to discuss with this person about how much you two agree or disagree with the statement. Imagine that within that time you have to try to agree on an answer together, using the scale from 0 to 10. Imagine that when the 5 minutes are over, you cannot speak to each other anymore and that if you do not reach an agreement, that is ok too."

To measure whether participants reached (Interaction Condition) or expected to reach (Imagine Condition) an agreement or not, they individually answered a set of questions, depending on condition: whether they were able to, or thought they would be able to, reach an

[5] This measure, "Sure", was added about a month after data collection had started, therefore there is missing data on this measure from 22 participants on our final sample.

agreement or not ("Agreement"), and if they answered 'Yes', what number they agreed on or thought they would agree on ("Point of Agreement"). This way of measuring agreement (expected or real) followed the method used in previous similar studies (Navajas et al., 2018, 2019; Niella et al., 2021).

In the Imagine Condition, because participants did not spend five minutes having a conversation, they instead spent five minutes imagining the conversation and writing about it after they answered the previous two measures. The instructions they received about imagining this conversation were as follows:

"Imagining this conversation, how do you think it would go? Describe it in a couple of sentences in the time provided. (You have 5 minutes. Please use the full 5 minutes to think and write your answer. When the time is up, the form will go to the next page automatically)."

Experimenter assessment

While participants completed the Post-manipulation form, experimenters completed an online form to answer a few measures that assessed what happened during the study session. They were asked to report some basic information (dyad number, condition, etc.), plus to report if anything unusual might have disrupted the normal course of the experiment.

For the Interaction Condition, they also answered a series of questions that would assess how well participants followed the instructions for the conversation and whether it seemed like they reached an agreement or not. They answered: a) "How well did they followed the instructions?" on a 4-point Likert scale from 0 = "Not at all" to 3 = "They exchanged perspectives and tried to reach a number that represented an agreement between their two opinions", b) "Did it seem like they reached an agreement?" (Yes or No); if yes, c) "To what

extent do you perceive they genuinely arrived to the number they agreed on?" For each participant, answering with a 3-point Likert scale from 1= "Not at all" to 3= "Absolutely".

Interaction Quality

After having (Interaction Condition) or imagining (Imagine Condition) the conversation, we measured participants' assessment of the conversation and their conversation partner (imagined or real). To do this, we used a questionnaire based on that used by Binnquist et al. (2022) in their assessments of cross-ideological conversations. Binnquist et al. (2022) used two separate questionnaires: a "Forecast Assessment" questionnaire at the pre-study stage and an "Experience Assessment" questionnaire at the post-study stage because they used a within-subject design. In the present study, the assessment questionnaire items were the same for both conditions, except for minor wording changes to fit the Imagine or Interaction Conditions. There were 8 items for the assessment of the conversation (e.g., "I feel this interaction would be/was enjoyable") and 8 items for the assessment of the conversation partner (e.g., "I believe their ideas would be/were convincing"), and participants rated each item using a 7-point Likert scale from "Strongly disagree" to "Strongly agree". In the online form, the order of the items for each assessment set was randomized each time it was opened.

Willingness to engage in the future

Within the Interaction Quality questionnaires, two items measured people's willingness to engage in similar conversations in the future (i.e., "I would be/am willing to engage in more interactions like this") and how willing they were to talk with their conversation partner again (i.e., "I think I would be/am willing to talk with them again"). Again, participants rated each item using a 7-point Likert scale from "Strongly disagree" to "Strongly agree".

Intellectual Humility

We included a 6-item scale of Intellectual Humility developed by Leary et al. (2017), which has been demonstrated to be a valid measure of the degree to which people recognize that their beliefs are fallible. The six items (e.g., "I reconsider my opinions when presented with new evidence") were measured on a 7-point Likert scale from "Strongly disagree" to "Strongly agree". The order of the Intellectual Humility items was randomized each time the survey was opened.

Demographics

The following demographic questions were asked: Age, Gender, Religion, Highest level of education attained, Highest level of education attained by either of the participant's parents, Political Party, and Political Orientation (Right, Center-Right, Center-Left, Left, Unsure/Prefer not to say).

For Political Party identification, answer options were suited for Argentina. They included the two main parties, "Frente de Todos" (Left/Center-left) and "Juntos por el Cambio" (Center-right), plus "Prefer not to say," and "Other" (along with a box for open-ended responses). We also included two questions for participants to answer how much they identified with each one of the two main parties using an 11-point Likert scale (0 = Not at all; 10 = A lot).

Setup

Communications with participants (invitation to participate, scheduling the study session, reminders, follow-up email) were conducted via email. All the online forms used in the study (including the Informed Consent) were created in Qualtrics. We generated a random ID number for each participant that they had to report when completing each one of the study's forms so that they could stay anonymous.

The study session took place in a pre-scheduled Zoom meeting. The experimenter was the "Host" in the meeting and sent the links to the forms that participants needed to complete via chat to both participants. The Experimenter set the chat features so that the participants could only chat with the Host but not with each other, to avoid any communication between participants beyond the instructed ones. Participants were required to keep their cameras on and their mics off the entire time, except for when they were instructed to speak to each other if they were in the Interaction Condition. The experimenter gave instructions for the experiment orally, as well as posting the written instructions on screen, except for when the participants had a conversation in the Interaction Condition (in this case, the experimenter would stop screen-sharing during that period). The full text of the instructions and the slides can be found in Appendix A. Upon consent, these Zoom sessions were recorded and stored and may be used for future research purposes.

Data Analysis

To analyze the data obtained in this study, R code was used[6]. The 'lme4' package was used to run all mixed effects models (Bates et al., 2015). Main effects and marginal means were calculated using the 'emmeans' package (Lenth, 2023). Principal Components Analyses were computed using the 'prcomp' function from the 'stats' package in R (R Core Team, 2022). Bootstrapped linear mixed effects models were calculated using the 'lmeresampler' package (Loy et al., 2023), bootstrapped correlation coefficients were calculated using the 'boot' package (Canty & Ripley, 2022). Data simulation and power estimation for a mixed effects model was done using the 'mixedpower' package (Kumle et al., 2021), and power for Chi-square tests was estimated using the 'DescTools' package (Signorell, 2023).

[6] Data and R code to reproduce the results in the next chapter are available at bit.ly/diss-nie-cod

CHAPTER III: RESULTS

Data Quality Screening

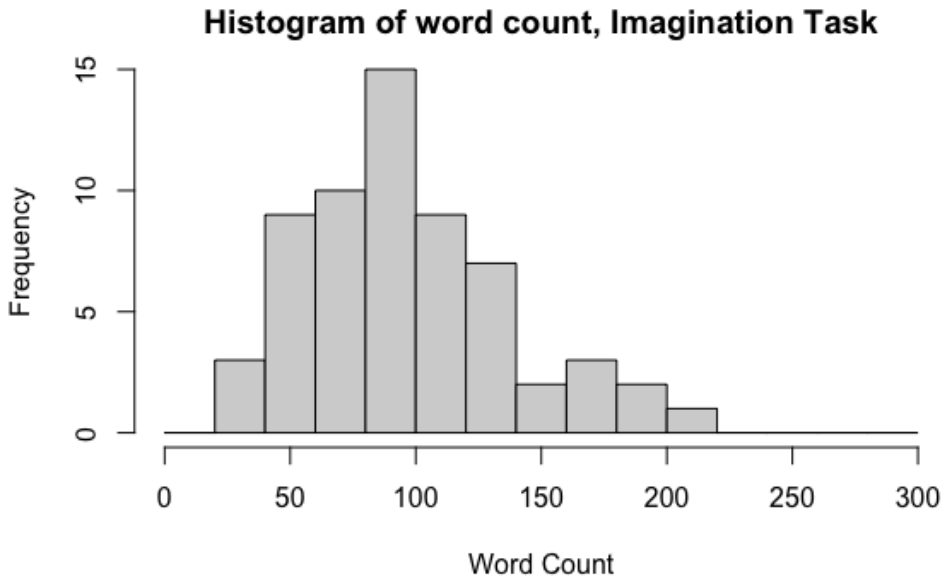
For data quality purposes, I first looked at the experimenter coding survey responses to analyze whether there was any problem with the execution of the manipulation (i.e., conversation task) in the Interaction Condition. Experimenters rated how well participants followed the instructions from 0 = "Not at all" to 3 = "They exchanged perspectives and tried to reach a number that represented an agreement between their two opinions", in a total of 52 dyads.

The vast majority of dyads (88.46%) were given the top score of 3. Only a handful (11.54%) were rated with a 2, and no dyads received a 1 or a 0 (Mean = 2.88, SD = 0.32, min = 2, max= 3). Moreover, when experimenters reported that the dyad seemed to have reached an agreement (69.23%), experimenters also coded how genuine the achieved agreement seemed to be for both participants, on a scale from 1 = "Not at all" to 3= "Absolutely". Again, no participant received a score below 2 (Mean = 2.81, SD= 0.40, min = 2, max = 3). All dyads were thus considered in the subsequent analyses.

For the Imagine Condition, to screen for data quality, I decided to look at how well participants followed the imagination task instructions ("*When you imagine this conversation, how do you think it would go? Describe it in a couple sentences, in the time provided")*. When looking at the word count of the text each participant wrote, I found that nobody wrote less than 27 words (Mean= 98.84, SD = 42.26, min = 27, max = 215), which may suggest everybody engaged with the task. A histogram of the word counts can be seen in Figure 1 below.

Figure 1

Histogram of word counts for the text produced by each participant in the Imagination task, in the Imagine Condition.



To check whether everyone stayed on task, a research assistant coded each text for how well it followed the task instructions using a scale from 0 to 3 (0 = "Did not write anything or what they wrote is incomprehensible"; 1 = " they wrote something but it is not related to the task, imagined conversation, conversation partner, and/or topic"; 2= "they wrote something that is related to the imagined conversation, conversation partner and/or topic, but it does not describe the imagined conversation with total clarity"; 3= "they clearly described how they imagined the conversation would go"). When analyzing the data from this coding work, I found that no participant received a score below 2 on their text; 24.59% received a 2; and 75.41% received a 3 (Mean = 2.75, SD= 0.43, min = 2, max = 3). Thus, again, all dyads were considered in the subsequent analyses.

Hypothesis 1: Agreement reported vs. agreement expected.

When looking at whether participants reached or expected to reach an agreement with their partner, we found that 77% of participants reported reaching an agreement when they had a conversation (Interaction Condition), and 72% reported expecting an agreement when they imagined it instead (Imagine Condition; see Table 2). The first pre-registered hypothesis compares the proportion of agreement reported (in the Interaction Condition) with the proportion of agreement expected (in the Imagine Condition), predicting the proportion to be higher among those who engaged in a conversation (Interaction Condition) than those who imagined it instead (Imagine Condition).

Table 2

Counts of answer to Agreement (Yes, No) grouped by Condition (Interaction, Imagine).

Agreement / Condition	**Interaction**	**Imagine**
No	25 (22.9%)	17 (27.9%)
Yes	84 (77.1%)	44 (72.1%)

To test this, I ran a Chi-Square Test for Independence comparing the counts of Agreement and Non-agreement in each condition (see Table 2). The test yielded $\chi^2 = 0.28$ (df = 1, $p = 0.596$, Cramer's V = 0.04), so there is a not a significant difference between the proportion of reported expected agreement in the Imagine Condition and the proportion of reported reached agreement in the Interaction Condition. This analysis was run with the full sample of 170 participants. A power analysis for a Chi-square test of proportions, based on the present sample size and obtained effect size, yields an estimated power of 0.08, which is very low, suggesting a

larger sample size is needed to make conclusions about whether a significant difference between conditions exists or not.

Additionally, I ran a similar analysis considering the answer to Agreement as a dyadic outcome (i.e., only one outcome per dyad) for the Interaction Condition, as the result from the conversation between the two participants in the dyad. In this case, for the Interaction Condition, I first filtered for dyads where both participants had provided the same answer to the question and only included these in the analyses – agreement or non-agreement was counted once for the dyad. A total of 15 dyads were filtered out because participants did not provide the same answer, or because I did not have enough information to conclude whether the dyad reached an agreement or not: 6 were 'full-dyads' (i.e., 12 participants) where each participant provided a different answer (e.g., Participant A answered 'Yes' and Participant Z answered 'No'), and 9 were 'half-dyads' (i.e., 9 participants), meaning that there was data for only one of the two participants in the dyad. In contrast, in the Imagine Condition all the participants' answers were counted, since each participant's answer accounted for one imaginary dyad. This resulted in 105 data points: 44 dyads in the Interaction Condition and 61 participants in the Imagine Condition. The counts for Agreement and Non-agreement in each condition can be found in Table 3.

Table 3

Counts of answer to Agreement (Yes, No) grouped by Condition (Interaction, Imagine). Agreement in the Interaction Condition is considered as one outcome per dyad.

Agreement / Condition	**Interaction**	**Imagine**
No	9 (20.4%)	17 (27.9%)
Yes	35 (79.5%)	44 (72.1%)

The test in this case yielded $\chi^2 = 0.409$ (df = 1, p = 0.522, Cramer's V = 0.062), so again there is a not a significant difference between the proportion of expected agreement among those who imagined the conversation and the proportion of dyads that reached agreement among those who did have the conversation. However, again, there is not enough power to determine whether the two conditions differed or not, because similar to the previous test, a power analysis for a Chi-square test of proportions, based on the present sample size and obtained effect size, yields an estimated power of 0.12.

Moreover, as an exploratory analysis using the full dataset (i.e., using all Interaction dyads regardless of answer-matching), I ran a Binary Logistic Mixed-effects Regression, which included dyad-level random effects to account for the nesting of observations within dyads. In this model, a single categorical predictor was used to compare the odds of agreement (reached or expected) in the Imagine vs. Interaction Condition.

I fit a logistic mixed model (estimated using ML and Nelder-Mead optimizer) with the sample of 170 participants to predict Agreement from Condition. The model included Dyad identification number as random effect for participants in all conditions. A summary of results can be found on Table 4. Within this model, participants in the Imagine Condition were the reference group. The effect of Condition is statistically non-significant. The 95% Confidence Intervals (CIs) and p-values were computed using a Wald z-distribution approximation.

Table 4

Summary of results for logistic mixed model to predict agreement from Condition. The model included dyad as random effect.

Predictors	*Odds Ratios*	*95% CI*	*p*
Intercept	4.39	1.41 – 13.63	0.010
Condition [Interaction]	1.76	0.49 – 6.36	0.390
Random Effects			
σ^2	3.29		
$\tau_{00\ dyad}$	3.35		
ICC	0.50		
Observations	170		
Marginal R^2 / Conditional R^2	0.011 / 0.510		

Based on the results of the logistic mixed model, I conclude that there is no statistically significant effect of Condition (whether participants had a conversation or imagined one) on the Agreement outcome (whether or not they reported reaching an agreement, real or imagined). For a more in-depth look at the measure of Agreement, histograms of the reported Point of Agreement for each Condition, and histograms of the difference between participants' initial opinion on the issue (Opinion Pre) and the reported Point of Agreement by Condition as well as by Gender, can be found on Appendix D.

Hypothesis 2: Qualitative aspects reported vs. imagined.

My second hypothesis was that participants would rate qualitative aspects of the conversation and their conversation partner (whether real or imagined) more favorably in the Interaction Condition (i.e., after having had a one-on-one conversation about a polarized issue) than in the Imagine Condition (i.e., after having imagined the conversation instead). To test this, I first used multiple multilevel models with each qualitative aspect's rating as the outcome variable, Condition (Interaction or Imagined) as the predictor and included dyad-level random effects to account for variability between dyads. Moreover, as per the pre-registered analysis, these models were also fitted with "Agreement" as an additional predictor to see whether agreement or non-agreement reported by the participant (whether real or imagined) influenced the assessment of the qualitative aspects, and whether it influenced the difference in ratings between conditions (i.e., an interaction effect).

The qualitative aspects of the conversation were classified as either positive or negative. The qualitative aspects of the *conversation partner* were all classified as positive, except for an item asking whether the conversation partner's arguments were "driven by emotion." A summary of the rated aspects, their classification, and item text can be found in Table 5. Participants rated each item on a 7-point Likert scale (1= "Strongly disagree" to 7= "Strongly agree").

Table 5

Summary of rated qualitative aspects, classification as positive or negative aspects, and the full item text as it appeared in the survey.

Assessment Focus	Classification	Aspect	Item in Survey
Conversation	**Positive**	Enjoyable	*I feel/felt this interaction would be/was enjoyable*
		Comfortable	*I would feel/felt comfortable in this interaction*
		Value	*I see value in engaging in interactions like this one*
	Negative	Stressful	*I feel/felt this interaction would be/was very stressful*
		Judged	*I would feel/ felt judged by the other participant*
		Difficult	*I feel/felt the interaction would be/was difficult to get through*
		Hard	*Reaching an agreement would be/was extremely hard to achieve*
Conversation Partner	**Positive**	Valid	*I believe their ideas would be/were valid*
		Convincing	*I believe their ideas would be/were convincing*
		Respect	*I think I could/can respect their opinions*
		Logic	*I believe their statements would be/were driven by logic*
		Like	*I think I would/can like them as a person*
		Impact	*I think their arguments would have/had an impact in my own opinion about the issue*
	Negative	Emotion	*I believe their statements would be/were driven by emotion*

Table 5 (continued)

Assessment Focus	Classification	Aspect	Item in Survey
Conversation Partner	**Positive**	Valid	*I believe their ideas would be/were valid*
		Convincing	*I believe their ideas would be/were convincing*
		Respect	*I think I could/can respect their opinions*
		Logic	*I believe their statements would be/were driven by logic*
		Like	*I think I would/can like them as a person*
		Impact	*I think their arguments would have/had an impact in my own opinion about the issue*
	Negative	Emotion	*I believe their statements would be/were driven by emotion*

A summary of results for the initial multilevel model comparing ratings between conditions for each aspect can be found in Table 6. A summary of results for the multilevel models including the individual answers to "Agreement" as an additional predictor can be found in Table 7, and the main effects for the predictors in these models are summarized in Table 8. The full final sample of 170 participants was used in these analyses. For the measures of 'Value,' 'Impact,' and 'Hard,' there was missing data for two participants (part of one 'full-dyad'); this was due to an error in the programming of the online form that was fixed immediately after and was not a fault of the participants.

Table 6

Summary of results of linear mixed models to predict each Aspect rating with Condition, and Dyad as a random effect.

	Enjoyable					Comfortable				
Coefficient	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*p*	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*p*
Intercept	4.98	-0.61	4.67 – 5.29	-0.84 – -0.39	**<0.001**	4.97	-0.54	4.62 – 5.31	-0.77 – -0.31	**<0.001**
Condition (Interaction)	1.31	0.95	0.92 – 1.70	0.67 – 1.24	**<0.001**	1.25	0.85	0.83 – 1.68	0.56 – 1.14	**<0.001**
Random Effects										
σ^2	1.50					1.84				
τ_{00}	0.00 $_{dyad}$					0.00 $_{dyad}$				
N	92 $_{dyad}$					92 $_{dyad}$				
Observations	170					170				
Marginal R^2 / Conditional R^2	0.210 / -					0.165 / -				

Table 6 (continued)

	Value					Valid				
Coefficient	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*
Intercept	6.14	-0.18	5.87 – 6.40	-0.44 – 0.07	**<0.001**	5.52	-0.38	5.25 – 5.80	-0.63 – -0.14	**<0.001**
Condition (Interaction)	0.30	0.28	-0.04 – 0.63	-0.04 – 0.60	0.082	0.68	0.60	0.33 – 1.02	0.29 – 0.90	**<0.001**
Random Effects										
σ^2	1.09					1.18				
τ_{00}	0.00 $_{dyad}$					0.00 $_{dyad}$				
N	91 $_{dyad}$					92 $_{dyad}$				
Observations	168					170				
Marginal R^2 / Conditional R^2	0.018 / -					0.082 / -				

Table 6 (continued)

	Convincing					Respect				
Coefficient	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*
Intercept	4.32	-0.41	3.92 – 4.71	-0.67 – -0.16	**<0.001**	6.12	-0.29	5.87 – 6.37	-0.55 – -0.02	**<0.001**
Condition (Interaction)	1.03	0.65	0.53 – 1.52	0.34 – 0.96	**<0.001**	0.43	0.45	0.12 – 0.74	0.12 – 0.78	**0.007**
Random Effects										
σ^2	2.06					0.74				
τ_{00}	0.21 $_{dyad}$					0.13 $_{dyad}$				
ICC	0.09					0.15				
N	92 $_{dyad}$					92 $_{dyad}$				
Observations	170					170				
Marginal R^2 / Conditional R^2	0.097 / 0.180					0.047 / 0.187				

Table 6 (continued)

	Logic					Like				
Coefficient	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*
Intercept	4.84	-0.27	4.48 – 5.19	-0.52 – -0.02	**<0.001**	4.97	-0.54	4.68 – 5.26	-0.78 – -0.31	**<0.001**
Condition (Interaction)	0.60	0.42	0.16 – 1.05	0.11 – 0.74	**0.008**	1.04	0.84	0.68 – 1.40	0.55 – 1.14	**<0.001**
Random Effects										
σ^2	1.94					1.24				
τ_{00}	0.01 $_{dyad}$					0.04 $_{dyad}$				
ICC	<0.01					0.03				
N	92 $_{dyad}$					92 $_{dyad}$				
Observations	170					170				
Marginal R^2 / Conditional R^2	0.042 / 0.045					0.164 / 0.189				

Table 6 (continued)

	Impact					Stressful				
Coefficient	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*
Intercept	4.42	-0.00	3.89 – 4.95	-0.28 – 0.27	**<0.001**	3.65	0.48	3.17 – 4.12	0.23 – 0.73	**<0.001**
Condition (Interaction)	0.01	0.00	-0.65 – 0.67	-0.34 – 0.35	0.982	-1.40	-0.75	-1.99 – -0.80	-1.06 – -0.43	**<0.001**
Random Effects										
σ^2	3.02					2.57				
τ_{00}	0.68 $_{dyad}$					0.50 $_{dyad}$				
ICC	0.18					0.16				
N	91 $_{dyad}$					92 $_{dyad}$				
Observations	168					170				
Marginal R^2 / Conditional R^2	0.000 / 0.183					0.128 / 0.271				

Table 6 (continued)

	Judged					Difficult				
Coefficient	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*
Intercept	2.57	0.40	2.22 – 2.93	0.16 – 0.64	**<0.001**	3.13	0.46	2.75 – 3.51	0.23 – 0.70	**<0.001**
Condition (Interaction)	-0.91	-0.62	-1.36 – -0.47	-0.92 – -0.32	**<0.001**	-1.16	-0.72	-1.63 – -0.68	-1.02 – -0.43	**<0.001**
Random Effects										
σ^2	1.98					2.27				
τ_{00}	0.00 $_{dyad}$					0.00 $_{dyad}$				
N	92 $_{dyad}$					92 $_{dyad}$				
Observations	170					170				
Marginal R^2 / Conditional R^2	0.089 / -					0.120 / -				

Table 6 (continued)

	Hard					Emotion				
Coefficient	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*
Intercept	3.81	0.34	3.25 – 4.38	0.06 – 0.62	**<0.001**	4.70	0.40	4.25 – 5.16	0.14 – 0.66	**<0.001**
Condition (Interaction)	-1.06	-0.53	-1.77 – -0.36	-0.88 – -0.18	**0.003**	-1.08	-0.63	-1.65 – -0.52	-0.96 – -0.30	**<0.001**
Random Effects										
σ^2	2.69					2.08				
τ_{00}	1.14 $_{dyad}$					0.59 $_{dyad}$				
ICC	0.30					0.22				
N	91 $_{dyad}$					92 $_{dyad}$				
Observations	168					170				
Marginal R^2 / Conditional R^2	0.063 / 0.341					0.093 / 0.292				

Table 7

Summary of results of linear mixed models for each Aspect predicted from Condition and Agreement, including interaction term and Dyad as random effect.

	Enjoyable					Comfortable				
Coefficient	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*p*	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*p*
Intercept	4.17	-1.21	3.60 – 4.74	-1.62 – -0.79	**<0.001**	3.94	-1.24	3.32 – 4.57	-1.66 – -0.81	**<0.001**
Condition (Interaction)	1.92	1.40	1.17 – 2.66	0.85 – 1.94	**<0.001**	2.18	1.47	1.37 – 2.99	0.92 – 2.02	**<0.001**
Agreement (Yes)	1.13	0.82	0.46 – 1.80	0.34 – 1.31	**0.001**	1.42	0.96	0.69 – 2.16	0.46 – 1.46	**<0.001**
Condition × Agreement	-0.86	-0.63	-1.72 – 0.00	-1.25 – 0.00	0.050	-1.29	-0.87	-2.24 – -0.35	-1.51 – -0.24	**0.008**
Random Effects										
σ^2	1.35					1.71				
τ_{00}	0.06 $_{dyad}$					0.00 $_{dyad}$				
ICC	0.04									
N	92 $_{dyad}$					92 $_{dyad}$				
Observations	170					170				
Marginal R^2 / Conditional R^2	0.261 / 0.293					0.231 / -				

Table 7 (continued)

	Value					Valid				
Coefficient	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*
Intercept	6.00	-0.31	5.50 – 6.50	-0.79 – 0.16	**<0.001**	5.06	-0.79	4.56 – 5.56	-1.24 – -0.35	**<0.001**
Condition (Interaction)	0.72	0.68	0.07 – 1.37	0.07 – 1.30	**0.030**	0.54	0.48	-0.11 – 1.19	-0.10 – 1.05	0.102
Agreement (Yes)	0.19	0.18	-0.40 – 0.78	-0.38 – 0.74	0.526	0.65	0.57	0.06 – 1.24	0.05 – 1.09	**0.032**
Condition × Agreement	-0.57	-0.54	-1.32 – 0.19	-1.26 – 0.18	0.142	0.14	0.12	-0.62 – 0.89	-0.55 – 0.79	0.724
Random Effects										
σ^2	1.09					1.10				
τ_{00}	0.00 $_{dyad}$					0.00 $_{dyad}$				
N	91 $_{dyad}$					92 $_{dyad}$				
Observations	168					170				
Marginal R^2 / Conditional R^2	0.034 / -					0.158 / -				

Table 7 (continued)

	Convincing					Respect				
Coefficient	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*
Intercept	4.06	-0.58	3.36 – 4.76	-1.02 – -0.13	**<0.001**	5.73	-0.70	5.29 – 6.17	-1.16 – -0.23	**<0.001**
Condition (Interaction)	0.46	0.29	-0.45 – 1.37	-0.29 – 0.87	0.320	0.52	0.55	-0.05 – 1.10	-0.06 – 1.15	0.075
Agreement (Yes)	0.35	0.22	-0.48 – 1.18	-0.30 – 0.74	0.407	0.54	0.56	0.03 – 1.04	0.03 – 1.10	**0.039**
Condition × Agreement	0.71	0.45	-0.35 – 1.77	-0.22 – 1.12	0.186	-0.15	-0.16	-0.81 – 0.51	-0.85 – 0.54	0.654
Random Effects										
σ^2	2.14					0.75				
τ_{00}	0.02 $_{dyad}$					0.08 $_{dyad}$				
ICC	0.01					0.09				
N	92 $_{dyad}$					92 $_{dyad}$				
Observations	170					170				
Marginal R^2 / Conditional R^2	0.150 / 0.157					0.088 / 0.175				

Table 7 (continued)

	Logic					Like				
Coefficient	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*
Intercept	4.94	-0.20	4.28 – 5.60	-0.66 – 0.27	**<0.001**	4.47	-0.95	3.93 – 5.00	-1.38 – -0.51	**<0.001**
Condition (Interaction)	-0.10	-0.07	-0.96 – 0.75	-0.67 – 0.53	0.816	1.21	0.98	0.52 – 1.91	0.42 – 1.55	**0.001**
Agreement (Yes)	-0.15	-0.10	-0.92 – 0.63	-0.65 – 0.44	0.712	0.69	0.56	0.07 – 1.32	0.06 – 1.07	**0.030**
Condition × Agreement	0.92	0.65	-0.07 – 1.92	-0.05 – 1.35	0.068	-0.27	-0.22	-1.07 – 0.54	-0.87 – 0.44	0.513
Random Effects										
σ^2	1.90					1.19				
τ_{00}	0.00 $_{dyad}$					0.04 $_{dyad}$				
ICC						0.04				
N	92 $_{dyad}$					92 $_{dyad}$				
Observations	170					170				
Marginal R^2 / Conditional R^2	0.076 / -					0.198 / 0.227				

Table 7 (continued)

	Impact					Stressful				
Coefficient	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*
Intercept	3.65	-0.41	2.75 – 4.56	-0.88 – 0.07	**<0.001**	4.37	0.87	3.54 – 5.21	0.42 – 1.31	**<0.001**
Condition (Interaction)	0.10	0.05	-1.09 – 1.29	-0.57 – 0.67	0.870	-1.81	-0.97	-2.92 – -0.70	-1.56 – -0.38	**0.001**
Agreement (Yes)	1.08	0.56	0.03 – 2.13	0.02 – 1.11	**0.043**	-1.01	-0.54	-1.96 – -0.06	-1.05 – -0.03	**0.038**
Condition × Agreement	-0.20	-0.11	-1.57 – 1.16	-0.82 – 0.61	0.768	0.60	0.32	-0.66 – 1.86	-0.35 – 0.99	0.347
Random Effects										
σ^2	3.10					2.47				
τ_{00}	0.44 $_{dyad}$					0.56 $_{dyad}$				
ICC	0.12					0.18				
N	91 $_{dyad}$					92 $_{dyad}$				
Observations	168					170				
Marginal R^2 / Conditional R^2	0.047 / 0.165					0.152 / 0.308				

Table 7 (continued)

	Judged					Difficult				
Coefficient	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*
Intercept	2.59	0.41	1.91 – 3.26	-0.05 – 0.87	**<0.001**	4.00	1.01	3.31 – 4.69	0.57 – 1.44	**<0.001**
Condition (Interaction)	-0.63	-0.43	-1.50 – 0.25	-1.02 – 0.17	0.159	-1.32	-0.82	-2.22 – -0.42	-1.38 – -0.26	**0.004**
Agreement (Yes)	-0.02	-0.01	-0.82 – 0.78	-0.55 – 0.53	0.960	-1.20	-0.75	-2.02 – -0.39	-1.26 – -0.24	**0.004**
Condition × Agreement	-0.37	-0.25	-1.39 – 0.65	-0.94 – 0.44	0.476	0.29	0.18	-0.76 – 1.33	-0.47 – 0.83	0.589
Random Effects										
σ^2	1.99					2.10				
τ_{00}	0.00 $_{dyad}$					0.00 $_{dyad}$				
N	92 $_{dyad}$					92 $_{dyad}$				
Observations	170					170				
Marginal R^2 / Conditional R^2	0.096 / -					0.196 / -				

Table 7 (continued)

	Hard					Emotion				
Coefficient	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*
Intercept	4.86	0.86	4.01 – 5.70	0.44 – 1.28	**<0.001**	4.33	0.18	3.55 – 5.10	-0.27 – 0.63	**<0.001**
Condition (Interaction)	-0.43	-0.21	-1.55 – 0.68	-0.77 – 0.34	0.446	-0.12	-0.07	-1.15 – 0.91	-0.67 – 0.53	0.821
Agreement (Yes)	-1.47	-0.73	-2.45 – -0.48	-1.22 – -0.24	**0.004**	0.53	0.31	-0.35 – 1.41	-0.20 – 0.82	0.236
Condition × Agreement	-0.70	-0.35	-1.98 – 0.58	-0.98 – 0.29	0.281	-1.29	-0.75	-2.45 – -0.13	-1.43 – -0.07	**0.030**
Random Effects										
σ^2	2.73					2.07				
τ_{00}	0.38 $_{dyad}$					0.54 $_{dyad}$				
ICC	0.12					0.21				
N	91 $_{dyad}$					92 $_{dyad}$				
Observations	168					170				
Marginal R^2 / Conditional R^2	0.235 / 0.330					0.121 / 0.303				

Table 8

Summary of main effects of each predictor for each item in the model with Condition, Agreement, and the interaction of the two as predictors.

Aspect	Contrast	Estimate	SE	df	*t*	*p*	Predictor
Enjoyable	Interaction - Imagine	1.487	0.221	101.1	6.713	**<0.001**	Condition
	Yes - No	0.702	0.220	162.8	3.193	**0.002**	Agree
Comfortable	Interaction - Imagine	1.533	0.239	100.7	6.402	**<0.001**	Condition
	Yes - No	0.776	0.241	161.4	3.220	**0.002**	Agree
Value	Interaction - Imagine	0.437	0.192	98.6	2.280	**0.025**	Condition
	Yes - No	-0.092	0.193	160.0	-0.477	0.634	Agree
Valid	Interaction - Imagine	0.609	0.192	100.7	3.175	**0.002**	Condition
	Yes - No	0.713	0.193	161.4	3.694	**<0.001**	Agree
Convincing	Interaction - Imagine	0.818	0.270	100.8	3.029	**0.003**	Condition
	Yes - No	0.705	0.271	161.7	2.599	**0.010**	Agree
Respect	Interaction - Imagine	0.445	0.173	101.5	2.577	**0.011**	Condition
	Yes - No	0.460	0.169	164.0	2.725	**0.007**	Agree
Logic	Interaction - Imagine	0.361	0.252	100.7	1.431	0.156	Condition
	Yes - No	0.317	0.254	161.4	1.246	0.215	Agree
Like	Interaction - Imagine	1.079	0.207	101.0	5.222	**<0.001**	Condition
	Yes - No	0.560	0.206	162.5	2.720	**0.007**	Agree

Table 8 (continued)

Aspect	Contrast	Estimate	SE	df	*t*	*p*	Predictor
Impact	Interaction - Imagine	-0.003	0.361	99.6	-0.008	0.993	Condition
	Yes - No	0.979	0.349	162.9	2.803	**0.006**	Agree
Stressful	Interaction - Imagine	-1.509	0.339	102.1	-4.451	**<0.001**	Condition
	Yes - No	-0.708	0.321	165.5	-2.204	**0.029**	Agree
Judged	Interaction - Imagine	-0.812	0.258	100.7	-3.144	**0.002**	Condition
	Yes - No	-0.204	0.260	161.4	-0.785	0.434	Agree
Difficult	Interaction - Imagine	-1.177	0.265	100.7	-4.438	**<0.001**	Condition
	Yes - No	-1.061	0.267	161.4	-3.975	**<0.001**	Agree
Hard	Interaction - Imagine	-0.782	0.339	99.6	-2.308	**0.023**	Condition
	Yes - No	-1.819	0.327	162.9	-5.553	**<0.001**	Agree
Emotion	Interaction - Imagine	-0.763	0.317	102.3	-2.408	**0.018**	Condition
	Yes - No	-0.116	0.298	165.8	-0.389	0.697	Agree

Our first set of results (Table 6) shows that people who had a conversation evaluated it (and the person they had the conversation with) more positively and less negatively than those who imagined the conversation instead. However, those who had the conversation did not find their conversation partner's arguments to have more of an impact on their own opinion than the impact expected by those who imagined the conversation. In terms of how valuable participants found the conversation to be, the results did not differ by condition.

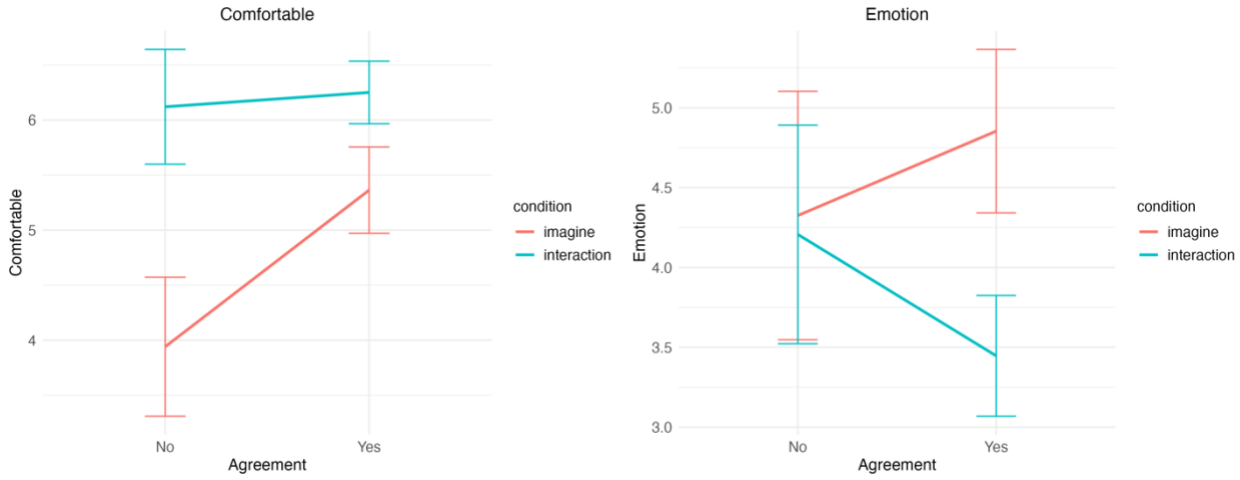
When controlling for whether participants reported or expected reaching an agreement or not, this set of models yielded two significant interaction effects. As can be seen in Figure 2, when participants had a real conversation (Interaction Condition), whether agreement was reached or not had less of an effect on how comfortable they found the conversation than when participants imagined the conversation (Imagine Condition). On the other hand, how emotion-driven they found their conversation partner's arguments was only significantly higher in the Imagine Condition than in the Interaction Condition when agreement was reached or expected, but the difference between conditions was not significant when disagreement was reported or expected.

Moreover, in this second set of models (adding Agreement and the interaction term as predictors), we can see by the main effects in Table 8 that for most aspects, when people had a conversation, they found it and their conversation partner to be more positive and less negative than participants in the Imagine Condition imagined them to be. For most aspects, participants also found the conversation and their partners to be more positive and less negative when an agreement was reported or expected than when it was not. In terms of how logic-driven or impactful they found their partner's arguments to be, there were no significant effects of the conversation condition. Moreover, whether participants reported or expected agreement or not

did not significantly affect how much value they saw in these conversations or how logic-driven they found their partner's arguments to be.

Figure 2

Plot illustrating estimated marginal means of Comfortable and Emotion ratings for each combination of levels in Condition and Agreement.



To have a clearer interpretation of these results, I ran Principal Components Analyses (PCAs) on the Positive and Negative aspects to examine whether grouping some or all of these aspects into fewer variables was appropriate. First, I standardized all variables and looked at the correlation matrixes for the Positive aspects and for the Negative aspects separately (Figure 3).

For both groups of aspects, I decided to keep all the aspects in the PCA. This included putting 'Emotion' in with the other Negative aspects, even though it did not show high correlations with any of the other Negative aspects. Results of the PCA did not vary with or without the "Emotion" aspect. Based on the results shown by the scree plots from both PCAs in Figure 4, I retained two components from the Positive aspects analysis (Eigen value > 1) and only one from the Negative aspects analysis (Eigen value >1). The loadings of each aspect in the

retained components, together with a summary of results can be found in Table 9 for the Positive aspects, and in Table 10 for the Negative aspects.

Figure 3

Correlation matrixes for Positive aspects (left) and Negative aspects (right).

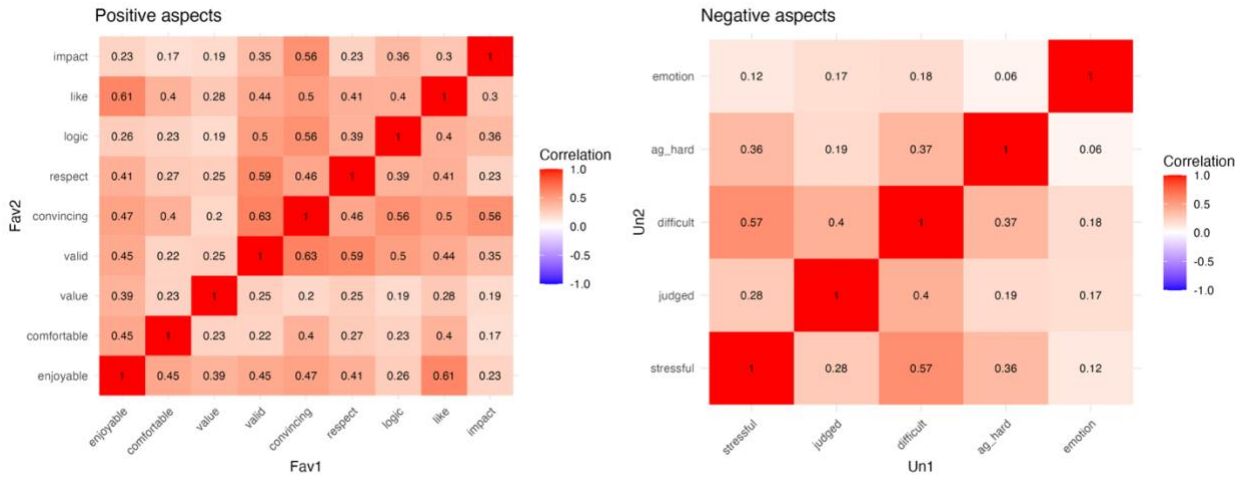


Figure 4

Scree plots of principal components from the PCAs of Positive (left) and Negative (right) aspects.

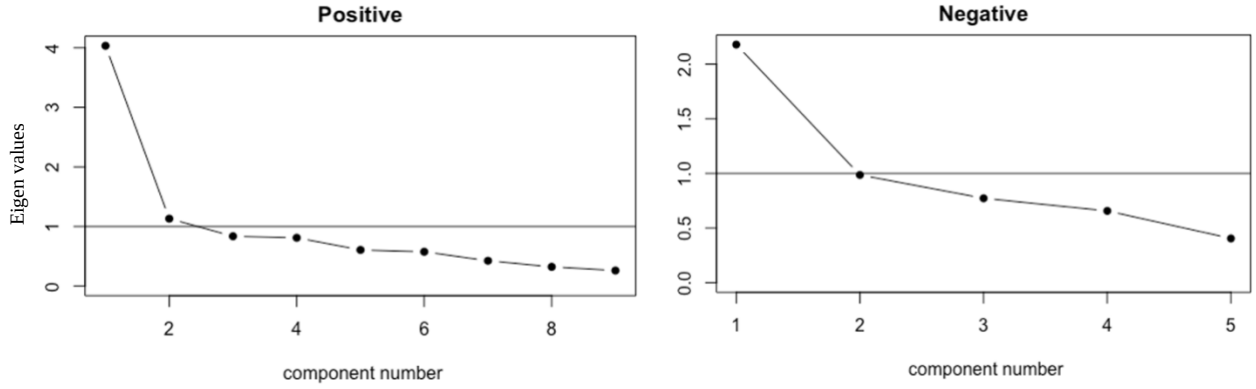


Table 9

Summary of PCA on Positive Aspects using varimax-rotation. Highlighted are the aspects with an absolute loading above .5.

PCA – Positive Aspects

	Component 1	**Component 2**
Enjoyable	-0.26	**0.81**
Comfortable	-0.13	**0.69**
Value	-0.06	**0.64**
Valid	**-0.74**	0.30
Convincing	**-0.80**	0.32
Respect	**-0.56**	0.40
Logic	**-0.76**	0.13
Like	-0.42	**0.65**
Impact	**-0.70**	0.03
SD	2.01	1.06
Proportion of Variance	0.45	0.13
Cumulative Proportion	0.45	0.57

Table 10

Summary of PCA on Negative Aspects. Highlighted are the aspects with an absolute loading above .5.

PCA – Negative Aspects

	Component 1
Stressful	**0.53**
Judged	0.42
Difficult	**0.57**
Hard	0.42
Emotion	0.22
SD	1.48
Proportion of Variance	0.44
Cumulative Proportion	0.44

Given the loadings for each aspect on each component of the PCA for Positive aspects, I concluded that two new variables could be created to aggregate the nine Positive aspects into two variables: “Good Arguments” which was computed as the average of the Valid, Convincing, Respect, Logic, and Impact aspects, and “Good Experience” which was computed as the average of the Enjoyable, Comfortable, Value, and Like aspects. Given the loadings of the aspects in the PCA for the Negative aspects, I concluded that two of the five items could be reduced to one variable: “Negative Experience” which was computed as the average of the Stressful and Difficult items. A summary with details of these new variables can be found in Table 11.

Table 11

Description of new variables computed from the Positive and Negative aspects as a result of Principal Components Analyses

New Variable	Average of	Item in Survey
Good Arguments	Valid	*I believe their ideas would be/were valid*
	Convincing	*I believe their ideas would be/were convincing*
	Respect	*I think I could/can respect their opinions*
	Logic	*I believe their statements would be/were driven by logic*
	Impact	*I think their arguments would have/had an impact in my own opinion about the issue*
Good experience	Enjoyable	*I feel/felt this interaction would be/was enjoyable*
	Comfortable	*I would feel/felt comfortable in this interaction*
	Value	*I see value in engaging in interactions like this one*
	Like	*I think I would/can like them as a person*
Unpleasant experience	Stressful	*I feel/felt this interaction would be/was very stressful*
	Difficult	*I feel/felt the interaction would be/was difficult to get through*

With this new reduced set of variables, I ran the second multilevel model that included Condition, Agreement, and their interaction as predictors, accounting for dyad-level random effects using each one of these three new variables as dependent variables. A summary of these results can be found in Table 12, and a summary of the main effects in Table 13.

Table 12

Summary of results of linear mixed models for Good Arguments, Good Experience, and Unpleasant Experience, with Condition, Agreement, and interaction term as predictors, with Dyad as random effect.

	Good Arguments					Good Experience				
Coefficient	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*
Intercept	4.68	-0.67	4.21 – 5.16	-1.12 – -0.22	**<0.001**	4.65	-1.30	4.26 – 5.04	-1.71 – -0.90	**<0.001**
Condition (Interaction)	0.29	0.28	-0.32 – 0.91	-0.30 – 0.86	0.348	1.50	1.57	1.00 – 2.01	1.04 – 2.09	**<0.001**
Agreement (Yes)	0.49	0.46	-0.07 – 1.05	-0.07 – 0.99	0.089	0.86	0.90	0.40 – 1.32	0.42 – 1.38	**<0.001**
Condition × Agreement	0.31	0.29	-0.41 – 1.03	-0.38 – 0.97	0.395	-0.74	-0.78	-1.33 – -0.16	-1.39 – -0.16	**0.013**
Random Effects										
σ^2	0.98					0.66				
τ_{00}	<0.01 $_{dyad}$					0.00 $_{dyad}$				
ICC	<0.01									
N	91 $_{dyad}$					91 $_{dyad}$				
Observations	168					168				
Marginal R^2 / Conditional R^2	0.142 / 0.144					0.295 / -				

Table 12 (continued)

	Unpleasant Experience				
Coefficient	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*P-Value*
Intercept	4.18	1.04	3.51 – 4.84	0.61 – 1.47	**<0.001**
Condition (Interaction)	-1.57	-1.01	-2.44 – -0.70	-1.58 – -0.45	**0.001**
Agreement (Yes)	-1.09	-0.71	-1.86 – -0.32	-1.21 – -0.21	**0.006**
Condition × Agreement	0.45	0.29	-0.55 – 1.45	-0.36 – 0.94	0.380
Random Effects					
σ^2	1.72				
$\tau_{00\ dyad}$	0.19				
ICC	0.10				
$N_{\ dyad}$	92				
Observations	170				
Marginal R^2 / Conditional R^2	0.211 / 0.289				

Table 13

Summary of main effects of each predictor for Good Arguments, Good Experience, and Unpleasant Experience in the model with Condition, Agreement, and the interaction term as predictors.

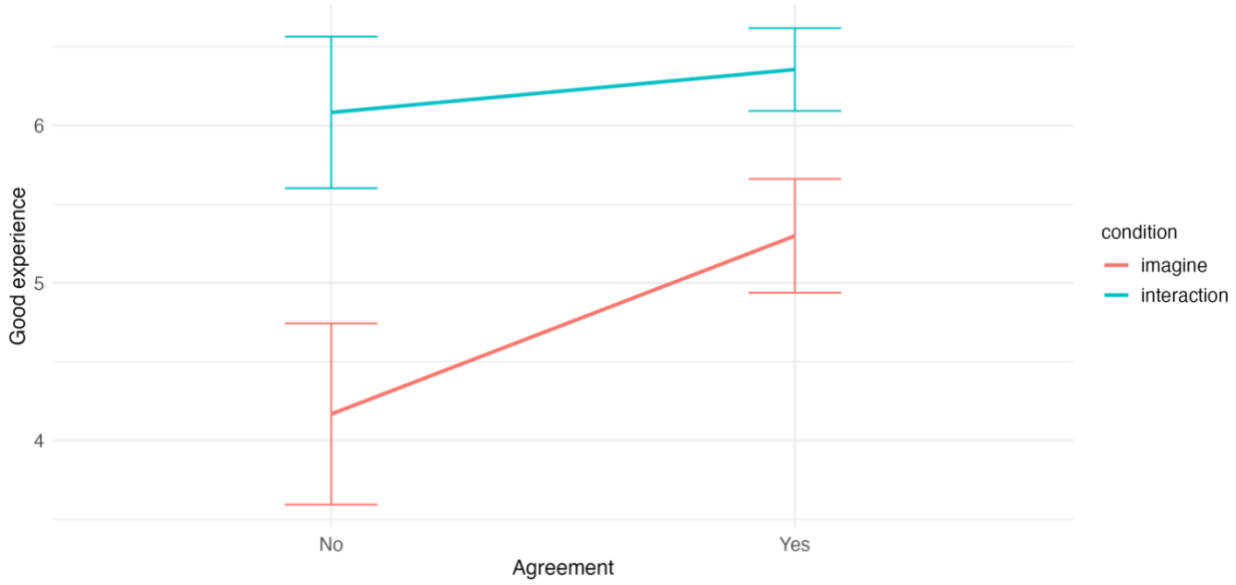
Variable	Contrast	Estimate	SE	df	*t*	*p*	Predictor
Good Arguments	Interaction - Imagine	0.449	0.183	98.6	2.46	**0.016**	Condition
	Yes - No	0.644	0.184	160.1	3.50	**0.001**	Agree
Good Experience	Interaction - Imagine	1.131	0.149	98.6	7.59	**<0.001**	Condition
	Yes - No	0.487	0.150	160.0	3.25	**0.001**	Agree
Unpleasant Experience	Interaction - Imagine	-1.343	0.262	101.5	-5.12	**<0.001**	Condition
	Yes - No	-0.871	0.256	164.1	-3.40	**0.001**	Agree

The summary of the main effects (see Table 13) suggests that when people had an actual conversation, they found it to be a better experience than what was expected by those who imagined having a conversation. Moreover, those who had the conversation found their conversation partner's arguments to be better than those who imagined their partner's arguments. And finally, those who had the conversation found it to be less of an unpleasant experience than those who imagined having a conversation. When agreement was reported or expected, the conversation and the partner's arguments were found to be better and the experience was reported to be

less unpleasant than when no agreement was reported or expected. Furthermore, the model yielded a significant interaction effect, suggesting that when people had a real conversation, reaching an agreement or not was less related to how good of an experience they found the conversation to be than when people imagined the conversation (in the latter case, they expected it to be less of a good experience if they did not expect an agreement than when they did; see Figure 5).

Figure 5

Plot illustrating estimated marginal means of Good Experience for each combination of levels in Condition and Agreement.



In sum, this collection of results suggests that people who were induced to have a short cross-ideological conversation, as opposed to imagining one, found the experience to be more favorable and less unpleasant than other participants imagined, as well as finding their conversation partners to be better interlocutors than other participants imagined. Moreover, failing to find agreement on the issue influenced people's assessment of the conversation less

than if they actually engaged in one. The prospect of not reaching an agreement more negatively affected evaluations of participants who merely imagined a conversation.

I ran a power analysis on a simulated data set to estimate the power of each fixed effect in the simulated model with Good Experience as the dependent variable. The power for this sample size was estimated to be .75 for the interaction term effect, .93 for the fixed effect of Agreement, and .99 for the fixed effect of Condition.

Hypothesis 3: Willingness to engage in the future.

The third pre-registered hypothesis in this study predicted that participants would be more willing to engage in such conversations in the future, including talking to their conversation partner again, in the Interaction Condition (i.e., after having had a conversation) than in the Imagine Condition (i.e., after having imagined a conversation instead).

To test this, multilevel models were run using each Willingness item rating as the outcome variable, Condition (whether participants had a conversation or imagined one) as a predictor, and including dyad-level random effects to account for variability between dyads. Moreover, as per the pre-registered analysis, these models also included Agreement as a predictor to see whether reported agreement or non-agreement (real or imagined) influenced the Willingness ratings and whether it influenced the difference in ratings between conditions (i.e., an interaction between predictors).

Willingness to engage in these conversations in the future was measured with two items: one was part of the conversation assessment questionnaire, and the other was part of the conversation partner assessment questionnaire. Participants rated each item on a 7-point Likert scale (1= “Strongly disagree” to 7= “Strongly agree”). A summary of these items can be found in Table 14. These two items had a correlation of 0.69 (CI 95% = [0.57, 0.78]). A summary of

results for the initial multilevel model comparing ratings between conditions for each aspect can be found in Table 15. A summary of results for the multilevel models, including agreement as a predictor can be found in Table 16, and a summary of the main effects can be found in Table 17. In all models, the intraclass correlation coefficient (ICC) was calculated to be 0.00, suggesting that none of the total variation in the dependent variable can be attributed to differences between dyads. Although the ICC is very low, we have included dyadic-level random effects in the model to account for the nested data structure and potential dependencies within dyads. The full final sample of 170 participants was used in these analyses. However, these two measures had missing data from two participants (part of one 'full-dyad'); this was due to an error in the programming of the online form that was fixed immediately after and not a fault of the participants.

Table 14

Summary of rated willingness to engage in the future and the full item texts.

Assessment	Aspect	Item in Survey
Conversation	Future	*I would be/am willing to engage in more interactions like this*
Conversation Partner	Talk Again	*I think I would be/am willing to talk with them again*

Table 15

Summary of results of linear mixed models for each Willingness Aspect, to predict Aspect rating with Condition, and Dyad as a random effect.

Aspect	*Marginal* R^2	ICC	Predictor	Estimate β	95% CI	*p*
Future	0.08	0.00	Intercept	5.95	5.68 – 6.22	**<0.001**
			Condition [Interaction]	0.62	0.29 – 0.95	**<0.001**
Talk Again	0.15	0.00	Intercept	5.71	5.48 – 5.94	**<0.001**
			Condition [Interaction]	0.78	0.50 – 1.07	**<0.001**

Table 16

Summary of results of linear mixed models for each Aspect to predict Willingness Aspect with Condition, Agreement, and the interaction term, with Dyad as random effect.

Aspect	*Marginal* R^2	ICC	Predictor	Estimate β	95% CI	*P*
Future	0.09	0.00	Intercept	5.59	5.09 – 6.08	**<0.001**
			Condition [Interaction]	1.09	0.45 – 1.73	**0.001**
			Agree [Yes]	0.51	-0.08 – 1.10	0.091
			Condition x Agree	-0.65	-1.40 – 0.10	0.088
Talk Again	0.17	0.00	Intercept	5.53	5.10 – 5.95	**<0.001**
			Condition [Interaction]	1.19	0.64 – 1.74	**<0.001**
			Agree [Yes]	0.26	-0.25 – 0.76	0.316
			Condition x Agree	-0.55	-1.19 – 0.09	0.094

Table 17

Summary of main effects of each predictor for Future and Talk Again in the model with Condition, Agreement, and the interaction term as predictors.

Model	Contrast	Estimate	SE	df	*t*	*p*	Predictor
Future	Interaction - Imagine	0.766	0.191	98.6	4.020	**<.001**	Condition
	Yes - No	0.181	0.192	160.0	0.945	0.346	Agreement
Talk Again	Interaction - Imagine	0.917	0.163	98.6	5.622	**<.001**	Condition
	Yes - No	-0.018	0.164	160.0	-0.107	0.915	Agreement

As can be seen in the results, having engaged in conversation, compared to imagining a conversation, significantly increased participants' reported willingness to engage in such conversations in the future as well as their willingness to talk to their conversation partner again.

In sum, these results suggest that having experienced a short cross-ideological conversation improves people's willingness to engage in future encounters.

Hypothesis 4: Opinion change

The fourth hypothesis predicted that participants' opinions on the moral topic would change more (in any direction) after having had the cross-ideological conversation (i.e., Interaction Condition) than after imagining such conversation (i.e., Imagine Condition).

To test this, a variable measuring the change in opinion for each participant was created by taking the absolute value of the difference between their level of agreement with the moral statement in the Pre-Measure form ("Individual Opinion Pre") and their answer to Individual Agreement in the Post-Manipulation form ("Individual Opinion Post"). This difference score was labeled "Opinion Change."

It is important to note that the distribution of change scores is very skewed to the right, indicating that the big majority of participants did not change their opinion after the manipulation or changed by only 1 point (a small portion changed by more than 2 points, and only a handful by 3 points or more; see Figure 6 below).

I ran a linear mixed model to predict Opinion Change from Condition and the initial individual opinion point (Opinion Pre) and accounting for Dyad random effects. A summary of the results can be found in Table 18. To solve for the non-normality of the dependent variable, I

also computed a bootstrapped model (1000 iterations) to compare results (see Table 19). The full final sample of 170 participants was used in these analyses.

Figure 6

Histogram of Opinion Change

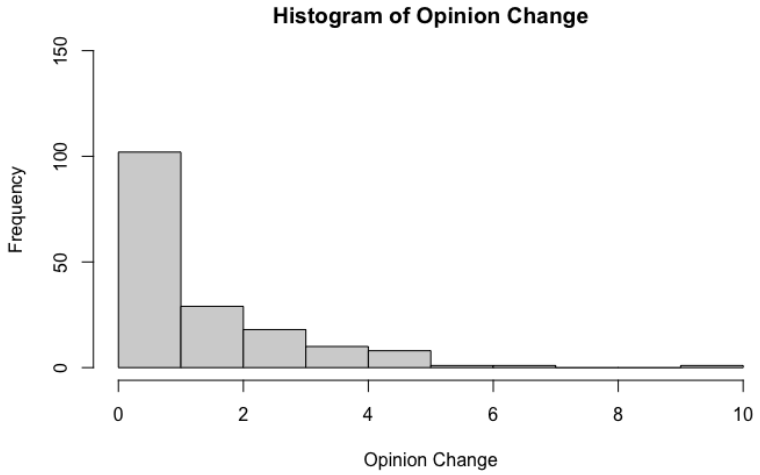


Table 18

Summary of results of a linear mixed model to predict Opinion Change with Condition and Opinion Pre, and Dyad as random effect.

	Opinion Change		
Coefficient	*Estimates*	*Conf. Int (95%)*	*P-Value*
Intercept	0.14	-0.11 – 0.40	0.260
Condition (Interaction)	-0.23	-0.54 – 0.08	0.149
Opinion Pre	0.16	-0.09 – 0.42	0.211
Condition × Opinion Pre	-0.26	-0.58 – 0.06	0.107
Random Effects			
σ^2	0.99		
$\tau_{00\ dyad}$	0.00		
$N_{\ dyad}$	92		
Observations	170		
Marginal R^2 / Conditional R^2	0.028 / -		

Table 19

Summary of bootstrapped results of a linear mixed model to predict Opinion Change with Condition and Opinion Pre, and Dyad as random effect

Coefficient	Original	Bootstrap Estimate	Standard Error	Bias	CI 95%
Intercept	0.144	0.153	0.123	0.008	(-0.104, 0.376)
Condition (Interaction)	-0.231	-0.232	0.161	-0.002	(-0.544, 0.086)
Opinion Pre	0.164	0.165	0.135	0.001	(-0.102, 0.428)
Condition × Opinion Pre	-0.261	-0.263	0.162	-0.002	(-0.577, 0.058)

What this initial set of results suggests (see Tables 18 and 19) is that how much a participant's individual opinion on the topic changed was not influenced by whether participants had a conversation or just imagined having one. Moreover, the participants' initial individual opinions on the topic did not influence how much their opinion changed after the conversation or imagination task.

As noted in the pre-registration, I wanted to explore how other measures could influence people's change in opinion, including variables from the Quality Assessment and Willingness questionnaires (such as how comfortable participants found the conversation or how they thought it would be, how willing they were to engage in conversations like this in the future, etc.), Opinion Pre (their initial point of agreement with the topic), plus the other measures of the Individual Opinion Pre questionnaire (how sure they felt about their initial opinion, how much they cared about the topic, how informed they felt about the topic). For the Quality Assessment measures, I used the reduced variables from the PCA used to test Hypothesis 2: Good Arguments, Good Experience, and Unpleasant Experience. To run this exploratory analysis, I computed the correlations of these variables with Opinion Change. I also ran a bootstrap of 1000

repetitions to obtain an estimated bootstrapped correlation for each variable. A summary can be found in Table 20.

Table 20

Bootstrapped Correlation Coefficients of Opinion Pre, Informed Pre, Sure Pre, Care Pre, Future, Talk Again, Good Arguments, Good Experience, and Unpleasant Experience with Opinion Change as single target variable.

Variable	Original	Bias	SE	Bootstrap Estimate	CI 95%
Opinion Pre	-0.036	-0.002	0.091	-0.038	(-0.213, 0.141)
Informed Pre	-0.213	-0.001	0.082	**-0.214**	(-0.391, -0.055)
Sure Pre	-0.141	< -0.001	0.084	-0.142	(-0.294, 0.032)
Care Pre	-0.155	0.001	0.080	-0.153	(-0.315, -0.014)
Future	-0.283	0.011	0.135	**-0.272**	(-0.569, -0.057)
Talk Again	-0.132	0.003	0.105	-0.129	(-0.346, 0.064)
Good Arguments	0.103	0.001	0.086	0.103	(-0.066, 0.277)
Good Experience	-0.134	0.005	0.101	-0.130	(-0.334, 0.061)
Unpleasant Experience	0.105	0.001	0.086	0.106	(-0.074, 0.272)

These results show that how informed participants initially reported feeling (Informed Pre) and how willing to engage in more conversations in the future they reported to be (Future) had the strongest correlations with how much their individual opinion changed. There is a negative correlation between change in opinion and how informed participants reported being on the topic, suggesting that the more informed they were, the less their opinions changed after having or imagining a cross-ideological conversation. Interestingly, this is also the case for the

correlation between Future and Opinion Change: the higher participants rated their willingness to engage in conversations like this in the future, the less their opinion changed.

For exploratory purposes, Individual Opinion was also measured a week after the experiment (with a variable named "Individual Opinion Long"). The final response rate for this measure was low (<50%) and the results obtained from analyzing the data did not bring further insights from what was found in Opinion Change immediately after the manipulation. A summary of the results from the analysis of Opinion Change Long (a week after the experiment session) can be found in Appendix B.

Finally, exploratory analyses were run on change (from pre- to post-manipulation) of the other aspects of Individual Opinion: Sure, Care, and Informed. No significant results were found. A summary of the analyses and results can be found in Appendix C.

Exploratory Analysis: Intellectual Humility

As an exploratory analysis, I investigated how the Intellectual Humility Scale (Leary et al., 2017) correlated with the Quality (how positively or negatively participants rated the conversation and conversation partners, real or imagined) and Willingness (how willing to engage in future interactions participants reported to be) assessment measures, as well as with Opinion Change (how much their opinion changed after the conversation, real or imagined). For this, I computed correlation coefficients for each measure with the Intellectual Humility scores, which were computed as the average of the items' responses (Cronbach's alpha = 0.76, CI 95% = [0.67, 0.83]), where higher values indicate greater Intellectual Humility. A summary of the coefficients can be found in Table 21. The full final sample of 170 participants was used in these analyses.

Table 21

Pearson correlation coefficients for Intellectual Humility score and Good Arguments, Good

Variable	Correlation with Intellectual Humility	CI 95%	*p*
Good Arguments	0.290	(0.144, 0.423)	**<.001**
Good Experience	0.331	(0.189, 0.460)	**<.001**
Unpleasant Experience	-0.142	(-0.287, 0.010)	0.068
Future	0.343	(0.202, 0.470)	**<.001**
Talk Again	0.277	(0.131, 0.412)	**<.001**
Opinion Change	-0.052	(-0.203, 0.100)	0.502

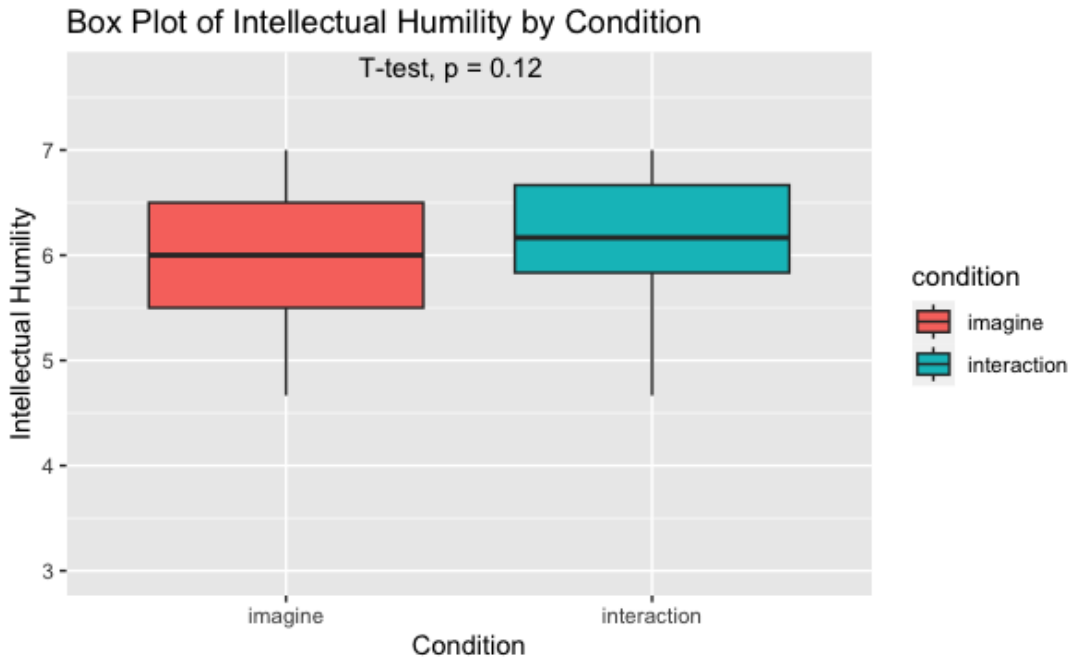
Experience, Unpleasant experience, and Opinion Change

As shown in Table 21, ratings of the conversation partner (Good Arguments) and the experience of the conversation (Good Experience) were significantly positively correlated with participants' self-reported Intellectual Humility. How unpleasant they rated the experienced to be (Unpleasant Experience) is not related to Intellectual Humility scores. How much participants' opinions changed after having or imagining a conversation also was unrelated to Intellectual Humility.

Moreover, to check for any variations of Intellectual Humility between whether participants had a conversation or imagined one, I produced a box plot of Intellectual Humility by condition (see Figure 7). As Figure 7 shows, there are no significant differences in Intellectual Humility scores between conditions.

Figure 7

Boxplot of Intellectual Humility across Imagine and Interaction conditions, with results of a t-test comparing means



Finally, I explored how Intellectual Humility may have influenced reporting an agreement or not. For this, I fitted a logistic mixed model (estimated using ML and Nelder-Mead optimizer) to predict Agreement from Intellectual Humility and Condition. The model included Dyad identification number as a random effect for participants in all conditions. A summary of the results can be found in Table 22.

The model suggests that participants' self-reported Intellectual Humility has no significant effect on whether they reported reaching an agreement or not (real or imagined). Moreover, there was no significant interaction between whether participants talked or imagined and their Intellectual Humility scores on the likelihood of agreement being reported.

Table 22

Summary of results for the logistic mixed model to predict Agreement with Intellectual Humility scores and Condition, including an interaction term and dyad as random effect.

	Agreement		
Predictors	*Log-Odds*	*CI (95%)*	*P-Value*
Intercept	-0.20	-6.71 – 6.31	0.952
Intellectual Humility	0.26	-0.83 – 1.35	0.634
Condition (Interaction)	5.37	-3.61 – 14.34	0.241
Int. Humility × Condition	-0.79	-2.25 – 0.68	0.293
Random Effects			
σ^2	3.29		
$\tau_{00\ dyad}$	3.00		
ICC	0.48		
$N_{\ dyad}$	91		
Observations	167		
Marginal R^2 / Conditional R^2	0.029 / 0.493		

In sum, Intellectual Humility seems to have a strong positive correlation with how good participants found the conversations to be, real or imagined, and how willing they were to engage in similar conversations in the future. Intellectual Humility did not correlate with how bad of an experience participants found the conversations to be or how much their opinion changed. Intellectual humility did not predict whether participants reached an agreement or not; nor did Intellectual Humility vary between conditions (Interaction or Imagine).

CHAPTER IV: DISCUSSION

The primary goal of this research project was to understand how short cross-ideological, virtual, face-to-face one-on-one conversations, as opposed to merely imagining these conversations, can help improve people's perspectives of such communication and those with opposite views. Additionally, I was interested in whether a short format of cross-ideological communication could end up in agreement more often than people would imagine, as well as whether it could significantly impact people's personal opinions on the issue they would discuss. For this, I ran a between-subjects design study with participants in Argentina holding or imagining these conversations on Zoom sessions and measuring different outcomes and assessments using online surveys during the session. We found promising significant results showing that people found these interactions better than they imagined under the conditions of this study. And although I did not find a significant difference between imagining and having conversations in terms of how often people reached an agreement or how much their opinion changed on the issue on which they initially disagreed, in this final chapter, I discuss potential reasons for these results that lay the ground for relevant future studies.

The effects of face-to-face conversation on the perceptions of cross-ideological communication

Regardless of whether participants in the conversations in this study achieved an agreement, I was interested in examining whether engaging in a real conversation resulted in more positive assessments of such conversations and participants' conversation partners than the expected assessments of these things when merely imagining such a conversation (Prediction 2). To test this, I used equivalent questionnaires in both the real and imagined conversation

conditions where participants rated qualitative aspects of the conversation and their conversation partner on a variety of scales: how enjoyable, comfortable, stressful, or challenging the conversation was or would be; how much they liked or would like their conversation partner; and assessments about their conversation partner's arguments in the discussion.

My study showed promising significant results, suggesting that engaging in a real cross-ideological conversation, rather than imagining one, resulted in more positive assessments of the conversation and the conversation partner (who, in the context of this study, was always somebody who held an opposite stance on a moral topic). Engaging in an actual conversation also resulted in less negative assessments of the conversation. When it came to how good of an experience participants found the conversation to be (meaning how enjoyable and comfortable they found it, as well as how much value they saw in it and how much they liked their conversation partner), the positive assessment was notably higher in the real conversation condition than in the imagination condition even when participants failed to reach or imagined not reaching an agreement on the moral issue with their partner. In other words, if they engaged in an actual conversation, there was a minimal additional ameliorative effect of reaching an agreement on participants' rating of the interaction. In contrast, if they merely imagined the conversation, imagining that they reached agreement led to much more positive evaluations than imagining that they did not.

Similarly, I was also interested in whether a short cross-ideological conversation had the power to increase people's willingness to engage in cross-ideological interactions in the future and whether these conversations would increase their willingness to talk again with people who held opinions opposite from their own, relative to the willingness of people who only imagined these discussions (Prediction 3). Once more, our results strongly supported our predictions:

people who had a conversation (versus imagining one) showed a greater willingness to engage in future discussions like the one they had and a greater willingness to talk again with the person they just talked to.

In addition, an exploratory analysis showed that self-reported Intellectual Humility was positively correlated with how positively participants rated the conversation (real or imagined) or how good they found or expected their conversation partner's arguments to be, as well as with their willingness to engage in more of these dialogues in the future and their willingness to talk with their conversation partner again. The strong correlations between Intellectual Humility and the conversation quality assessment and willingness measures can be a first step towards validating these measures, used in this study and others (Binnquist et al., 2022), as a way of measuring how to foster certain aspects of Intellectual Humility in intervention studies aimed at reducing polarization.

The set of results for conversation assessment and people's willingness to engage in more cross-ideological interactions seems to be in line with recent work on misperceived polarization, which has shown that people's incorrect views of those across the political divide can have important negative implications for their attitudes towards those on the other side, but that minor corrections in these misperceptions can reduce these negative attitudes (Lees & Cikara, 2021; Mernyk et al., 2022). In the current study, I have shown that engaging in a short interaction with somebody across a moral divide can positively affect perceptions of that person and interactions with them. Further research is needed to understand whether these outcomes can be generalized to people's general perception of people across ideological divides (not just the person they spoke with or imagined). If so, these short interactions may be effective as a possible intervention to correct misperceived polarization.

It is promising to see how much more favorably people who engaged in a conversation (versus those who merely imagined one) felt about cross-ideological communication in our particular design. In this design, the only difference between the Interaction and Imagine conditions was whether participants got a chance to talk to each other or not. As noted earlier, in both conditions, participants got to see each other, know each other's opinion on the topic, be in the same experimental session together, and spend time imagining their conversation. However, an actual one-on-one conversation made people feel more favorable about these kinds of interactions, regardless of all the elements that brought the imagination task as close to the actual conversation task as possible.

Furthermore, these findings may contribute to solving the problem shown in various reports that find that people avoid face-to-face conversations, especially politically or morally charged conversations (Chen & Rohla, 2018; Dunn, 2019). Aligning with Epley and Schroeder's (2014) findings on how people expect to be more uncomfortable conversing with strangers than they actually are when they are induced to have a conversation with strangers, the results of my study also show this pattern in polarized conversations on moral issues.

Binnquist et al.'s (2022) study suggests that affective forecasting may explain my results. They found that people assessed cross-ideological communication as going better than they forecasted they would go, and my study also found this. However, Binnquist et al.'s analyses comparing predicted and actual outcomes were exploratory; they were testing other hypotheses within their study that went beyond comparing forecasts and assessments of the conversation. For instance, they incorporated other factors as part of their experiment, such as having all participants talk with an ingroup member before engaging in the cross-ideological conversation –

this may have prepared participants for how to present their ideas in the cross-ideological conversation, which may have also influenced how these conversations were rated afterward.

Binnquist et al.'s design aimed to show an affective forecasting effect by having people predict the quality of the conversation before having one. In contrast, in my study, I specifically tried to compare the effects of a real conversation with an imagined one. My study was explicitly designed to make these comparisons using a between-subject design. The direct comparison of the two conditions provides robust evidence about the power of face-to-face communication to improve people's perceptions about and willingness to engage in cross-ideological communication.

My results also have implications for Intergroup Contact Theory, which has long shown how contact between members of conflicting groups can help reduce prejudice from ingroup members against outgroup members (Pettigrew, 1998; Pettigrew & Tropp, 2006). In my study, I obtained evidence of how contact, in a specific experimental setting, can potentially reduce conflict between opposing parties on measures other than prejudice reduction. I found improvement in how people perceive these particular interactions with the opposing party and their willingness to engage in them, which is relevant to managing conflict caused by polarized moral and political ideas and, thus, a potentially valuable factor in maintaining healthy democracies (Gutmann & Thompson, 1998). More importantly, these effects emerged after using a simple, easy-to-apply, short contact format. This paradigm can be used in future intervention studies of conflict resolution through intergroup contact (Paluck & Clark, 2020; Tropp & Mallett, 2011).

Opinions and agreement in cross-ideological communication

Reached vs. imagined agreement

The primary outcome I was interested in was whether people would be more likely to reach an actual agreement in a conversation about a contentious topic than they were to expect to reach an agreement when they imagined such a conversation. In this study, participants were asked to try to agree with the other person about a moral statement they were presented with. Given previous evidence showing how small independent groups composed of people with opposing opinions could reach consensus on moral issues (Navajas et al., 2019) and preliminary studies that found more agreement in real conversations than imagined ones (Niella et al., 2021), I predicted that agreement would be reported significantly more by those who had a conversation than by those who merely imagined it.

However, this hypothesis was not supported. A variety of factors may explain why no differences in agreement between the two conditions were found. First among these is that the analysis was sufficiently powered. In addition, in my previous studies, when people were asked to imagine a conversation, they did not have a real person in front of their eyes that they could imagine speaking to; they only had information about the other person's opinion. In contrast, in the present study, on top of knowing the person's position on the topic, participants also found themselves in the same online call session and could see each other's faces. The presence of imagery and attention to one individual might invoke a greater sense of closeness to the other participant (Lee & Feeley, 2016; Slovic, 2007), bringing the Imagine Condition closer to what happened during the Interaction Condition. This might explain why agreement levels were similar in the Interaction and Imagine Conditions in this study, suggesting that the mere presence of another person is in itself a big contributor to the likelihood of reaching or expecting

consensus. Furthermore, attention to the presence of a specific individual is a factor often missing in interactions in social media, where consensus and harmonious communication seem to be rare (Rathje et al., 2021; Sunstein, 2018).

Moreover, in previous studies, people in the Interaction Condition spoke face-to-face in person within a group or crowd experiment setting, surrounded by other people busy completing the task at the same time (Navajas et al., 2019; Niella et al., 2021). In contrast, in the current study, all sessions were conducted via Zoom, one dyad at a time, with an experimenter present. This Zoom meeting might have deprived people of some of the positive aspects of face-to-face communication, such as certain physical aspects that cannot be captured in video calls (bringing a possible greater chance for disagreement), while simultaneously providing more of the positive aspects of face-to-face interactions to those in the Imagine Condition (i.e., being able to see the other person's face). It must also be noted that there was more evident monitoring in the current study due to the recording and the presence of an experimenter, which may have inhibited participants in the Interaction Condition from vociferously disagreeing. It may also have encouraged participants in the Imagine Condition to think that reaching an agreement was something expected in the study, and, thus more possible.

Another essential factor to consider is the conversation's duration. Participants were only allowed to talk for five minutes in the Interaction Condition and were stopped by the experimenter once the time was up. They may not have had enough time to reach an agreement. In contrast, in previous studies, even though participants had a similar time frame, the interactions were less controlled and participants were not always on extreme opposite sides of the issue they discussed. Therefore, further research could vary the amount of time allowed for

discussion to see if agreement in the conversation condition goes up when participants have more time to reach it.

It is also important to discuss the fact that, in this study, reported agreement was very high in either condition (above 70%). As mentioned above, this might be due to people perceiving the instruction of reaching an agreement as something expected and therefore deciding to reach it (or expecting it) regardless of their personal beliefs on the subject, which was then supported by not seeing significant changes on participants' individual opinion after the manipulation. Moreover, as seen in the distribution of reported Points of Agreement (see Appendix D), for both conditions, we see a high peak in values in the middle of the scale, meaning that a large portion of participants agreed on a number roughly halfway between their two extreme positions. This is one way of reaching a compromise under these circumstances, although any change in attitudes was not reflected in the later individual opinions of participants, suggesting that participants might have just been acquiescing to the instructions when deciding to agree in the middle.

Moreover, in the context of this experiment, any agreement participants reported reaching did not have particularly high stakes; the topics discussed were not particularly highly debated issues in the sample's country at the time of the experiment. This might explain why participants easily reached agreements or expected to do so often. This raises questions about what "reaching an agreement" actually means in the in current study, beyond just picking a number. Further research needs to be done to understand whether participants would still agree on a number at such high rates if, for instance, the agreement reached had greater repercussions for the participants (e.g., having to agree on an issue for which a participant had previously

stated a position on social media) or if the topic they discussed is a highly divisive one within society (e.g., because a referendum regarding its governance is up for a vote).

Opinion change after cross-ideological communication

Another main research question in this study was whether people's individual opinions about a moral topic could change after engaging in a conversation with somebody who held an opinion opposing their own and whether this change would be greater than after imagining such a conversation instead. Part of the problem with polarization is people's reluctance to accept that those on the opposite side might have valid points or ideas. Therefore, I wanted to test to what extent engaging in an exchange of opinions can change people's position on a charged topic and whether imagining such an exchange has any impact on people's opinions, even if the effect is less than when an actual conversation takes place. Our results suggest that engaging in a cross-ideological conversation versus imagining one had little effect on people's positions, as did whether participants reached or expected to reach an agreement.

The lack of difference in opinion change between conversation and interaction conditions could be due to the fact (as previously mentioned) that the experimental paradigm compared two conditions that were actually similar. First, the time for conversations (imagined or real) was limited in both conditions (five minutes). On top of that, participants were on Zoom, with a recording in process and an experimenter present, following a task. This set of characteristics might have impeded the chances of differential impact on people's attitudes from either the interaction or imagination activities. Studies with longer and less controlled experimental manipulations might be needed to find differences in the impact of conversation versus imagination on people's opinions.

Interestingly, my exploratory analyses showed that people's willingness to engage in more conversations in the future was negatively correlated with how much people's opinions on the moral issue changed: The more willing people were to engage in future conversations, the less their opinion changed. This might sound counterintuitive. However, it might speak to the fact that people who are less likely to change their opinion might find talking with people holding opposite positions exciting or easy, as they might think they would not be affected by these opposite extremes. As mentioned, this is just an exploratory speculation; more research is needed to understand the relationship between people's stability on an opinion and their willingness to talk about that opinion with others.

There was also a negative correlation between how informed participants reported feeling on the topic and how much their individual opinion changed. This might reflect how people who feel informed about a topic (whether correctly or not) might resist changing their minds even when presented with conflicting evidence. For instance, research shows that high confidence in a minority opinion, can carry great weight in a group decision (Juni & Eckstein, 2015; Navajas et al., 2018). This, in turn, may feed conflict in spaces where people tend to express their opinions without the favorable aspects of face-to-face communication, such as on many social media platforms (Ko et al., 2014; Rathje et al., 2021; Sunstein, 2018): a person's post can be seen by both allies and opponents, exacerbating the way they defend their opinion as they engage with outgroup members while showing loyalty to their ingroup, especially if they feel highly informed about the topic and are less willing to change their opinion. These dual effects may also contribute to the perpetuation of misinformation, given that how informed people feel does not reflect on the quality or veracity of said information (Del Vicario et al., 2016; Pantazi et al., 2021).

General Limitations

As mentioned, one goal of this research was to identify a route that might help people across ideological divides reach a consensus and be more willing to exchange opinions and interact with opposing ideas, as a critical element of healthy democracies (Gutmann & Thompson, 1998). However, even though our study paired up people at opposite extremes on different moral topics, none of these topics were necessarily among "hot button" issues, such as the abortion laws debate in the US at the moment (Blazina et al., 2021). Our choice of the contentious topics used in the present study was in part influenced by the challenging aspect of recruiting participants on opposite sides of a moral spectrum and having two of them attend the same experimental session. Binnquist et al.'s (2022) study, which had many similarities to the current study, only successfully recruited about 21% of the participants who initially showed interest in participating. Similarly, in the current study, only 15% of potential participants actually took part. To make the recruiting process feasible, I had to appeal to an existent subject pool and participants whose opposing views on moral topics I already knew.

Working with an existing subject pool and a set of previously tested moral issues resulted in the use of six different issues. Some of these issues (sex work, freedom of speech and womb surrogacy) were generally picked more often than others (i.e., these issues tended to have more polarized responses within the subject pool). As a potential product of participants' time availability when pairing dyads up, together with their varied attendance rate, some issues were used in different proportions between the two different conditions, thus possibly confounding effects of issue and condition. Future research should aim to control for an equal proportion of topics discussed between conditions.

Furthermore, it is relevant to mention that the sample size of this study was on the lower end of the sample size goal range that was set at pre-registration. The higher end of the sample goal range was based on the effect size from the Hypothesis 1 analysis (proportion of agreement between conditions comparison) in one of the exploratory preliminary studies. However, prior work suggests that effect sizes often turn up to be smaller in replication studies (Open Science Collaboration, 2015). The results in the present sample come from a test of proportions with very low estimated power (< .13), suggesting that a larger sample size is needed to determine whether a significant difference in proportions between conditions exists. Therefore, aiming for greater sample sizes in future replication studies would be beneficial for testing my primary hypothesis. Nevertheless, the power analysis for the linear mixed effects model to predict Good Experience with Condition and Agreement estimated there was sufficient power for each predictor with our sample size, suggesting that my sample size was not a limitation for the Condition effects found in the said model.

Another peculiar aspect of my study is that for the mixed effects models, the variance of the dyadic random effects (τ_{00}) tended to be low or zero. This suggests that there was very little variance attributable to individual dyads. Perhaps this is because they were all built to be made up of two people holding extreme opposite positions on a moral issue. At any rate, in the case of our study, dyadic effects did not play an important role in our models. Further research is needed to check whether our findings would still hold when variance attributable to dyads is present.

It is also important to consider that participants in the Imagine Condition were asked whether they expected they would reach an agreement in the imaginary conversation before working on the imagination task where they wrote how they imagined the conversation to go. This may have affected the way they imagined and described the conversation. For instance, if

they expected an agreement, this might have primed the imagined conversation to be more positive, or vice versa, which, in turn, may have affected the assessment of said conversations. Nevertheless, as seen in our results, those who engaged in a conversation had an overall more positive assessment of the conversation than those who merely imagined it, regardless of whether agreement (real or imagined) was present or not. However, for certain aspects of how positively the conversation was rated (how comfortable they expected it to be, or their scores on the combined measure of expected good experience), participants who imagined disagreement rated them sharply lower. In a parallel manner, for ratings of one negative aspect of the conversation – how emotional participants expected their conversation partner's arguments to be – only in agreement, those who imagined it rated this negative dimension higher than those who had a conversation. Therefore, future work could seek to understand whether these differences unfold if agreement expectancy is asked after the imagination task, or not asked at all.

The reasoning behind asking participants to report whether they expected agreement or not first was to replicate the design and analysis from my previous studies, where participants were only asked to answer this when presented with the hypothetical conversation scenario, without asking them to take the time to imagine it or write about it at any point. Further research is needed to understand whether completing the imagination writing task first would change the proportion of participants expecting agreement in the Imagine Condition.

Another potential limitation related to the imagination task is the wide range in the number of words participants wrote during the five minutes they were provided to imagine the conversation. However, as the results of our quality-screening coding suggest, all participants stayed at least moderately on task; everyone wrote something, and nobody wrote something unrelated to what they were asked to write about. A possible explanation for the big difference in

the number of words written could be that the only limit participants were provided was time. Some might have started writing as they were imagining the conversation. In contrast, others might have taken a few minutes to think about the imagined conversation before they started writing, leaving them with a smaller window of time to write and therefore writing fewer words. Future studies could control for this variation by introducing two separate time limits, one for thinking about the imagined scenario and another exclusively for writing about it, or including a word count minimum and maximum.

Similar to other studies that rely on voluntary involvement, the present research might face the issue of self-selection bias. Although participants who signed up to participate did not explicitly know the study involved engaging in a face-to-face conversation about a moral issue with another individual holding opposing views, they were informed that this could happen when they signed the informed consent before the session started. The proportion of participants who left the study after learning what it involved was negligible. Nevertheless, the study was advertised as a social cognition study about conversations, which might have drawn a specific group of people to participate – possibly individuals who were non-representative of the larger population of polarized individuals. However, I suspect that the significant difference found between assessments of the conversations by those who imagined them and by those who actually had them might hold or possibly be even greater among polarized individuals unwilling to engage in conversations. An additional bias in selection was that I only selected participants with extreme opposing views on the moral issues. An interesting new direction for this research would be to study what the effects of face-to-face conversations are on people with more moderately distanced views would be.

Another limitation is that this study does not address the question of whether the positive effects of engaging in cross-ideological conversations under the conditions of this experiment persist in the long term. For instance, we do not know whether participants' increased willingness to engage in more of these conversations in the future would last for an hour, a day, or longer outside of the study setting. If the effects do persist, it is surprising that people generally seem to be pessimistic about these conversations if they actually go as well as they did in the current study. Could it be because, even after experiencing a positive conversation, people forget about the positive aspects of it? The effects of a good cross-ideological conversation experience may tend to fade with time. Or is it that because of the negative picture of communication about these topics provided by social media, people hardly ever engage in these conversations and form their expectations based on the divisive speech on these platforms? This highly divisive context may drive people to build overly pessimistic expectations about any kind of cross-ideological interaction, thus making these kinds of interactions, regardless of how they go, quite rare. Alternatively, there may be other aspects of the particular paradigm tested in my study that differ from real-life settings that made the conversations go better than usual. Further research is needed to answer these questions by looking at 1) whether and how long the positive perceptions of the conversations persist and 2) why people have pessimistic expectations about these conversations in general (e.g., hate speech in social media).

Finally, the conversations took place on Zoom rather than in person, with an experimenter present who gave participants a specific task (to try to reach an agreement in five minutes). These features of the paradigm create some artifacts that differ from what generally happens in "real life." Being in a virtual meeting platform like Zoom might provide a greater sense of distance from the opposing interlocutor, which in turn might influence participants to

feel more comfortable with disagreement when talking about a difficult topic (e.g., moral issues) than if they were talking in person. However, as my results suggest, using the setting of a virtual online meeting, these conversations can not only happen in a harmonious way, but they can also improve people's perceptions of such conversations and their willingness to engage in them in the future relative to people who only imagine these conversations.

Future Directions

Given the potential self-selection bias problem, it may be valuable for future research to investigate the similarities and differences between self-selected participants and the broader population of polarized individuals. Such an analysis could shed light on the factors that motivate individuals to participate in cross-ideological communication, which could inform efforts to promote greater participation in these activities.

Another relevant future study of people with opposing moral or political views trying to reach consensus could use a different agreement task that could provide more practical insights about the outcomes of cross-ideological conversations. In the present study, participants were asked to reach a consensus on a moral topic. Such a task might have motivated participants to present their ideas and perspectives on the topic in a clear way to the other person. However, an agreement between conflicting views about how acceptable or not a certain moral statement is, does not provide clear practical implications for society. In future studies, it would be interesting to provide tasks where participants need to agree on something more applicable to real life, such as agreeing on a specific public policy that is related to a moral issue.

Social media might play an essential role in how people perceive face-to-face cross-ideological conversations and how they expect them to go. Evidence shows that social media

makes animosity towards the opposite party salient (Rathje et al., 2021), possibly feeding into people's pessimism about these conversations. Therefore, an interesting future study would be to understand how social media and the dialogues within it influence people's perception of others across the divide and how these perceptions affect their expectations and willingness to engage in face-to-face cross-ideological communication.

My study had strangers with opposing views talk to each other. However, recent research has shown that face-to-face communication on hot-button topics among friends and family has been reduced as well (Chen & Rohla, 2018). Relationship type might affect the outcomes of cross-ideological dialogue, and people's expectations about the results of talking about contentious issues with close others versus strangers. Therefore, an interesting subsequent study would be to understand how the results of the present project hold or change with different levels of closeness between the conversation partners.

Given that we recorded both video and audio of the conversations in the Interaction Condition, future research will examine the prosody and vocabulary of the dialogues to understand whether specific language patterns predict different outcomes in the conversation. Similarly, we collected text in the Imagine Condition, where participants described how they imagined the conversation to go. This strategy was adopted as a way to make both conditions as similar as possible, but coding and running exploratory text analyses on these narratives might allow us to gain further insights into people's expectations about how these conversations will go. These narratives would be a novel source of data, given that they are in Spanish. Most mainstream research on how people interact in polarized environments has been done in the United States, in English (Huff, 2021). However, a large proportion of the world communicates

in Spanish. Therefore, these analyses may expand what is known about how people deal with conflict and polarization.

The fact that this experiment was conducted in Argentina raises some intriguing directions for future studies. For example, there are some cultural differences between Argentina and the U.S. (where a lot of previous research on the topic has been conducted). For instance, Argentina seems to be one of the countries to have the highest tolerance for physical closeness, even with strangers, whereas in the U.S., this tolerance is significantly lower (Sorokowska et al., 2017). Moreover, the U.S. ranks higher in terms of "tightness" (how much people stick to norms and rules, including social norms) relative to some South American countries that are culturally similar to Argentina (Gelfand et al., 2011). However, the results of the present study are generally in line with results from research that was conducted in the U.S. (Binnquist et al., 2022). An interesting future line of research would be to look for differences in the outcomes of my study if the data were collected in other countries that show stark contrasts in certain cultural aspects, such as how much people obey social norms of politeness versus cultures that are more comfortable with confrontation and disagreement.

The present research suggests that short conversations in an online format (i.e., video calls) still provide many of the benefits of one-on-one, face-to-face conversation. These findings hold great promise for the potential of video call paradigms to enable effective cross-ideological communication, not only for benefiting researchers as an efficient format to study this sort of communication but also for participants as a potential format that can be scaled to motivate this type of communication in everyday society – for instance, perhaps there could be a "quick video chat" button on online debate forums such as Facebook groups.

Furthermore, fieldwork is needed to understand where the pessimism about cross-ideological face-to-face conversations is strongest and then design interventions suitable to these specific environments, which requires collaboration across multiple areas of expertise. This type of work is relevant since, as mentioned before, misperceptions that happen across an ideological divide go beyond conflicting racial, religious, advantaged vs. disadvantaged groups, where much of the work applying Intergroup Contact Theory interventions has focused. It may also be time to apply contact interventions in less obvious conflict areas. For instance, my collaborators, together with a set of organizations, are currently running a version of "My Country Talks" in Argentina (Fundacion Bunge & Born, 2023), where they look for people agreeing or disagreeing with specific policy measures related to economics, politics, and society that are part of the country's current agenda. They then pair up people with opposite perspectives and invite them to get together and talk on a video call or in person in a public space. They will measure this intervention's impact, including using some of the measures used in the current research project.

In order to try to apply the findings of this research to possible future interventions, it is important to acknowledge aspects of the particular experimental context that potentially contributed to the positive outcomes that I found. One of these may be how positive the experience of the conversation (imagined or actual) was. In an exploratory analysis (see Appendix D), I found that higher ratings on the "good experience" factor significantly increased the chances of participants reaching agreement in my experiment. Thus, an important pre-condition for agreement may be that participants found the conversation to be a pleasant experience, something that likely included liking their conversation partner. In the current study, participants generally found the conversations pleasant – the overall mean score on the "good experience" factor in Interaction Condition was 6.24 (SD = 0.75; on a scale where 7 was the

highest score). The finding that more pleasant conversations are more likely to lead to agreement also provides an intriguing avenue for future research.

Other specific aspects of my experimental context that may have contributed to the successful outcomes can be summarized as follows: 1) The conversation was between strangers – politeness norms might be stronger with strangers than close others (Mourad, 2021; Ye, 2019). 2) The conversation took place on Zoom, which might have made people feel more comfortable facing an opposing point of view than in person since it provides some distance. 3) Participants were given the common goal of trying to reach an agreement, which might have motivated them to listen to each other and try to reach a consensus. 4) The experimenter – who provided the instructions to participants to try to reach an agreement – may have functioned as an authority figure who was passively present during the conversation. 5) The topic being discussed was a polarizing one but not a hot-button topic at the time of the experiment. Additionally, the agreement position participants did not have major implications for their lives or their community. Points 1, 3, and 4, together with the fact that participants found the interaction to be a positive experience, resemble some of Allport's prerequisites for the Contact Hypothesis to work (Allport, 1954; Pettigrew, 1998). These characteristics may be important in building paradigms for future Intergroup Contact studies and interventions that look to reduce conflict between groups who are at different sides of moral or political polarization.

Finally, when sharing my research in multi-disciplinary settings with experts on international law, conflict resolution, or community action planning, I have always received comments on how relevant these findings are for their field. They believe that sharing this information as I do (i.e., sharing scientific evidence in an accessible way to the broader community) would be of great relevance and would have a positive impact among the

communities they work with. This, to me, suggests that, together with cross-ideological conversation interventions, a significant next step would be to increase work on scientific communication about these areas of research in the areas of work on conflict resolution (community centers, non-profits, etc.) that need it the most.

Concluding remarks

All in all, my study helped us see how a short five-minute cross-ideological conversation on Zoom can improve perceptions about talking to people holding extreme opposing views and engaging in more exchanges with those across an ideological divide. In the present day, where polarization is high, face-to-face communication is low, and threats to democracies grow worldwide, this finding brings optimism and offers possibilities about how these trends can be reversed. The current study shows that these conversations are not only possible under certain circumstances but can have genuinely positive outcomes. Continuing work on understanding what characteristics make cross-ideological communication harmonious and fruitful can lead to building conversation spaces to reduce conflict and division in society.

APPENDIX A: STATEMENTS, DETAILED MEASURES, & EXPERIMENT SCRIPT

Statements

These are the statements participants will rate on the point of agreement. They rate all of them in the pre-screening and then work with only one in the experiment.

- Immortal: "If scientifically possible, people should be immortal."
- Gene: "Human gene editing should be allowed to prevent diseases."
- Vegetarian: "All people should be vegetarians."
- Surrogacy: "Surrogacy to have children should be legal."
- Speech: "There should be no limit to freedom of speech."
- Sex Work: "The supply and demand of sex work should be legal."

Measures

Individual Opinion questionnaire

Individual opinion

"How much do you agree or disagree with this statement? Please answer using the following scale" (11-point Likert-scale, 0 = "Absolutely disagree", 5 = "Nor agree nor disagree", 10 = "Absolutely agree").

Sure

"How sure do you feel about your answer? Please answer using the following scale" (7-point Likert-scale, 1 = "Not at all", 7 = "A lot").

Care

"How much do you care about this issue? Please answer using the following scale" (7-point Likert-scale, 1 = "Not at all", 7 = "A lot").

Informed

"How informed do you feel about this issue? Please answer using the following scale" (7-point Likert-scale, 1 = "Not at all", 7 = "A lot").

Agreement questionnaire

Agreement

Interaction condition: "Were you able to reach an agreement?" Yes/No

Imagine condition: "Do you think you would be able to reach an agreement with this person?" Yes/No

If they answered 'Yes':

Point of agreement

Interaction condition: "What number did you agree on?" (11-point Likert-scale)

Imagine condition: "What number do you think you would agree on?" (11-point Likert-scale)

Experimenter Assessment

Basic information:

- Experimenter name
- Dyad Number
- Participant A ID code
- Participant Z ID code
- Condition: (i.e., interaction condition in this case)
- Issue they discussed
- Participant A pre-position on the issue (from the pre-screening survey in Stage 1)
- Participant Z pre-position on the issue (from the pre-screening survey in Stage 1)

Interaction condition only:

- How well did they follow the instructions? (0 = not at all; 1 = They just picked a number, but did not really exchange opinions; 2 = In between – they exchanged opinions, but then picked a number without reaching a real consensus; 3 = They exchanged perspectives and tried to reach a number that represented an agreement between their two opinions)
- Did it seem like they really reached an agreement? (Yes or No)

If yes:

- What number did it seem they agreed on?

- To what extent do you perceive they genuinely arrived to the number they agreed on? (Participant A) (1= Not at all, 2= Kind of, 3= Absolutely)

- To what extent do you perceive they genuinely arrived to the number they agreed on? (Participant Z) (1= Not at all, 2= Kind of, 3= Absolutely)

Both conditions:

- Any comments about this session? (Only comment if something outstanding happened that could have potentially interrupted the ordinary course of the study)

Interaction Quality

Participants were asked the following questions about their experience in Stage 2. For each statement, they provided their agreement on a 7-point scale: 1 = “Strongly Agree”, 2 = “Disagree”, 3 = “Somewhat Disagree”, 4 = “Nor agree/Nor Disagree”, 5 = “Somewhat Agree”, 6= “Agree”, 7 = “Strongly Agree”.

Conversation assessment

Thinking about the conversation you just had [if Interaction condition] / you just imagined [if imagine condition]:

Imagine condition

1. I feel this interaction would be enjoyable
2. I feel this interaction would be very stressful
3. I would be afraid of being judged by the other person present
4. I would feel comfortable in this interaction
5. I feel the interaction would be difficult to get through
6. I would be willing to engage in more interactions like this
7. I see value in engaging in interactions like this one
8. Reaching an agreement would be extremely hard to achieve

Interaction condition

1. I felt this interaction was enjoyable
2. I felt this interaction was very stressful
3. I felt judged by the other participant
4. I felt comfortable in this interaction
5. I felt this interaction was difficult to get through
6. I am willing to engage in more interactions like this
7. I see value in engaging in interactions like this one
8. Reaching an agreement was extremely hard to achieve

Partner assessment

Thinking about the person you just had [if Interaction condition] / you just imagined having [if imagine condition] the conversation with:

Imagine Condition

1. I believe their ideas would be valid
2. I believe their ideas would be convincing
3. I think I could respect their opinions
4. I believe their statements would be driven by emotion
5. I believe their statements would be driven by logic
6. I think I would like them as a person
7. I think I would be willing to talk with them again
8. I think their arguments would have an impact in my own opinion about the issue

Interaction Condition

1. I believe their ideas were valid
2. I believe their ideas were convincing
3. I think I can respect their opinions
4. I believe their statements were driven by emotion
5. I believe their statements were driven by logic
6. I think I can like them as a person
7. I think I am willing to talk with them again
8. I think their arguments had an impact in my own opinion about the issue

Demographics

- Age
- Gender (Male, Female, Non-Binary, Gender Fluid, Prefer not to say, Other)
- Religion (Catholic, Jew, Protestant, Evangelical, Atheist, Agnostic, Adventist/Jehovah's Witness, Mormon, Muslim, Other, None in particular),
- Highest level of education attained (None, Primary, High School, College or Associate's degree, Masters or Doctorate degree, Other)
- Highest level of education attained by either of the participant's parents (same options as for own level of education)
- Political Party
- Political Orientation (Right, Center-Right, Center-Left, Left, Unsure/Prefer not to say)

Intellectual Humility

In this study, I used the 6-item scale developed by (Leary et al., 2017):

Now, please read the following statements about yourself carefully. For each one, indicate your level of agreement or disagreement using the following scale:

1 (Strongly disagree), 2 (Disagree), 3 (Somewhat disagree), 4 (Neutral), 5 (Somewhat agree), 6 (Agree), 7 (Strongly agree)

1. I question my own opinions, positions, and viewpoints because they could be wrong.
2. I reconsider my opinions when presented with new evidence.
3. I recognize the value in opinions that are different from my own.

4. I accept that my beliefs and attitudes may be wrong.
5. In the face of conflicting evidence, I am open to changing my opinions.
6. I like finding out new information that differs from what I already think is true.

Experiment script

In Table 23, you will see the text in the slides presented on screen with the instructions, and what are the instructions read/said out loud by the experimenter - most of the time the oral and written instructions are the same. In the original experiment, these are all in Spanish, here, you will see the translated version. In brackets [], I indicate the experimenter's actions in certain stages. First, as participants join in Zoom, the experimenter makes sure their names are changed to “Participant A/Z” accordingly, and if not, the experimenter edits it. The experimenter also sets the chat box so that the participants can only chat with the Host of the meeting (the experimenter). Then, the experimenter starts sharing their screen and the experiment begins and unfolds as specified in the table below.

Table 23

Experiment script per condition: Experimenter actions (left column) and text in slides (right column).

Imagine Condition	
Slide 1	
[The Experimenter reads the instructions off the slide]	Welcome to our study! Before you start, please make sure your camera is turned on and your microphone is turned off. If you have any questions during the session, you can send me a private message through the chat box.
Slide 2	
[The Experimenter reads the instructions off the slide. If a participant answers "No" experimenter sends link to Consent Form and waits until they read and sign. If everyone gave consent to record session, experimenter starts recording]	Did you complete the informed consent? Answer "Yes" or "No" in the chat.
Slide 3	
[The Experimenter reads the instructions off the slide]	Do you have your Participant Code at hand? If you don't know it, you can find it in the confirmation email. To confirm that you know/have it on hand, write "Yes" in the chat.
Slide 4	
[The Experimenter reads the instructions off the slide]	Now I am going to present a statement about a moral dilemma. Each one's task is to determine how much they agree or disagree with the statement based on their own beliefs. To provide your answers, you will be using an online form – please do not say your answers until I send you the link in the chat. This stage is individual. Please be honest with your answer. Remember that your responses are kept anonymous.
Slide 5	
[The Experimenter reads off the slide]	{Statement text} Please use the link to Form 1 provided in the chat and fill it out. When you're done, reply "Done" in the chat.

Table 23 (continued)

Imagine Condition	
Slide 6	
[The Experimenter reads the instructions off the slide. Then proceeds to text in the chat to each participant privately and accordingly: "Participant A/Z answered X", where "X" is the number they responded to the statement in the pre-screening survey]	Now, in the chat I'm going to share what each one's response was to the question about how much they agree with the statement using the 0-10 scale. Each participant will see what the other's response was privately. {Box with the statement as reminder} Statement: " – " {Box with the 0-10 scale as reminder} Scale: 0= "Totally disagree" 1 = "Strongly disagree" 2 = "Disagree" 3 = "Somewhat disagree" 4 = "Barely disagree" 5 = "Neither agree nor disagree" 6 = "Barely agree" 7 = "Somewhat agree" 8 = "Agree" 9 = "Strongly agree" 10 = "Totally agree"
Slide 7	
"In this next task you have to imagine the following" [Experimenter reads the instruction on the bullet points in the slide]	Next task – **Imagine** • Imagine that you have 5 minutes to talk with this person about how much you both agree or disagree with the statement. • Also, imagine that in that time you should try to agree on an answer together, on the scale of 0 to 10 Imagine that when the 5 minutes are up, you can't talk anymore, and if you don't come to a consensus, that's fine too.
Slide 8	
[The Experimenter reads the instructions off the slide]	Now, go to Form 2 and answer all the questions carefully. The link is in the chat. When you're done, reply "Done" in the chat.

Table 23 (continued)

Imagine Condition	
Slide 9	
[The experimenter changes to this slide as soon as the first participant replies “Done”, but does not read the slide until both participants finish the form]	Thank you for your participation! You are now free to leave. In the next few minutes you will receive an email requesting information to process the payment of your honorarium. In addition, in a week you will receive an invitation to fill out a short form. If you complete it, you will enter a raffle for $10,000 pesos.
Interaction Condition	
Slide 1	
[The Experimenter reads the instructions off the slide]	Welcome to our study! Before you start, please make sure your camera is turned on and your microphone is turned off. If you have any questions during the session, you can send me a private message through the chat box.
Slide 2	
[The Experimenter reads the instructions off the slide. If a participant answers “No” experimenter sends link to Consent Form and waits until they read and sign. If everyone gave consent to record session, experimenter starts recording]	Did you complete the informed consent? Answer “Yes” or “No” in the chat.
Slide 3	
[The Experimenter reads the instructions off the slide]	Do you have your Participant Number at hand? If you don't know it, you can find it in the confirmation email. To confirm that you know/have it on hand, write "Yes" in the chat.

Table 23 (continued)

Interaction Condition	
Slide 4	
[The Experimenter reads the instructions off the slide]	Now I am going to present a statement about a moral dilemma. Each one's task is to determine how much they agree or disagree with the statement based on their own beliefs. To provide your answers, you will be using an online form - please do not say your answers until I send you the link in the chat. This stage is individual. Please be honest with your answer. Remember that your responses are kept anonymous.
Slide 5	
[The Experimenter reads off the slide]	{Statement text} Please use the link to Form 1 provided in the chat and fill it out. When you're done, reply "Done" in the chat.
Slide 6	
"Alright, now I'm going to give you the instructions for the next task" [Experimenter reads the instructions on the bullet points in the slide]	Next Task: • You have 5 minutes to talk about how much you both agree or disagree with the statement • You should try to agree on an answer together, on a scale from 0 to 10 • You must use the entire time to chat, you cannot finish before. • When the 5 minutes are up, you must stop talking. When there is 1 minute left, I will let you know in the chat. • If you can't reach a consensus, that's okay too.

Table 23 (continued)

Interaction Condition	
Slide 7	
"As a reminder, this is the statement, and this is the scale. I will include this and the instructions in the chat. When I say NOW you can turn your mics on and start talking. During the 5 minutes, I will stop sharing screen and will turn my mic and camera off." [The experimenter sends the statement, scale, and instructions in the chat and stops sharing the screen] "NOW" [Experimenter turns off their own mic and camera]	{Box with the statement as reminder} Statement: " – " {Box with the 0-10 scale as reminder} Scale: 0= "Totally disagree" 1 = "Strongly disagree" 2 = "Disagree" 3 = "Somewhat disagree" 4 = "Barely disagree" 5 = "Neither agree nor disagree" 6 = "Barely agree" 7 = "Somewhat agree" 8 = "Agree" 9 = "Strongly agree" 10 = "Totally agree"
Slide 8	
[At 4 minutes, the experimenter turns off the mic and quickly says "You have 1 minute left", then turn off mic again. When 5 minutes are up, the experimenter turns their camera and mic on and talks] "Stop! Please turn off your mics. Thank you."	Thanks for joining the conversation. Please turn off your microphones.
Slide 9	
[The Experimenter reads the instructions off the slide]	Now, go to Form 2 and answer all the questions carefully. The link is in the chat. When you're done, reply "Done" in the chat.
Slide 10	
[The experimenter changes to this slide as soon as the first participant replies "Done", but does not read the slide until both participants finish the form]	Thank you for your participation! You are now free to leave. In the next few minutes you will receive an email requesting information to process the payment of your honorarium. In a week you will receive an invitation to fill out a short form and you will enter a raffle for $10,000 pesos upon completion

APPENDIX B: LONG-TERM OPINION CHANGE EXPLORATORY ANALYSIS

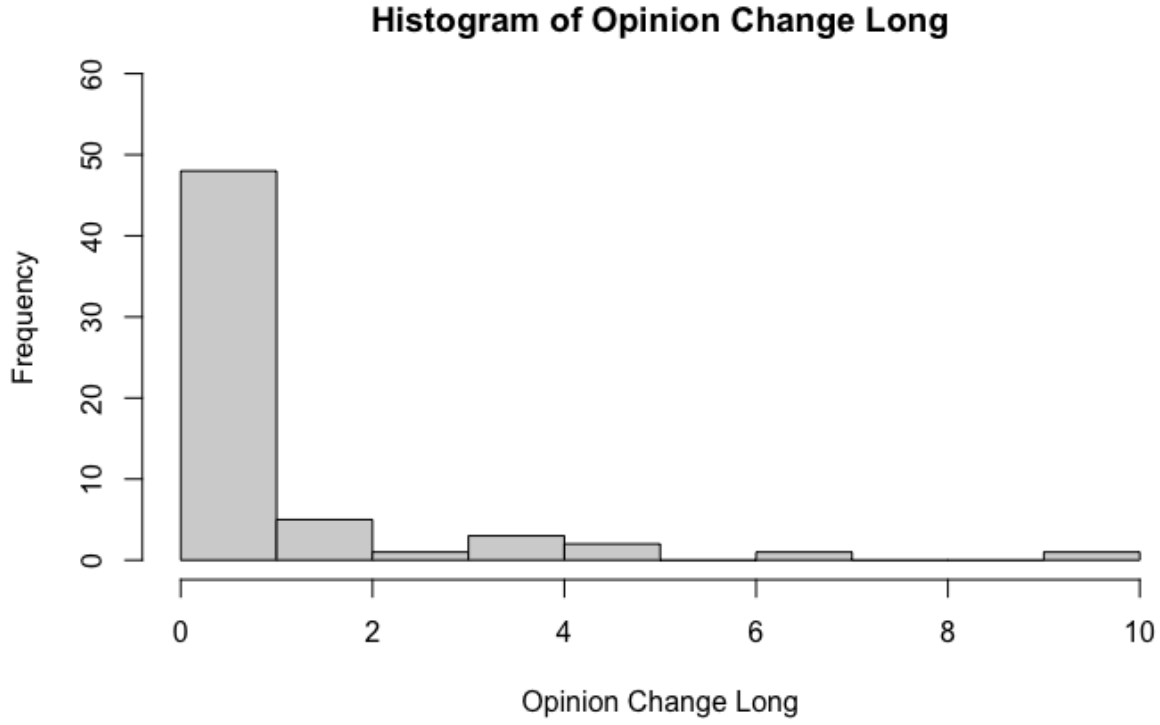
As part of the study, we also invited participants to report on their individual opinion a week after the experiment to explore whether opinion change effects remained in the long term. Response rates were low, and we ended up with only 61 responses (21 in Imagine Condition, 40 in Interaction Condition). Moreover, given the lack of significant results or the skewed distribution of opinion change immediately after the manipulation, I did not expect important findings to come out of looking at this data. Nevertheless, in the present section, I summarize results analyzing opinion change from before the manipulation to a week after the manipulation.

This was measured by taking the absolute value of the difference between their level of agreement with the moral statement in the Pre-Measure form ("Individual Opinion Pre") and their answer to Individual Agreement in the Post-Experiment form a week after ("Individual Opinion Post-Long"). This difference score was labeled "Opinion Change Long."

As in Opinion Change after the manipulation, the distribution of Opinion Change Long scores is very skewed to the right, indicating that the big majority of participants did not change their opinion a week after the manipulation or only changed by 1 point (very few changed by 2 points, and only a handful by more than 2 points; see Figure 8 below).

Figure 8

Histogram of Opinion Change Long (from before the manipulation, to a week after the manipulation)



I ran a linear mixed model to predict Opinion Change Long from Condition and the initial individual opinion point (Opinion Pre) and accounting for Dyad random effects. A summary of the results can be found in Table 24. To solve for the non-normality of the dependent variable, I also computed a bootstrapped model (1000 iterations) to compare results (see Table 25).

Table 24

Summary of results of a linear mixed model to predict Opinion Change Long from Condition and Opinion Pre, including Dyad as random effect.

	Opinion Change Long		
Coefficient	*Estimates*	*CI (95%)*	*P-Value*
Intercept	2.19	0.77 – 3.60	**0.003**
Condition (Interaction)	-0.87	-2.72 – 0.98	0.349
Opinion Pre	-0.17	-0.39 – 0.06	0.139
Condition × Opinion Pre	0.10	-0.18 – 0.39	0.480
Random Effects			
σ^2	3.63		
$\tau_{00\ dyad}$	0.00		
$N_{\ dyad}$	45		
Observations	61		
Marginal R^2 / Conditional R^2	0.054 / -		

Table 25

Summary of bootstrapped results of a linear mixed model to predict Opinion Change Long with Condition and Opinion Pre, including Dyad as random effect.

Coefficient	Original	Bootstrap Estimate	Standard Error	Bias	CI 95%
Intercept	2.189	2.222	0.703	0.033	(0.779, 3.533)
Condition (Interaction)	-0.873	-0.923	0.949	-0.050	(-2.684, 1.037)
Opinion Pre	-0.168	-0.169	0.112	-0.001	(-0.386, 0.053)
Condition × Opinion Pre	0.101	0.105	0.147	0.004	(-0.191, 0.385)

What this set of results shows is that how much a participant's individual opinion on the topic changed in the long term (a week after) was not influenced by whether participants had a conversation or just imagined having one. Moreover, the participants' initial individual opinions on the topic did not influence how much their opinion changed a week after the conversation or imagination task.

Next, I explored how other measures could influence people's change in opinion a week after the manipulation, including variables from the Quality Assessment and Willingness questionnaires, Opinion Pre (their initial point of agreement with the topic), plus the other measures of the Individual Opinion Pre questionnaire (how sure they felt about their initial opinion, how much they cared about the topic, how informed they felt about the topic). For the Quality Assessment measures, again, I used the reduced variables from the PCA: Good Arguments, Good Experience, and Unpleasant Experience. Moreover, I also explored how Opinion Change Long (a week after the manipulation) related to Opinion Change (immediately after manipulation). To run this exploratory analysis, I computed the correlations of these variables with Opinion Change Long. I also ran a bootstrap of 1000 repetitions to obtain an estimated bootstrapped correlation for each variable. A summary can be found in Table 26.

Table 26

Bootstrapped correlation coefficients of Opinion Pre, Informed Pre, Sure Pre, Care Pre, Future, Talk Again, Good Arguments, Good Experience, Unpleasant Experience, and Opinion Change with Opinion Change Long as single target variable.

Variable	Original	Bias	SE	Bootstrap Estimate	CI 95%
Opinion Pre	-0.190	0.010	0.133	-0.179	(-0.475, 0.041)
Informed Pre	-0.337	0.010	0.118	**-0.327**	(-0.590, -0.128)
Sure Pre	-0.128	-0.008	0.131	-0.135	(-0.352, 0.155)
Care Pre	-0.211	0.009	0.123	-0.203	(-0.474, 0.018)
Future	-0.405	0.058	0.231	**-0.348**	(-0.994, -0.118)
Talk Again	-0.181	0.026	0.168	-0.155	(-0.556, 0.101)
Good Arguments	0.016	0.019	0.149	0.035	(-0.303, 0.286)
Good Experience	-0.242	0.025	0.162	-0.217	(-0.643, -0.001)
Unpleasant Experience	0.005	-0.011	0.159	-0.006	(-0.310, 0.309)
Opinion change	0.743	-0.027	0.139	**0.716**	(0.582, 1.000)

These results show that how informed participants initially reported feeling (Informed Pre) and how willing to engage in more conversations in the future they reported to be (Future) had some of the strongest correlations with how much their individual opinion changed a week after the experiment. There is a negative correlation between change in opinion and how informed participants reported being on the topic, suggesting that the more informed they were, the less their opinions changed after having or imagining a cross-ideological conversation. This is also the case for the correlation between Future and Opinion Change: the higher participants

rated their willingness to engage in conversations like this in the future, the less their opinion changed. This goes in line with the results we obtained for Opinion Change immediately after the manipulation.

Furthermore, we see that how much a participant's opinion changed immediately after the manipulation is strongly and positively correlated with how their opinion changed a week after the experiment.

APPENDIX C: EXPLORATORY ANALYSIS OF CHANGE IN CHARACTERISTICS OF INDIVIDUAL OPINION

To explore whether the measures around participants' Individual Opinions: Sure, Care, and Informed changed from before to after the manipulation, I computed change measures for each one of them by calculating the absolute value of the difference between the answer in the Pre-Manipulation Form and the answer in the Post-Manipulation Form. To look at whether having engaged in a cross-ideological conversation (Interaction Condition) or having imagined one instead (Imagine Condition), or whether an agreement was reached (or expected) or not had any effects on these change measures, I ran mixed effect models to predict each change measure with Condition, Agreement, and the interaction of both, including Dyad as random effect. A summary of the results can be found in Table 27. As it can be seen in the results, no significant effects from Condition or Agreement were found on the change of how sure participants felt about their opinion or the change of how informed or how much they cared about the topic.

Table 27

Results from linear mixed models of Sure Change, Care Change, and Informed Change, with Condition, Agreement, and the interaction term as predictors, including Dyad as random effect.

	Sure Change					Care Change				
Coefficient	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*p*	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*p*
Intercept	0.37	-0.37	-0.10 – 0.85	-0.86 – 0.12	0.118	0.47	-0.16	0.05 – 0.89	-0.64 – 0.32	**0.029**
Condition (Interaction)	-0.01	-0.01	-0.65 – 0.63	-0.67 – 0.65	0.984	0.09	0.10	-0.46 – 0.64	-0.52 – 0.73	0.748
Agreement (Yes)	0.38	0.39	-0.17 – 0.94	-0.18 – 0.97	0.177	0.24	0.28	-0.26 – 0.74	-0.29 – 0.85	0.338
Condition × Agreement	0.15	0.16	-0.59 – 0.89	-0.61 – 0.92	0.685	-0.20	-0.23	-0.84 – 0.44	-0.96 – 0.50	0.535
Random Effects										
σ^2	0.91					0.78				
τ_{00}	0.00 $_{dyad}$					0.00 $_{dyad}$				
N	81 $_{dyad}$					91 $_{dyad}$				
Observations	147					167				
Marginal R^2 / Conditional R^2	0.047 / -					0.007 / -				

Table 27 (continued)

	Informed Change				
Coefficient	*Est.*	*Std. Beta*	*CI (95%)*	*Std. CI*	*p*
Intercept	0.32	-0.13	-0.03 – 0.68	-0.61 – 0.34	0.075
Condition (Interaction)	0.03	0.04	-0.44 – 0.51	-0.59 – 0.68	0.893
Agreement (Yes)	-0.03	-0.04	-0.44 – 0.38	-0.59 – 0.50	0.883
Condition × Agreement	0.20	0.27	-0.34 – 0.74	-0.46 – 0.99	0.468
Random Effects					
σ^2	0.44				
$\tau_{00\ dyad}$	0.12				
ICC	0.22				
$N_{\ dyad}$	91				
Observations	167				
Marginal R^2 / Conditional R^2	0.019 / 0.232				

APPENDIX D: ADDITIONAL FIGURES AND ANALYSES

An in-depth look at Agreement measure

Histograms of the reported point of agreement broken down by Condition can be found below in Figure 9. We can observe that a big concentration of participants that reported agreement seem to have reached or imagined an agreement in the middle (5 on the 0 to 10 Likert scale). In the Interaction Condition, there seems to be a wider distribution among more extreme points of agreement than the ones expected in the Imagine Condition. Furthermore, the histograms in Figure 10 show the difference between participants' initial opinion on the issue (Opinion Pre) and the reported Point of Agreement, by Condition. The distributions are similar between Conditions. We can see a high frequency in 0 or -1, which might belong to all the participants who reported or anticipated a point of agreement closer to their own opinion. In contrast, we also see high peaks on values below -2 and above 2, which may represent the participants on either extreme who reported or expected a point of agreement in the middle.

Figure 9

Histograms of Reported Point of Agreement, per Condition

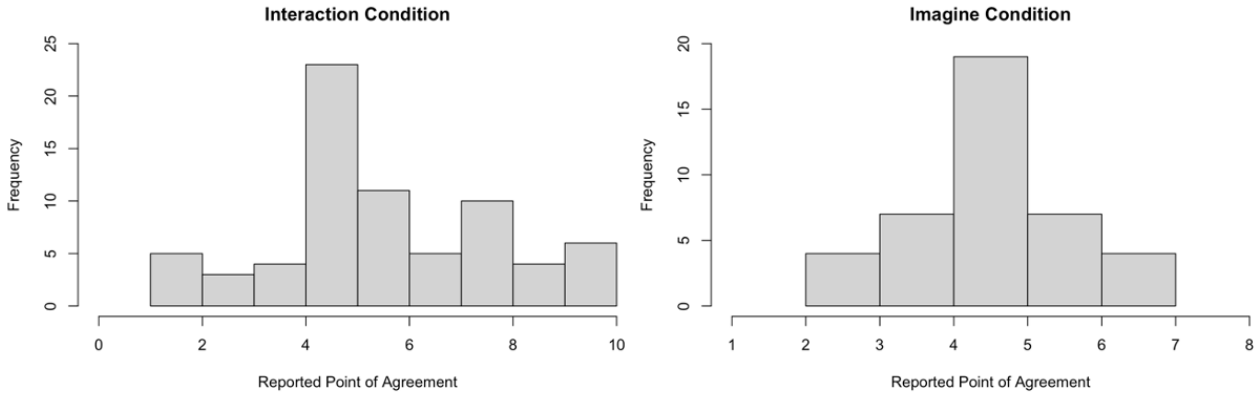
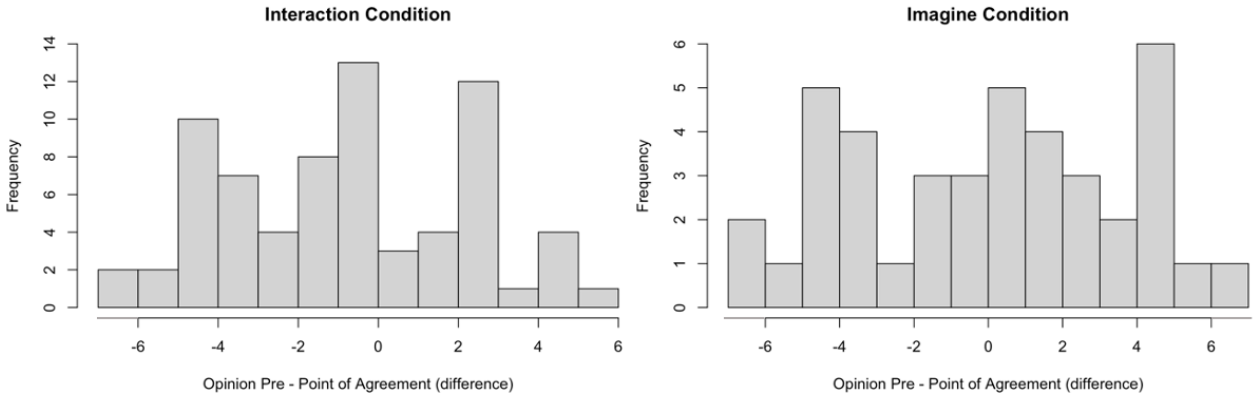


Figure 10

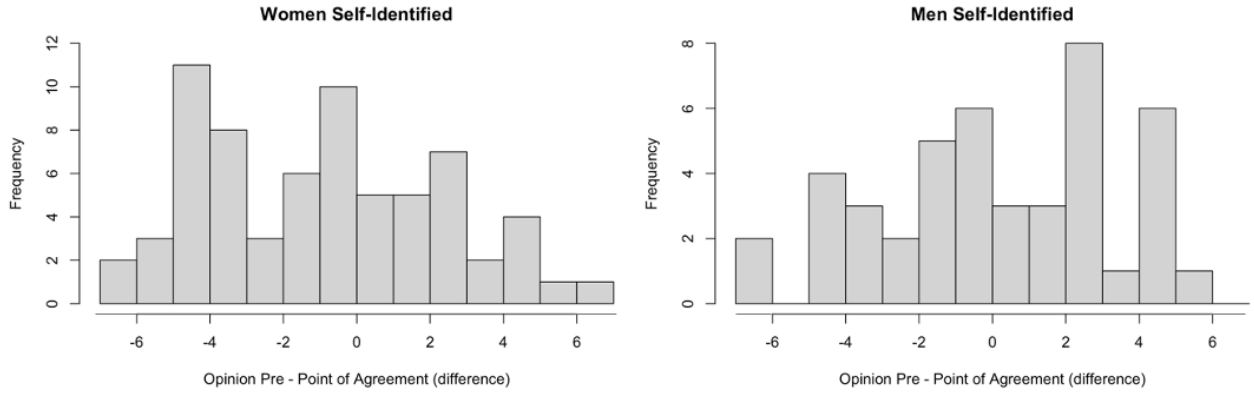
Histograms of the difference between participants' initial opinion on the issue (Opinion Pre) and the reported Point of Agreement by Condition



To explore if there were any differences on gender in terms of how far participants were willing to move from their initial opinion on the Point of agreement, I plotted histograms of the difference between participants' initial opinion on the issue (Opinion Pre) and the reported Point of Agreement, by gender (given that only less than 10% of participants reported a gender different than male or female, they were excluded in this analysis). When looking at the histograms in Figure 11, there does not seem to be major differences between distributions for self-identified men or women, except that men's distribution of distance from the initial opinion to the point of agreement seems to be skewed to the left, and women's to the right. This suggests that men may be moving up more often to a point of agreement on the statement, and women moving down to a point of agreement with the statement.

Figure 11

Histograms of the difference between participants' initial opinion on the issue (Opinion Pre) and the reported Point of Agreement by Self-Identified Gender.



Good Experience as predictor of Agreement

To test whether participants' rating of the conversation (real or imagined) as an overall Good Experience (a combination of items as a result of the PCA) predicted higher chances of reaching or expecting agreement, I ran a mixed effect logistical regression to predict Agreement from Good Experience and controlling for Condition and the interaction between the two. Results are summarized in Table 28 below. As seen in the table, Good Experience significantly predicts higher chances of Agreement (real or imagined).

Table 28

Results from logistic model to predict Agreement with Condition and Good Experience, including interaction term and dyadic random effects.

	Agreement		
Predictors	*Odds Ratios*	*Std. Error*	*P-Value*
Intercept	0.00	0.00	**0.024**
Condition (Interaction)	4454.05	21050.09	0.075
Good Experience	5.15	3.23	**0.009**
Good Experience x Condition (Int.)	0.22	0.18	0.061
Random Effects			
σ^2	3.29		
$\tau_{00\ dyad}$	4.87		
ICC	0.60		
$N_{\ dyad}$	91		
Observations	168		
Marginal R^2 / Conditional R^2	0.109 / 0.641		

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