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How In-Person Conversations Shape Political Polarization: Quasi-Experimental Evidence from a Nationwide Initiative

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How in-person conversations shape political polarization: Quasi-experimental evidence from a nationwide initiative

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Abstract

Growing political polarization is often attributed to "echo chambers" among like-minded individuals and a lack of social interactions among contrary-minded individuals. We provide quasi-experimental evidence on the effects of in-person conversations on individual-level polarization outcomes, studying a large-scale intervention in Germany that matched pairs of strangers for private face-to-face meetings to discuss divisive political issues. We find asymmetric effects: conversations with like-minded individuals caused political views to become more extreme (ideological polarization); by contrast, conversations with contrary-minded individuals did not lead to a convergence of political views, but significantly reduced negative beliefs and attitudes toward ideological out-group members (affective polarization), while also improving perceived social cohesion more generally. These effects of contrary-minded conversations seem to be driven mostly by positive experiences of interpersonal contact.

Keywords: polarization, intergroup contact, behavioral political economy

JEL-Codes: D90, Z13, C99, J15

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1 Introduction

Many societies have become increasingly divided into distinct ideological groups over recent years, with political disagreement also turning into growing animosity and distrust across groups.[1] These trends in ideological and "affective" polarization can have far-reaching consequences – including in economic (e.g., McConnell et al., 2018; Duchin et al., 2023; Guirola, 2023) and social spheres (e.g., Chen and Rohla, 2018; Dimant, 2023) – and endanger societal cohesion as well as the functioning of democracy itself (Iyengar et al., 2019).

Social interactions are commonly believed to play a pivotal yet double-edged role in shaping polarization. On the one hand, facilitating interactions among *contrary-minded* individuals may have the potential to counteract political polarization, for example by challenging prior views and by fostering empathy and understanding for the perspectives of ideological out-group members (Allport, 1954; Mutz, 2006). On the other hand, interactions among *like-minded* individuals may lead to a further entrenching and widening of ideological divides in society, for example through the mutual reinforcement of shared beliefs and group identities (Sunstein, 2009). These ideas have received substantial attention in the context of filter bubbles and echo chambers in online social media in recent years (e.g., Allcott et al., 2020; Peterson et al., 2021; Levy, 2021; Nyhan et al., 2023). Yet, social interactions that occur offline and in person still form an integral part of human experiences.[2] The wealth and depth of face-to-face conversations in daily life presumably point to a profound impact on individuals' political views and their beliefs and attitudes toward others. However, establishing rigorous evidence on the role of real-world in-person conversations for polarization is challenging. First, "naturally occurring" conversations are typically not randomly assigned. Second, inducing controlled variation in conversations often interferes with their unguided, open-ended, and spontaneously-flowing nature.[3]

In this paper, we overcome these challenges and study the causal impact of in-person conversations among politically like-minded versus contrary-minded partners at the individual level. To do so, we partnered with a nationwide newspaper initiative, *Germany Talks*,

[1] See, for example, Gentzkow (2016); Pew Research Center (2014); Iyengar and Westwood (2015); Ahler and Sood (2018); Ruggeri et al. (2021); Boxell et al. (2022).

[2] See, for example, Pinker (2015). Recent studies of pandemic lockdowns find that virtual interactions, e.g., through video calls, are no perfect substitute for in-person interactions when it comes to personal well-being (Stieger et al., 2023) or workplace productivity (Brucks and Levav, 2022). In-person interactions may also be more memorable, as they can be associated with a more unique profile of sensations, shared experiences, and related memories (Kahana, 2012; Bordalo et al., 2020a) – e.g., physical location, other people, background noises, clothes, feel of a handshake, eating cake together, etc. Moreover, non-verbal cues during face-to-face communication may facilitate neural synchronization and empathy between conversation partners (e.g., Jiang et al., 2012; Drimalla et al., 2019; Wohltjen and Wheatley, 2021).

[3] For example, important prior contributions study conversations through door-to-door canvassing (Pons, 2018; Green et al., 2003; Gerber and Green, 2000; Broockman and Kalla, 2016; Kalla and Broockman, 2020).

that aims to provide a platform for informal discussions about contentious political topics by matching pairs of strangers from all across Germany for a private face-to-face conversation on one day of the year. Importantly, the program only creates the matching between registered participants and establishes the first contact. Once connected, the participant pairs are free to arrange a meeting at a time and place of their own choosing, and talk about whichever topics they want. The conversations are neither observed nor constrained by any preimposed structure or guideline, apart from an officially communicated meeting date. We complemented the 2018 iteration of the *Germany Talks* program with a baseline survey (five days before the official date) and an endline survey (one week after the official date) to measure the effects of different conversations on three dimensions of polarization: (i) ideological polarization, defined as a shift of (political) views toward more extreme positions;[4] (ii) affective polarization, defined as negative beliefs and attitudes toward ideological out-group members; and (iii) the general perception of social cohesion in society, which relates to concepts like social and civic capital (e.g., Guiso et al., 2011; Durante et al., 2023).

To empirically identify the causal effects of in-person conversations, we isolate quasi-random variation in whether individuals who registered for *Germany Talks* (*GT*) actually had the possibility to talk with their partner. Specifically, we first exploit that many matched participants did not meet after all, which gives us a natural comparison group. While not random per se, due to potential self-selection, we further focus exclusively on a subpopulation of registered participants for whom the opportunity to have a conversation or not was out of their own hands. This arises from a particular feature of the program. After registering for *GT* and being matched into pairs, participants received an email from the organizers that included a brief introduction of the matched partner as well as a take-it-or-leave-it offer to accept the match. Contact was established if, and only if, both sides accepted. From the perspective of the person who was first to accept a match (*first-accepter*), this implies that having the chance for a conversation or not was completely contingent on the partner's decision. Our identification strategy builds on this quasi-experimental feature by restricting the analysis to these first-accepters and comparing those who were subsequently accepted by their partner (Treatment) with those who were not (Control). Thus, the only remaining source of potential selection is that partners' acceptance decision may have been influenced by certain characteristics of the first-accepter that are observable from the introduction email by *GT*. We address this concern by using the Chernozhukov et al. (2018) double machine learning approach to control for the partner's relevant information set through a large vector

[4] In some cases, the term *issue polarization* is used when investigating changes in views (see, e.g. Mason, 2015; Allcott et al., 2020). It reflects the extent to which partisans' views are in line with the average opinion of their party. Another related term is *opinion polarization* (Balietti et al., 2021).

of covariates. In summary, we identify the intention-to-treat (ITT) effect of an in-person conversation by first conditioning on being *willing* to self-select into treatment, and then conditioning on any observable characteristics that may correlate with the propensity of *actual* selection into treatment.[5]

Furthermore, we distinguish between the effects of conversations with politically *like-minded* (LM) versus *contrary-minded* (CM) partners by exploiting the fact that there was significant variation in the extent of prior disagreement across matched pairs. While the *Germany Talks* matching algorithm had the objective of pairing participants who took opposing positions on several political topics included in the registration questionnaire, it was constrained by the available partner pool within an acceptable geographical proximity (20km radius). Hence, in practice, many of the participants were matched with partners who already held mostly similar political views. We split the pool of first-accepters into two subsamples based on ex ante political distance: those who were matched with like-minded partners (N^{LM}= 775) and those who were matched with contrary-minded partners (N^{CM}= 748), as defined by the number of political topics from the registration questionnaire on which they disagreed. Within each subsample, we then assign individuals into treatment or control group, as explained above, and estimate the ITT effects of having an in-person conversation separately for the LM and the CM sample.

This unique combination of unobtrusive yet exogenous variation through the *GT* program provides an ideal field setting for us to study how different dimensions of polarization are shaped by face-to-face conversations with people who either hold similar or opposing political views. Our main findings are threefold.

The first set of results concerns the effect of in-person conversations on ideological polarization. Using the distance of an individual's political views relative to the centrist viewpoint as a proxy measure, we show that conversations with like-minded partners lead to a significant increase in ideological polarization by around 15% of a standard deviation (SD). By contrast, there is no evidence that talking with a contrary-minded partner leads to either a moderating or an entrenching effect on political views. This is not driven by avoidance of contentious topics: disagreement is associated both with a higher likelihood of discussing a topic and with a longer overall meeting duration. Thus, our findings suggest that one-shot discussions across ideological groups are unlikely to lead to large immediate opinion changes.

The second set of results concerns the effect on affective polarization, i.e., animosity and prejudice toward out-partisans and people with different policy views more generally (Orr and Huber, 2020). We proxy affective polarization through a series of survey questions on beliefs

[5] It is the ITT because, in 13% of cases, participants did not meet after all despite having accepted each other. We include these cases in the treatment group to avoid potential selection issues.

and attitudes toward ideological out-group members, including negative stereotypes as well as (un-)willingness to engage in personal contact. There is little evidence that talking with a person who holds similar political views has a significant impact on beliefs about people with opposing political views. In contrast, we find that a single face-to-face conversation with a contrary-minded individual can lead to a considerable reduction in affective polarization by about 27% of a SD on average. This finding is in line with the long-standing idea that fostering positive intergroup contact can reduce animosity, e.g., by correcting exaggerated stereotypes and misperceptions (Bordalo et al., 2016; Bursztyn and Yang, 2022; Hartman et al., 2022). It may also point to the potential of long-term indirect effects on reducing ideological polarization by breaking echo chambers that arise from preferences for homophilic interactions with like-minded individuals.

The third set of findings concerns the effect of in-person conversations on beliefs and attitudes toward other members of society in general, encompassing people from across all ideological groups. Our estimates suggest that talking with a contrary-minded partner improves perceptions of social cohesion by about 20% of a SD, as measured by generalized trust and beliefs about whether people care about the well-being of others. The effects of meetings with like-minded partners are also positive, but statistically insignificant when combined into a single index measure.

Overall, our empirical results paint a coherent picture. On the one hand, in-person discussions with like-minded partners lead to an echo chamber effect of mutually reinforcing views. On the other hand, conversations with contrary-minded partners have the potential to reduce affective polarization and improve overall social cohesion, even though they do not necessarily have a direct moderating effect on ideological views in the short run. A series of robustness checks confirms that these patterns are unlikely to be driven by compositional differences, avoidance of contentious topics, or disappointment among control group participants. Instead, additional analyses using survey data on subjective meeting impressions suggest that the beneficial effects of conversations on social beliefs and attitudes are mostly driven by experiences of interpersonal contact rather than by outcomes of the political discussion. While particularly positive interpersonal experiences are associated with considerably stronger reductions in affective polarization, any beneficial effects may be nullified or even reversed by negative experiences. By contrast, the polarizing effect of like-minded conversations on ideological views seems to be present irrespective of how positively the partner and the discussion were perceived. Thus, our study points to the importance of creating (physical) space and opportunity for interpersonal contact and civil discussion across different political groups, as well as the troubling prospects of growing geographical clustering of residents by political opinions (Bishop, 2009; Brown and Enos, 2021).

Related literature. Our paper broadly relates to three interconnected strands of literature. First, we contribute to research exploring and evaluating interventions against political polarization. For example, a recent megastudy by Voelkel et al. (2023) tests the effectiveness of 25 short interventions (e.g., reading a text, watching a video, or answering a specific set of questions) that were designed to reduce animosities between parties, but none of these pair contrary-minded strangers for an in-depth discussion.[6] A number of closely related studies that match pairs of out-partisan strangers for video- or text-based conversations on a predefined topic find significant improvements in how Americans feel and react to out-partisans in general (Santoro and Broockman, 2022; Combs et al., 2023; Rossiter, 2023; Rossiter and Carlson, 2023). We advance the literature by providing causal evidence on the impact of open-ended face-to-face conversations that are neither guided nor observed and take place in a natural environment – thus mimicking many features of naturally occurring conversations.[7]

Building on our study, a second paper on the effects of *Germany Talks* by Blattner and Koenen (2023) evaluates the 2021 iteration of the program, which was only conducted online (due to the Covid-19 pandemic) but otherwise mostly similar in format as the 2018 iteration we study.[8] Reassuringly, they confirm our finding that talking with contrary-minded partners can help reduce affective (but not ideological) polarization, using an alternative identification strategy – based on variation in the timing of the endline survey and the conversations – and additional outcome measures. Thus, our studies jointly demonstrate the benefits of in-depth conversations with ideological out-group members that include extensive discussions about political issues of contention. This is consistent with Rossiter (2023) but contrasts with Santoro and Broockman (2022), who only observe significant effects when out-partisans were instructed to talk about a non-political topic. Compared to Blattner and Koenen, we are able to also study conversations with like-minded (rather than just contrary-minded) partners and show that effects are asymmetric. Furthermore, the conversations took place in person rather than via video call, which can allow for richer modes of non-verbal interaction

[6]Further related interventions use priming of national identity (Levendusky, 2018), correction of misperceptions (Voelkel et al., 2021), meditation (Simonsson and Marks, 2020), making out-party friendships more salient (Voelkel et al., 2021) or narrative writing (Warner et al., 2020). Balietti et al. (2021) show that exposure to an essay written by an ideological out-group member can lead to a convergence of political opinions, in particular when individuals share many non-political attributes.

[7]See Amsalem et al. (2022) for an observational study using self-reported contact. Brown (2022a,b) provides evidence on aggregate neighborhood effects on political preferences and engagement. In the legislative context, several studies document effects of parliamentary seat neighbors on politicians' voting, speech, and co-sponsorship behavior (Saia, 2018; Harmon et al., 2019; Lowe and Jo, 2021). Our paper also broadly relates to research on deliberative democracy (Habermas, 1984; Gutmann and Thompson, 2009) and deliberative polls, where ordinary citizens are invited to "mini-public" gatherings to engage in structured and moderated group deliberations (Levendusky and Stecula, 2021; Fishkin et al., 2021; Schkade et al., 2007).

[8]Another notable difference is that the 2021 iteration introduced a "rolling system", which allowed participants to get matched and have conversations repeatedly from May until the federal election in September.

and higher memorability (see footnote 2).

Second, the paper contributes to the large literature that builds on the contact hypothesis by Allport (1954). Meta-analyses by Paluck et al. (2019) and Pettigrew and Tropp (2006) find that intergroup contact can generally help reduce prejudice and hostilities toward outgroup members, although Paluck (2016) highlights the paucity of causal evidence using real-world interventions with adults. Recent studies include, for example, Rao (2019) and Lowe (2021), who evaluate the effect of social interactions between different castes in India, as well as a number of studies on contact across ethnic, racial, or religious groups (e.g., Schindler and Westcott, 2021; Scacco and Warren, 2018; Finseraas and Kotsadam, 2017; Corno et al., 2022; Carrell et al., 2015; Boisjoly et al., 2006). Our study combines the emphasis on natural real-world social interactions in the intergroup contact literature with the above-mentioned literature stream on polarization and segregation based on political ideology.

Finally, we contribute to research investigating echo chambers and one-sided exposure to like-minded information sources, mainly in the context of (social) media. For example, Allcott et al. (2020) show that the deactivation of Facebook leads to a reduction of ideological but not affective polarization, while Levy (2021) finds the mirror-inverted effect when studying exposure to counter-attitudinal news on Facebook. In contrast, Bail et al. (2018) find a backlash effect when being confronted with opposing views on social media. A series of large-scale studies in collaboration with Facebook around the 2020 US election documents large ideological segregation in user exposure to social media posts but overall little evidence for effects on polarization outcomes (Guess et al., 2023b,a; Nyhan et al., 2023).[9] More generally, Boxell et al. (2017) argue that internet use alone cannot explain the rise in political polarization in the US. We contribute to this literature by extending the analysis from (social) media to in-person conversations. Our results document the polarizing effect of talking with people who hold similar political views, while also showing that conversations with contrary-minded people do not necessarily lead to a convergence of views in the short run. This points toward asymmetric responses in the social exchange of beliefs, potentially due to confirmation bias (Glaeser and Sunstein, 2009; Oprea and Yuksel, 2022).

The remainder of the paper is structured as follows. In Section 2, we introduce the intervention *Germany Talks* and our study design. Section 3 describes the empirical strategy. Our main results on the effects of in-person conversations on polarization are presented in Section 4, while Section 5 evaluates potential underlying mechanisms. Section 6 concludes with a discussion on external validity and avenues for future research.

[9] Other related studies include Pariser (2011); Gentzkow and Shapiro (2011); Prior (2013); Flaxman et al. (2016); Halberstam and Knight (2016); Martin and Yurukoglu (2017); Sunstein (2018); Beam et al. (2018); Eady et al. (2019); Peterson et al. (2021); Di Tella et al. (2021).

2 Setting

2.1 Background

This study focuses on in-person conversations that took place within the scope of the *Germany Talks* initiative in 2018, during a time period of growing social and political divides in Germany.

Political situation. In the wake of the 2015/16 European migrant crisis, the recently founded far-right populist party Alternative für Deutschland (AfD) rapidly gained in popularity, fuelled by anti-establishment sentiments and controversies around the welcoming stance toward asylum seekers that was taken by Chancellor Angela Merkel and her "Grand coalition" government composed of the traditionally largest two parties in the German multi-party system – the centre-right Christian Democratic Union (CDU) and the centre-left Social Democratic Party (SPD). In the 2017 federal election, the AfD received the third-highest vote share of all parties (12.6%), thereby becoming the largest opposition party upon their first entry into the German federal parliament, and the first radical right-wing populist party since WWII to be a major political force in Germany.[10] This came as a shock to many observers and as a threat to established parties, especially since Germany had often been seen as a role model of political moderation and depolarization (Munzert and Bauer, 2013; Boxell et al., 2022). Animosity between partisans was also perceived to be rising to alarming levels, even exceeding aversion based on nationality (Helbling and Jungkunz, 2020). This prompted the federal president of Germany, Frank-Walter Steinmeier, to state in his 2018 Christmas address: "Wherever you look – especially on social media – we see hate; there is shouting and daily outrage. I feel that we Germans are spending less and less time talking to each other. And even less time listening to each other.".

Germany Talks. The *Germany Talks* program ("Deutschland Spricht", henceforth *GT*) was first initiated in 2017 by Germany's largest weekly newspaper, DIE ZEIT, as a response to the contemporary political situation in Germany. The purpose of *GT* is to enable interpersonal conversations across political camps to counteract the perceived trends in ideological and affective polarization. The idea is simple: participants who sign up to the program are matched to each other based on their political views and then encouraged to meet in person with each other on a predetermined date. For example, in its second iteration in 2018, about

[10] The AfD was first founded ahead of the 2013 federal election in Germany as a moderately right-wing party by Euro-sceptic intellectuals, but with a vote share of 4.7% it fell short of the 5% minimum electoral threshold and thus failed to secure any seats in the federal parliament. Its subsequent success has been attributed to a rightward shift in its agenda that tapped into anti-immigration and anti-establishment attitudes (Arzheimer and Berning, 2019; Hansen and Olsen, 2019).

4200 pairs of participants met across Germany on September 23 to engage in face-to-face discussions on topics ranging from immigration to the #MeToo movement. Since its foundation, it has established itself as an annual initiative with thousands of people meeting and talking to each other each year. Although its roots lie in Germany, the *My Country Talks* program (www.mycountrytalks.org) has since expanded to other regions and countries all over the world, among others the US (*America Talks*) and Europe (*Europe Talks*). Overall, at the time of writing, the program has matched more than 90,000 pairs of participants from over 100 countries.

2.2 Study design

We study how in-person conversations initiated by the 2018 *GT* program affected participants' political views (ideological polarization) as well as their beliefs and attitudes toward people with opposing political views (affective polarization) and other members of society in general (perceived social cohesion). To do so, we partnered with the *GT* organizers and complemented the program by sending out a baseline and endline survey to all participants. Figure 1 provides an overview of the overall study design.

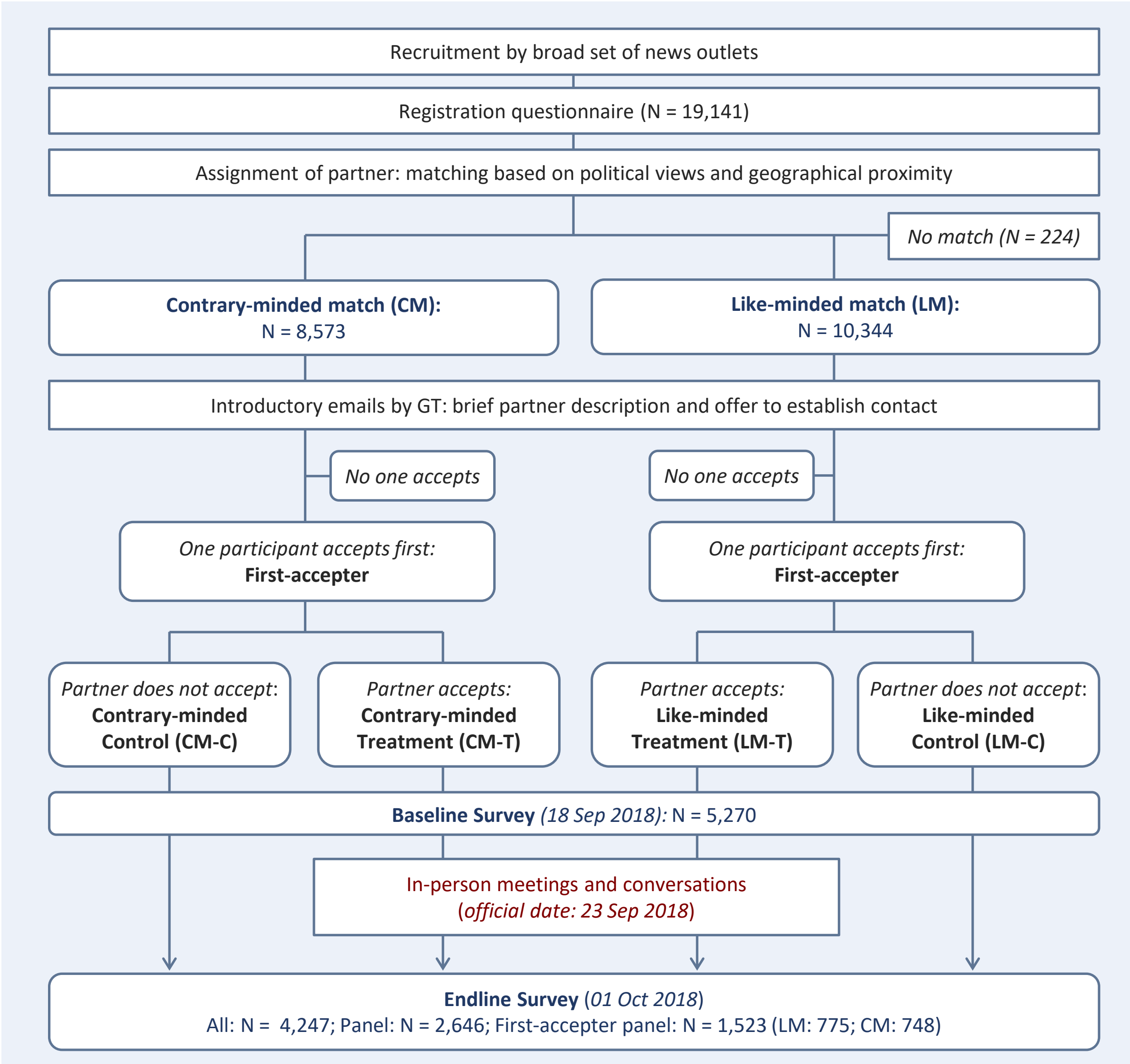
Recruitment and registration. In 2018, *GT* was conducted in cooperation with a broad set of German news outlets. Together, the participating partners had considerable outreach ranging from large daily and weekly newspapers and their online platforms, over pure online media to major public television. With respect to political orientation, the participating news outlets reflected a broad political spectrum with a focus on the center-left (Pew Research Center, 2018). The intervention was promoted on these platforms and participants could register either online on the respective websites or via ordinary mail.

In order to register for the program, individuals had to answer seven binary political registration questions on a range of topics that were chosen carefully by the organizers to capture contemporary political controversies, ranging from immigration and border controls to climate policy.[11] In addition to these questions, applicants had to state their name, age, gender, place of residence and answer five non-political free-response questions to briefly describe themselves.[12] Our data set includes 19,141 participants who were successfully recruited and completing the registration questionnaire.

[11]The translated questions are listed in the following in no particular order: (1) Can Muslims and non-muslims coexist in Germany? (2) Did the public debate about sexual harassment and #metoo have any positive effects? (3) Should meat be taxed higher in order to reduce its consumption? (4) Should German city centers become car-free? (5) Should Germany implement stricter border controls? (6) Are Germans worse off today than 10 years ago? (7) Is Donald Trump good for the US?

[12]The five open questions were: What do you do for a living? You are a friend of....? What do you do in your free time? How would you describe yourself? What are your dislikes?

Figure 1: Quasi-experimental setting



Matching. After registration, people were assigned a partner based on their political views and place of residence. The main objective of the algorithm was to match as many participants as possible while fulfilling two conditions, with the first being that the matched partner had to be located within a 20-kilometer perimeter, and the second being that political distance (defined as the number of differently-answered political registration questions) between partners is maximized. Hence, the available partner pool within an acceptable geographical proximity constituted a limiting constraint to the extent of contrary-mindedness within possible participant pair matches. The algorithm was executed exactly once: there was no chance of changing partners or being matched to another partner later on. This ensures that the type of the matched partner remains exogenous to each participant's own preferences.

Variation in political distance: like-minded vs. contrary-minded. To investigate how the impacts of face-to-face discussions vary depending on the political alignment of the conversation pair, we divide participants into two subsamples based on their political distance from their matched partners: (i) the *contrary-minded (CM)* sample, which includes the 46% of participants whose partner answered more than half (i.e., four or more) of the political registration questions differently; (ii) the *like-minded (LM)* sample, which includes the 54% of participants whose partner answered less than half (i.e., three or fewer) of the political registration questions differently. This precise distinction between LM and CM was constructed by us – they played no role in the matching process and participants were unaware of the classification.

Note that, in the context of the German multi-party system, it is less natural and straightforward to divide participants along party lines compared to in the US, where partisan identity is regularly used to define in-group and out-group (e.g., Ehret et al., 2022; Dimant, 2023). Moreover, we do not always have information on the partner's party preference, as this was elicited by us in the baseline survey, which was often completed only by one participant in a matched pair. Therefore, we divide individuals along ideological lines based on their responses to the political registration questions, which every GT participant had to complete. Throughout the analyses, we will also show that the results are robust to alternative definitions of like- versus contrary-minded partners based on a latent class analysis.

Introduction email by *GT*. Each successfully paired individual received an email introducing the matched partner. This email contained a list of the political registration questions that the partner had answered differently, the partner's first name, age, gender and the answers to the non-political free response questions. Based on this information, the participants could decide whether they wanted to accept the suggested partner or not. As soon as one participant within a pair accepted, the other person was notified. Contact was established via exchange of email addresses if, and only if, both sides accepted. If one side did not accept by the officially communicated date for the meeting (23 Sep 2018), no contact was established, no new matches were generated, and no meeting took place.

Variation in meeting availability: treatment vs. control. While virtually all registered participants could be matched by the algorithm into pairs, not all the matched pairs accepted the invitation by GT and actually managed to meet. To circumvent self-selection into meeting or not meeting, we restrict our analysis to those participants who accepted their partner first. Thus, the decision of the second-moving partner essentially determined whether the first-accepter would have the opportunity for a meeting or not. We exploit this feature by defining treatment and control groups in the following way: *treated partici-*

Table 1: Overview of participant assignment into groups

	Like-minded sample (LM)	**Contrary-minded sample (CM)**
Meeting (Treated)	First-accepters, assigned to a like-minded partner who accepted as well. ($n = 514$)	First-accepters, assigned to a contrary-minded partner who accepted as well. ($n = 455$)
No meeting (Control)	First-accepters, assigned to a like-minded partner who did not accept. ($n = 261$)	First-accepters, assigned to a contrary-minded partner who did not accept. ($n = 293$)

Notes. This table summarizes the different subgroups of first-accepters in our study. Participants were either matched to a like-minded partner (LM) or a contrary-minded partner (CM), see columns. Contact was established to arrange a meeting depending on whether the partner accepted the match (treatment) or not (control), see rows.

pants are those first-accepters whose partners also accepted the match invitation by *GT*, in which case email contact was established and the partners could arrange a time and place for their in-person meeting; *control participants* are those first-accepters whose partners did not accept, in which case no contact was established and there was no chance of meeting or communicating with the partner. Table 1 summarizes the assignment of participants into treatment group versus control group (due to meeting availability) and into like-minded sample versus contrary-minded sample (based on political distance of the matched partner).

There are two key takeaways here for our empirical analysis. First, by restricting our analysis only to first-accepters, we eliminate any scope for self-selection into treatment or control group. Second, after receiving the notification about their first-moving partner accepting the match, the second-moving partners' acceptance decision could not be influenced by any first-accepter characteristics beyond what is visible from the brief introduction email sent by *GT*. Thus, even if the second-mover's acceptance decision was endogenous to the matching outcome, it can be treated as conditionally exogenous once controlling for information from the introduction email. Section 3 will discuss our identification strategy and balance between treatment and control conditions in more detail.

Meetings. After contact had been established, the organizers of *GT* played no further role and participants had to organize the exact time and location of the meetings themselves. The officially communicated date for all meetings was September 23, 2018, and 89% of treated participants in our sample reported that they indeed met on that day. The conversations were not observed, moderated, or guided in any way. They mostly took place in natural settings like cafés, parks, or in people's homes and lasted for more than 2 hours on average. More details will be provided below in Section 2.4.

Surveys. To complement the program, we designed a baseline survey and endline survey that were sent out by the organizers of GT to all registered participants, independent of whether they accepted each other or not. The baseline survey was sent out on September 18 five days prior to the meetings and required on average 14 minutes to answer. It included questions on socio-demographics, expectations about the conversation, as well as baseline measures of political views and beliefs and attitudes toward different members of society. The endline survey was sent out on October 1, one week after the officially suggested conversation date, and required on average 12.5 minutes to answer. It contained questions about the program and meeting itself – whether they had one, and, if yes, what was discussed and how they perceived the experience – as well as the outcome measures, which will be described in more detail in Section 2.5. In total, 2,645 participants completed both the baseline and the endline survey.

Ideally, the baseline survey would have been distributed before participants could learn about their partners and before first contact can be established to arrange the meetings. Unfortunately, the survey was sent out only on September 18 by the organizers, more than one week after the introductory emails had been sent. In principle, participants had time to accept each other until the official meeting day (September 23). In practice, however, most acceptance decisions and assignments to treatment (acceptance decision of the partner) had already taken place before participants completed the baseline survey. In fact, by that point in time, 98% of the treated participants had already learned that the partner had also accepted. Consequently, measures that were elicited in the baseline survey may potentially be affected by information about the matched partner from the GT introduction email as well as mutual acceptance and first email communication with the partner (to arrange the in-person meeting). For this reason, we only use measures from the baseline survey that are unlikely to be influenced by the first contact (such as political ideology and time-invariant characteristics). We do not use any potentially endogenous sensitive "social measures" like stereotypes or perception of social cohesion for our main results. However, we will provide robustness checks demonstrating that our findings are largely robust to including these potentially endogenous baseline survey measures.

2.3 Sample composition

Appendix Tables A1 and A2 present descriptive statistics on the composition of the full estimation sample of first accepters who responded to both our surveys (N = 1,523), as well as the like-minded (N^{LM}= 775) and the contrary-minded subsamples (N^{CM}= 748). Participants resemble the general German population with regard to age distribution and state of residence, although urban regions (e.g., Berlin, Hamburg) are overrepresented. The female share in our sample is 37%, possibly reflecting gender gaps in political interest (Fraile and Gomez, 2017) or in feeling comfortable with meeting a stranger. Participants are also on average more educated (67% university degree), less likely to have a migration background (10%), and less likely to have low income (23% below 1,500 Euro per month) compared to the German population. Finally, participants tend to be strongly politically interested (only 2% non-voters) and left-leaning in their political ideology (69% to the left of the center) and party preference (only 7% AfD). Overall, the composition of our sample likely results from reader-/viewership of the participating news outlets – which lean center-left –, as well as from selection into participating in the program. This is not necessarily a drawback, as voluntary participation is a general constraint on interventions like *Germany Talks* that cannot "force" engagement.

The LM and CM subsamples are generally comparable in terms of demographic and socio-economic characteristics, with the exception of gender composition (42% female in LM and 32% in CM). Furthermore, the like-minded sample is generally less conservative in its political ideology, which results mechanically from the “excess supply” of registered *GT* participants that are politically left-leaning: the matching algorithm aims to maximize the political distance between partners and therefore tends to achieve in finding contrary-minded partners for right-leaning participants, whereas many left-leaning participants could only be matched with other left-leaning partners. This also partly explains differences in the gender composition, as female participants in our sample were generally less conservative than male participants.

In addition to self-reported political identity, we further conduct a latent class analysis (LCA) that assigns participants to political ideology classes based on the correlational patterns of their answers to the seven political questions in the registration questionnaire – for details, see Appendix A.3. The LCA suggests that participants can be broadly assigned into two distinct ideological groups: a large leftist camp (83%) and a small rightist camp (17%). One advantage of this approach is that it can be applied to all participants of *GT*, irrespective of whether they responded to our baseline survey or not. This means that we can also compare (mis-)alignment of political camps within each participant pair – even if we do not have baseline survey data from both partners. Appendix Table A3 shows that 97% of participants in LM were matched to a partner from the same ideological class and that 74% of participants in CM were matched to a partner from the different class, thus validating our definition of conversations with like-minded versus contrary-minded partners. Throughout our analyses, we will also present robustness checks using latent class to directly define LM and CM instead.

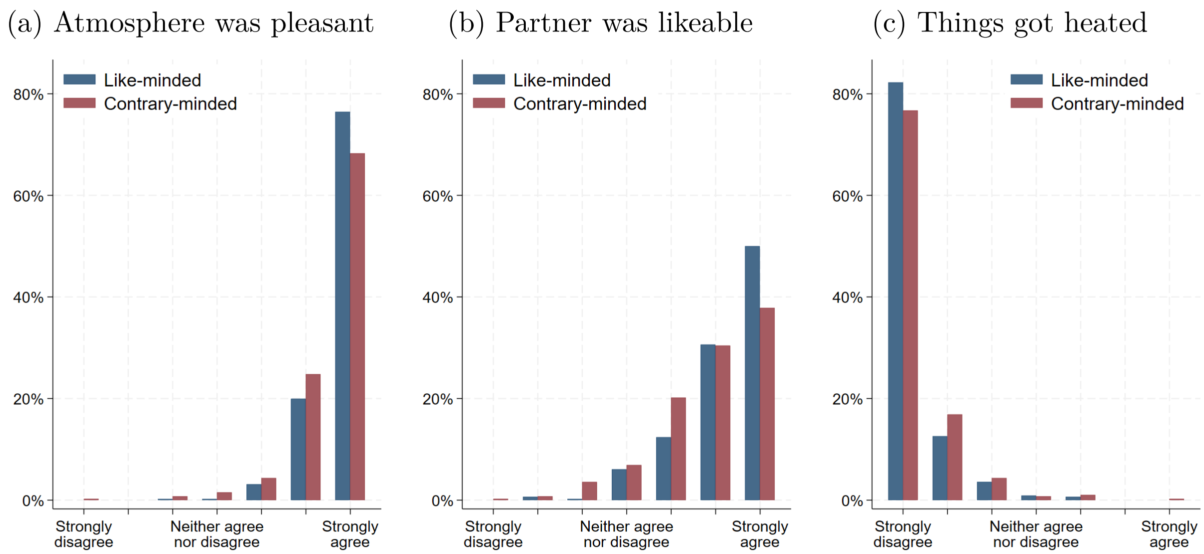
2.4 In-person conversations

Once both participants accepted the match, contact was established through *GT*. Participants were then free to communicate with each other via email and organize the in-person meeting at an exact time and location of their own choosing. The officially communicated date for the meetings was September 23, 2018, and 89% of treated participants who met with each other reported that their meeting indeed occurred on the official date, while 96% reported meeting within a 4-day window around that date. The in-person conversations were held privately; no third-party moderator or researcher was observing or guiding the discussion, and no rules or topics were predefined.

What were the participants' expectations and aims going into the meeting? Based on baseline survey responses, most participants seemed to instinctively understand the importance of listening to their counterparts rather than just talking about their own position (see Appendix Figure A1). While 81% agreed that they hoped to learn something about their partner's views, only 20% expressed an explicit desire to convince them of their own view. Note that the baseline survey was sent out before the conversation, but after matches were announced, so expectations could already be adjusted based on information about the partner at that point. For example, we observe that participants with contrary-minded matches were slightly more cautious about trying to learn about the other point of view, although still very optimistic overall.

To shed more light on what happened during the meetings, the endline survey included questions about topics that were discussed and subjective impressions about the conversation and the overall experience of taking part in *Germany Talks*. Based on survey responses, meetings lasted on average about 140 minutes and mostly took place in natural settings like cafés, parks, or people's homes. Even the shortest reported meeting in our sample had a duration of 40 minutes, thus suggesting that participants took considerable time to get acquainted with each other and discuss their viewpoints. Participants overwhelmingly rated their conversation as a positive experience. Figure 2 plots the distribution of participants' subjective perceptions of the meeting with their partner from the endline survey, separately for the LM sample and the CM sample. 95% of all participants stated that the atmosphere was pleasant, and only about 1% reported that there were loud and heated disputes during the conversation. Furthermore, 75% of participants found their conversation partner to be likeable overall, whereas only about 2.6% disagreed with the statement. Importantly, we observe little difference in the probability of reporting a negative experience depending on whether participants talked to someone with similar or opposing views. Although individuals who were matched with a like-minded partner found the meeting to be somewhat more enjoyable and their partner more likeable on average, these differences are concentrated at

Figure 2: Perceived meeting experience



Notes. Figure plots the distribution of subjective meeting experiences among treated participants based on the endline survey that was sent out 1 week after the meeting. Participants were asked to rate their (dis-)agreement to a number of statements on a seven-point Likert scale. Only participants who met with their partner received this set of questions.

the positive end of the distribution. Hence, it is unlikely that average treatment effects of contrary-minded conversations are heavily influenced by potential acrimony or antipathy.

Although participants did not have to follow any preimposed structure or guidelines, we find that the conversations generally touched on some of the political topics that were introduced in the registration process. This may be partly due to participants using these as suggested starting points, and partly due to the organizers choosing topics that capture the most salient contemporary political controversies. The most discussed topics were stricter border control (53%) and car-free inner cities (52%), whereas the least discussed topic of the *political registration questions* was whether Germans are worse off today than ten years ago (33%), see Appendix Figure A2. Moreover, the likelihood of discussing a topic is generally higher if the pair disagreed on it in the registration questionnaire than if they agreed (see Appendix Figure A3). This suggests that the meetings were seen as a platform for earnest discussion about contentious social and political issues. Did participants have the impression that these political discussions resulted in a shift in opinions? While the majority believed that they had learned something about their partner's point of view (88%) and that their respective views have somewhat converged (60%), their feelings were more mixed about whether either side had truly managed to convince the other by the end of the meeting (33%) – see Appendix Figure A4.

2.5 Outcome measures

We evaluate the effect of in-person conversations through GT on various attitudes and beliefs that we elicited in the endline survey. These can be linked to three broad categories of societal outcomes: (1) ideological polarization, (2) affective polarization, and (3) social cohesion. Table 2 provides a detailed overview of the outcome measures we use, and Appendix A.3 contains detailed information on how variables are constructed. All outcome measures are standardized within each subsample by subtracting the respective control group means and dividing by the control standard deviations.

To study the role of conversations with like-minded or contrary-minded individuals for ideological polarization in political views, we elicited agreement with eleven different political statements in the baseline and endline survey (on a 7-point Likert scale). These include seven topics chosen by GT for the registration process to match conversation partners, such as border controls and the #MeToo movement, as well as four additional topics that we introduced to capture other contentious topics with a salient left-right divide, such as same-sex marriage and redistributive taxation. Table 2 provides an overview of the topics. Statements were deliberately formulated in a simplistic and provocative manner. We summarize the overall political ideology as the vector of all eleven opinions in order to construct two measures of the extremeness of overall political views, as a proxy for ideological polarization. The first measure defines extremeness as (Euclidean) distance to the *absolute* centrist viewpoint, i.e., the center of the answer scale. This reflects the general strength of or conviction about one's views. The second measure defines extremeness as (Euclidean) distance to the *relative* centrist viewpoint, i.e., the average (pre-meeting) viewpoint in our sample. This reflects the extent to which an individual's opinion departs from the political center in Germany.

To assess the role of conversations for affective polarization, we measured beliefs and attitudes about generic contrary-minded persons. Importantly, we did not ask participants about their conversation partners, but instead asked them to imagine a generic person who holds opposing political views. The beliefs about ideological out-groups we elicited consist of stereotypes and prejudices that were communicated by former participants of GT, namely that contrary-minded individuals are (cognitively) incapable of understanding complex contexts, poorly informed, have different moral values, and lead completely different lives than oneself. To reduce dimensionality, we construct a summary measure of negative stereotypes by implementing a principal component analysis (PCA) and extracting the first component (see Table A23). Furthermore, we measured attitudes toward ideological out-groups by eliciting the willingness to engage in close interpersonal contact with people who hold opposing political views. To construct an overall index variable of affective polarization, we run a PCA on the four negative stereotype measures as well as willingness to engage in personal

Table 2: Outcome Variables

Variable	Statement
Political views (ideological polarization)	
Topics from the registration questionnaire	
Coexistence	Muslims and Non-Muslims can coexist in Germany.
#metoo	The public debate about sexual harassment and #metoo had some positive effects.
Tax Meat	Meat should be taxed higher in order to reduce its consumption.
Car-free City Centers	German city centers should be car-free.
Border Control	Germany should implement stricter border controls.
Germans worse off	Germans are worse off today than 10 years ago.
Trump	Donald Trump is good for the USA.
Additional topics in baseline and endline	
Same-Sex Marriage	Marriage should only be allowed between a man and a woman.
Cooperation within EU	Germany should deepen its cooperation with other EU countries.
Income Tax	To reduce the gap between rich and poor, the tax rate for top earners should be increased.
Trustworthiness Media	Altogether, German media are trustworthy.
Beliefs/attitudes toward ideological out-group (affective polarization)	
Overall Stereotype	
Cognitive Abilities	This person is incapable of understanding complex contexts.
Poorly Informed	This person is poorly informed.
Moral Values	This person has completely different moral values.
Way of Life	This person leads a completely different life.
Willingness to engage in personal contact	I would like this person to be in my personal environment. (rev.)
Perception of overall social cohesion	
Generalized trust	One can trust most people in Germany.
Perceived prosociality	Most people in Germany do not care about the well-being of others. (rev.)

Notes. The table shows all elicited variables that we use to construct our outcome measures. *Overall Political Opinion* is a vector consisting of the eleven single political views. Out of this vector, we construct both ideological polarization measures. See Section 4.1 for more details. *Overall Stereotype* is the first principal component of a PCA of all four stereotypes as detailed in Section 4.2. To elicit the affective polarization measures, we asked participants to picture some person who gave *very different* answers to the seven political attitude questions. The last column shows the corresponding scales. Some variables, denoted by (rev.), are reversed for interpretational reasons. Participants had to state their agreement to the statements (political attitudes, perception of social cohesion) and the extent to which they apply (stereotypes) on seven-point Likert-Scales.

contact (see Table A24).

Finally, we elicited two measures of beliefs about other members of society, referring both to those who hold similar political views and those who hold different political views: (1) how trustworthy fellow citizens are in general, and (2) the extent to which German citizens care about the well-being of others. These are aimed at capturing perceptions of overall social cohesion in Germany and also relate to certain facets of social and civic capital (Putnam, 2000; Guiso et al., 2011; Durante et al., 2023). We create a single summary measure of overall perceived social cohesion by combining responses to the generalized trust and the perceived prosociality questions (with equal weights) into a single index variable.

3 Empirical approach

Identification strategy. Not all registered participants who were matched through GT could meet with their partner in the end. In principle, this gives us a natural comparison group to examine the effects of in-person conversations among participants who actually had the opportunity to meet with their partners. However, even conditional on registering for the program, there is likely self-selection into being willing to stick with the initial decision to participate and accept the meeting once being informed about their match.

Our identification strategy exploits a quasi-experimental feature of the program that allows us to eliminate self-selection effects by focusing on a subset of registered participants who were all willing to meet their partner, but for whom the opportunity for an actual meeting was out of their own hands. Recall that once the algorithm had generated the matched pairs, participants received a notification email from the GT organizers that included a brief introduction of their matched partner as well as a take-it-or-leave-it offer to accept the match. Contact between the participants was only established once both sides accepted each other. Hence, from the perspective of the *first-accepter*, i.e., the first participant in a pair who accepts their match, assignment into treatment (meeting opportunity) or control (no meeting opportunity) is as good as random, as it completely depends on whether their exogenously matched partner subsequently chose to accept them or not. Hence, restricting the analysis to the subpopulation of first-accepters eliminates any *self*-selection effects.

One remaining source of potentially systematic selection is that second-moving partners may condition their decision on information about the first-accepter that is visible to them from the introduction email, which includes the first name, age, gender, a list of the political registration questions that they had answered differently, and the answers to the non-political free response questions. Thus, after controlling for a suitable set of covariates to capture the second-mover's information set, treatment assignment of the first-accepter should be conditionally independent of their potential outcomes. While we are able to observe age, gender, region, and answers to the political registration questions, we did not receive access to names and answers to the non-political free response questions from the organizers of GT due to data protection concerns. Therefore, we use a host of supplementary participant characteristics that we elicited from the baseline survey to proxy for any relevant information that may have been conveyed through the name and the free responses.

In summary, our identification approach identifies the effect of an in-person conversation on individual-level polarization outcomes by first conditioning on being *willing* to self-select into treatment, and then controlling for observable characteristics that may correlate with the propensity of *actual* selection into treatment. Note that in 13% of cases, matched pairs did not manage to meet despite having accepted each other. This may be due to illness or other external factors, but as we have no detailed information that could explicitly alleviate concerns about selection effects, we include these participants in the treatment group and estimate intention-to-treat (ITT) effects instead of treatment effects on the treated. This implies that our estimates are likely to provide a lower bound, but as the compliance rate is relatively high, the ITT estimates can be expected to be close to the real average effect of actual in-person conversations in our sample. Importantly, non-compliance is one-sided by construction. Contact was only established if both partners had accepted each other, so participants in the control group had no chance of meeting their partner and compliance rate was thus 100%.[13] Furthermore, there is no evidence for differential compliance rates among treated participants in the LM sample (87.2%) and the CM sample (86.8%).

Estimation. We implement our identification strategy by estimating ITT effect of in-person conversations on first-accepters (separately for the LM and the CM sample) based on the following partially linear statistical model:

$$Y_i \;=\; \alpha \,+\, \beta\, T_i \,+\, g(\mathbf{x}_i) \,+\, \rho\, Y_i^b \,+\, \epsilon_i\,. \tag{1}$$

Y_i denotes the outcome variable of interest from the endline survey (see Section 2.5). The treatment variable T_i indicates whether a conversation has taken place ($T_i = 1$) or not ($T_i = 0$), which in turn is determined by whether the first-accepter i was accepted by the partner. Thus, β is our coefficient of interest that represents the average ITT effect of a political face-to-face discussion. Y_i^b denotes the baseline value of Y_i, which we include in some specifications to adopt for a difference-in-differences setup.[14] ϵ_i is the individual-specific error term.

To address the concern that the second mover's decision to accept or reject the meeting may have been influenced by certain first-accepter characteristics that are observable from the introductory mail sent by GT, we additionally consider a partially linear function $g(\mathbf{x}_i)$ of individual-level covariates to try and capture the partners' information sets. Firstly, the vector of control variables $\mathbf{x}_i$ contains basic information (hard facts) about participant i that

[13]There were two participants who stated in the endline survey that they had a meeting even though the partner did not accept them. We do not know whether this was an accidental misclick or an intentional misrepresentation. We drop them from our analysis, although including them does not change our results.

[14]Ideally, we would want to do so for all our analyses. However, responses in the baseline survey that are related to beliefs and attitudes about others are likely to be endogenous, as contact had already been established between almost all treated participant pairs by that time. For more details, see Section 2.2. Hence, Y_i^b excludes the baseline values for the measures of affective polarization and perception of social cohesion. Nevertheless, we will present robustness checks in which we include these potentially endogenous baseline measures.

we observe (age, gender, region of residence, combinations of answers to political registration questions), and proxies for surname (migration background, and education and income). Secondly, we consider an additional set of covariates accounts for the fact that the answers to the open questions were unobserved by capturing potentially visible information. It comprises political self-classification, party, political engagement, religion, religiousness, marital status and the number of politically contrary-minded people in one's social environment. Appendix A.3.1 describes the controls in more detail.

Moreover, to take into account that second-movers may react to certain combinations of traits and views, we further construct a broad set of interactions between different first-accepter characteristics. Considering this high-dimensional covariate vector necessitates the selection of a limited subset of controls to include in the final regression model. We follow a hands-off approach by implementing the "double" machine learning (DML) method proposed by Chernozhukov et al. (2018), a data-driven procedure for choosing a relevant set of control variables to achieve conditional independence while also obtaining asymptotically valid standard errors.[15] In addition to the DML estimates, we will report robustness checks for our main results based on ordinary least squares (OLS) regressions that include non-zero coefficients for all participant characteristics but severely restrict the number of potential interaction terms. The point estimates are generally similar, but standard errors tend to be higher when using OLS instead of DML, presumably due to the inclusion of irrelevant covariates as well as the non-inclusion of variance-reducing interaction terms.

Identifying assumptions. As we restrict our analysis to first-accepting participants whose treatment status is fully contingent on choices made by their exogenously matched partner, we can rule out any self-selection effects by design. Therefore, the main identifying assumption for our empirical approach is that, conditional on the included vector of control variables $\boldsymbol{x}_i$, treatment assignment of the first-accepter (i.e., the partner's acceptance decision) is uncorrelated to unobserved components ϵ_i of the outcome variable. This would be violated if (latent) attitudes of the first-accepter are shining through in the introductory mail in a way that is (1) not measurable by the researcher, (2) observable to the partner, (3) discerned by the partner, (4) important enough to influence the partner's acceptance decision, *and* (5) systematically related to the outcome variables of interest as measured in the endline survey. Additionally, identification could be violated if there is selective attrition (i.e., non-response to the endline survey) depending on treatment status.

[15] For the orthogonalization steps in DML, we use Lasso with tenfold cross-fitting. The reported estimates for the figures and tables in the main text are based on twenty-five repetitions of the DML estimator, each with a different (random) sample partition. This improves finite-sample robustness of the estimates but also dramatically increases computing time (for more details, see Chernozhukov et al., 2018). Results presented in the online appendix are based on ten repetitions of the DML estimator each.

Balance checks. To assess these identifying assumptions, we conduct several balance checks and empirical validation exercises. First, we check whether treated and control participants are comparable with regard to observable variables such as socio-economic characteristics and baseline political views. To do so, we conduct Logit regressions of treatment status as binary dependent variable on a host of observable characteristics elicited from the registration and baseline questionnaires. Overall, there is little evidence for large systematic differences between treated and control participants. Appendix Table A4 shows that first-accepters who were subsequently accepted versus not accepted by their partner are similar in terms of socio-economic characteristics – such as age, education, income, migration background, religion – as well as political attitudes and engagement. There are only two patterns that are significant and consistent in both the LM and the CM sample: women are more likely to be accepted than men, and people who agree that Donald Trump is good for the U.S. are less likely to be accepted. However, overall (within-sample) model performance is poor, with pseudo-R^{2}s of around 0.06, and, importantly, the F-tests of joint significance cannot reject the null hypothesis that none of the true coefficients are different from zero in the LM sample ($p = 0.390$) or the CM sample ($p = 0.331$).

Additionally, we conduct balance checks where we allow for potential interaction effects between characteristics and run Lasso regressions with tenfold cross-validation. This also helps us gain a better sense of the extent to which potential differences between treatment and control are driven by systematic influences of information from the introduction email by GT on second-movers' acceptance decisions (as opposed to random variation). Appendix Table A5 shows that out-of-sample predictive power is poor, with the out-of-sample R^2 being almost precisely zero in the LM sample and only about 1.1% in the CM sample. Note that our covariate vector captures partner characteristics that are highly salient in the introduction email by GT, for example, age, gender, migration background (through the name), and political views. Hence, these results suggest that second-movers' acceptance decisions seem to be mostly determined by factors that are unrelated to the specific partner match – such as unavailability/alternative plans for the official meeting date, or other behavioral barriers that drive a gap between intentions (i.e., registering for GT) and actions, e.g., inattention/forgetfulness, inertia, second thoughts about talking with a stranger (e.g., Epley and Schroeder, 2014; Sandstrom and Boothby, 2021; Atir et al., 2022). Throughout our analyses, we will also benchmark the main DML estimates of average ITT effects against OLS estimates without any additional control variables in $g(x_i)$. In general, we find that the point estimates are both quantitatively and qualitatively similar.

Finally, Table A6 shows that there is no evidence for selective attrition, with mean response rates to the endline survey (conditional on responding to the baseline survey) being about 49-50% for both treated and control participants, in both the like-minded and the contrary-minded samples.

4 Main results

We will proceed in three steps to present our main results on the effects of in-person conversations with like-minded versus contrary-minded individuals. First, we will look at the effects of the conversation on the extremeness of political views, as an individual-level proxy for ideological polarization. Second, we will look at the effects on negative beliefs and attitudes toward individuals who hold opposing political views, as an individual-level proxy for affective polarization. Third, we will look at effects on perceived social cohesion more generally, encompassing beliefs about how trustworthy and prosocial other members of societies are.

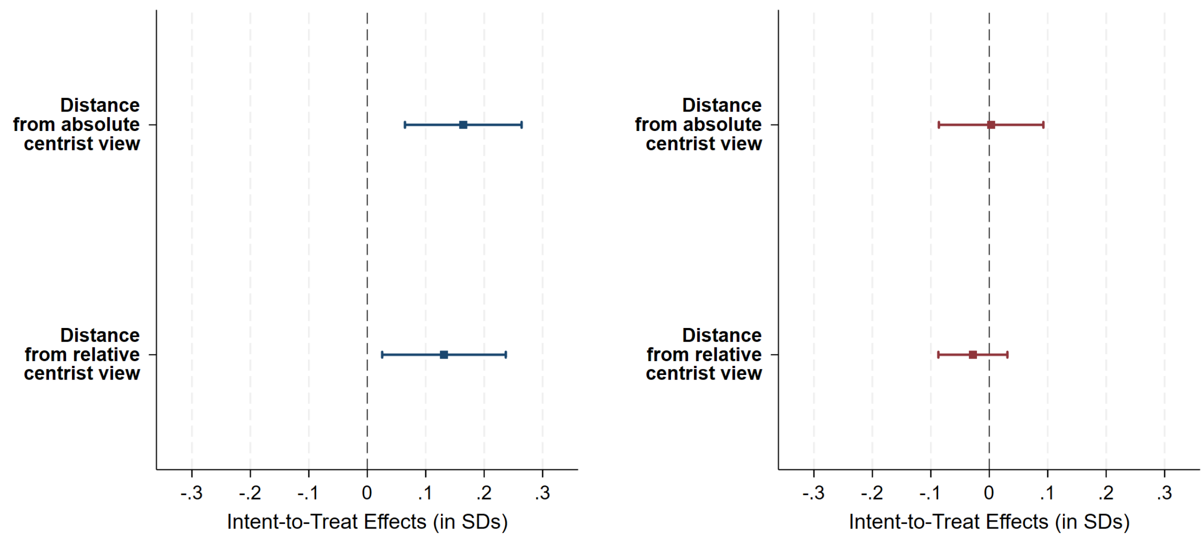
4.1 Effects on ideological polarization

Many scholars argue that deliberations among citizens lead to more agreement within society. However, there is a concern that discussions can yield the exact opposite. Homophily and echo chambers among like-minded people may lead to confirmation and reinforcement of each other's views (Sunstein, 2009), fueling ideological polarization. Even if confronted with contrasting viewpoints, it is unclear what to expect as discussions may result in a "backfire" effect (Bail et al., 2018; Wojcieszak, 2011). In this section, we explore how personal conversations with strangers impact the formation of social and political opinions, depending on whether the conversation partner holds similar or opposing views.

Outcome measures. As described in section 2.5, we elicited (dis-)agreement with eleven different political viewpoints in the baseline and endline survey, covering a range of social, economic, and political topics ranging from immigration to income taxation (see Table 2 for an overview). We operationalize ideological polarization by measuring how extreme an individual's overall opinion is in terms of distance to the centrist viewpoints on each topic, defined in two alternative ways. The first measure calculates the (Euclidean) distance to the absolute mid-point of each scale, whereas the second measure calculates the (Euclidean) distance to the relative mid-point, i.e., the average baseline opinion of the sample. All measures are standardized within each subsample using the respective control group moments. Note that the (control group) standard deviation is similar in CM and LM for the first measure of political distance, but for the second measure (distance to the sample average), the

Figure 3: Effect of in-person conversations on ideological polarization

(a) Conv. with like-minded partner (b) Conv. with contrary-minded partner



Notes. ITT effects of in-person conversations on standardized measures of ideological polarization, proxied by how extreme individuals' views over 11 political topics are in terms of Euclidean distance (a) to the absolute centrist view (mid-point of the scale), and (b) to the relative centrist view (average opinion in the sample). The outcome measures are described in Section 4.1. Estimates are based on 25 repetitions of the Chernozhukov et al. (2018) double machine learning estimator with tenfold Lasso cross-fitting and different random sample splits each. Error bars represent 95% confidence intervals.

standard deviation is about twice as high in CM relative to LM. Thus, a one unit change on an absolute scale corresponds to different unit changes in terms of SDs in each sample.

Findings. Figure 3 presents our estimates for the average ITT effects for our two measures of ideological polarization. We find that the effect of in-person conversations is asymmetric. Participants who are matched to a like-minded partner display a significant increase in extremeness of viewpoints in the endline survey, with similar estimated effects of around 13-16% of a standard deviation. In contrast, participants who meet with a contrary-minded partner do not become more moderate in their opinions subsequently, with all point estimates being insignificant and close to zero. At the same time, we also do not find any evidence for backlash effects that Bail et al. (2018) document for exposure to opposing views on social media.[16] Table 3 further shows that the asymmetry in effects of like-minded versus contrary-minded conversations is statistically significant when running the analyses in the

[16] Appendix Figure A5 plots the effects for each of the eleven political topics separately. While the estimates are naturally more noisy, the point coefficients for the LM condition are all positive, although close to zero for three items (#MeToo, car-free inner cities, and Germans being worse off). In contrast, for the CM condition, the coefficients generally fluctuate around zero in both directions. Importantly, the point estimates are on average similar for the seven questions that were saliently used for registration to the *GT*

Table 3: Effect of in-person conversations on ideological polarization

	Distance of political views from the centrist view					
	Absolute centrist view			Relative centrist view		
	(1) LM	(2) CM	(3) Pooled	(4) LM	(5) CM	(6) Pooled
Treated	0.164***	0.003	0.139***	0.131**	-0.028	0.062**
	(0.051)	(0.046)	(0.044)	(0.054)	(0.030)	(0.031)
Treated × CM			-0.121**			-0.088**
			(0.059)			(0.044)
Contrary-minded (CM)			0.024			0.076*
			(0.058)			(0.043)
Double ML	Yes	Yes	Yes	Yes	Yes	Yes
No. of selected controls	83	92	268	68	107	265
Observations	729	710	1439	729	710	1439
Like-minded sample	✓		✓	✓		✓
Contrary-minded sample		✓	✓		✓	✓

Notes. Coefficients present estimated average ITT effects on standardized measures of ideological polarization, defined as level of (dis-)agreement to the centrist view on eleven political topics. Disagreement is calculated either as Euclidean distance to the mid-point of the scale (columns 1-3) or as distance to the average view in the sample (columns 4-6). For more details, see Section 4.1. Estimates are based on 25 repetitions of the Chernozhukov et al. (2018) double machine learning estimator with tenfold Lasso cross-fitting and different random sample splits each. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

pooled sample and interacting the treatment indicator with an indicator for contrary-minded match ($p < 0.05$).

Robustness. Appendix Tables A7 and A8 demonstrate robustness to using alternative metrics (Mahalanobis distance and Manhattan distance) to construct our ideological polarization variables rather than Euclidean distance. Furthermore, the results are very similar when, instead of using the DML approach, we estimate average ITT effects using OLS with a fixed set of control variables or no control variables at all (see Appendix Tables A9 and A11). Finally, Appendix Table A13 shows that our results are robust to defining the conversation partner by alignment of ideological classes, obtained from a latent class analysis (see Section 2.3).

Interpretation. The limited effect of conversations with contrary-minded partners may potentially mask considerable heterogeneity, as polarizing/backfiring effects and de-polarizing effects could cancel each other out (Baysan, 2021). This could occur not only across partic-

program – and may thus interpreted as suggested conversation topics – and the four additional questions we included in the baseline and endline surveys.

ipants but also across different attitudes stated by the same person. To shed light on this, we look at a (Euclidean) distance measure of change in political opinions from baseline to endline political survey, which captures the magnitude of opinion change while deliberately ignoring its direction. Figure A6 plots the average ITT effects on this additional outcome measure and confirms that conversations with like-minded partners lead to a substantial adjustment in political views relative to the baseline, whereas there is no statistically significant adjustment in views following conversations with a contrary-minded partner. Hence, it seems unlikely that a single discussion with a person holding opposing views induces fundamental reconsideration of one's own political positions.

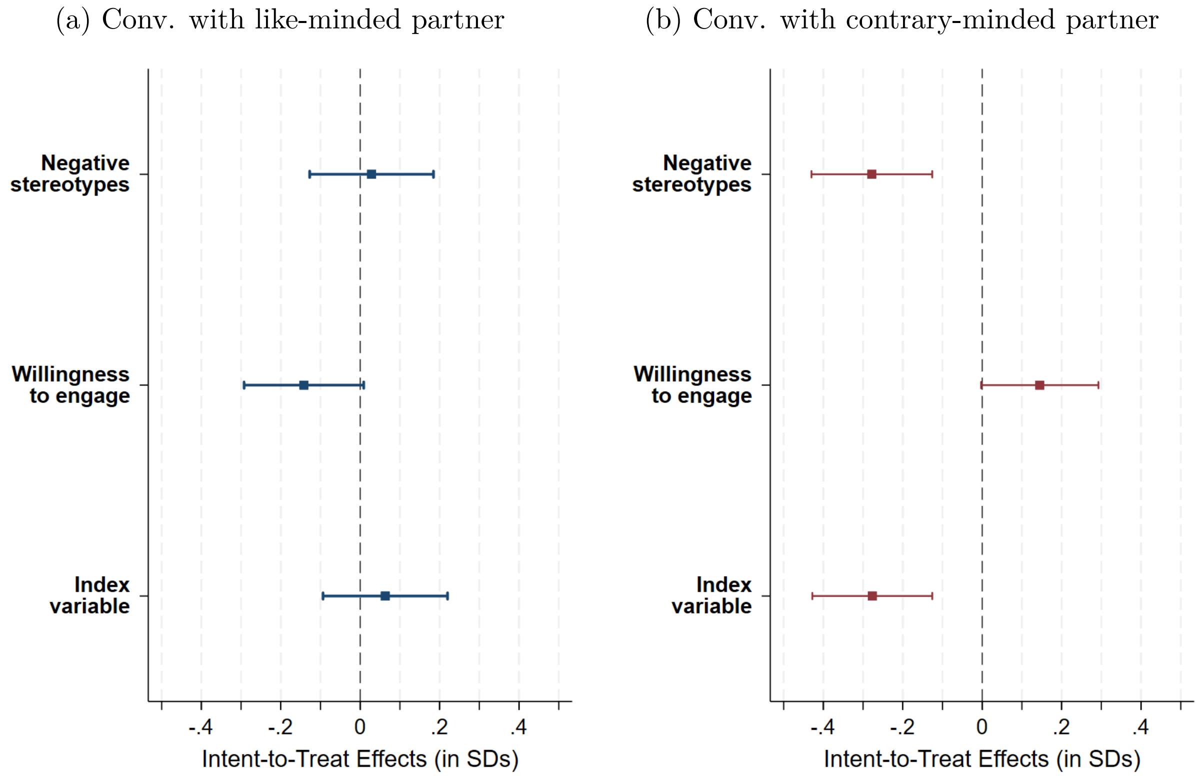
Is the polarizing effect of like-minded meetings large? Boxell (2020) reports an increase of political polarization in the U.S. by about 0.38 SDs between 1996 and 2016. Allcott et al. (2020) study the impact of a four-week-long deactivation of Facebook and find a reduction in their index of issue polarization of approximately 0.1 SDs. Compared to these benchmarks, our effect size of around 15% of a SD seems meaningful, although these comparisons are to be interpreted with caution, as our study took place in Germany instead of the U.S. and as we use different measures of ideological polarization.

4.2 Effects on affective polarization

Beyond the effect on ideological polarization, conversations about political topics with different partners may have an impact on affective polarization, i.e., people may adjust their beliefs and attitudes toward ideological out-group members. For example, related research on prejudice reduction through interaction suggests that interpersonal conversations between contrary-minded persons may lead to a reduction of negative stereotypes (Allport, 1954; Fishkin et al., 2021; Kalla and Broockman, 2020). In this section, we therefore turn attention to estimating the impact of face-to-face discussions with members of one's own and the other political camp on affective polarization.

Outcome measures. As described in Section 2.5, we include measures of attitudes and beliefs about generic contrary-minded persons, defined as someone with opposing political views on the seven political registration questions. Thus, it extends beyond only the direct conversation partner (in CM-T) to encompass ideological out-group members more generally. We elicited stereotypes regarding whether contrary-minded individuals are cognitively less capable, poorly informed, have different moral values and lead completely different lives. We reduce dimensionality by implementing a PCA and extracting the first principal component. Additionally, we elicited participants' willingness to have a contrary-minded person in their social environment. To estimate the overall effect on affective polarization, create an index

Figure 4: Effect on beliefs and attitudes toward people with opposing views



Notes. ITT effects on standardized measures of affective polarization: (i) negative stereotypes about a person with opposing political views, defined as the first principal component of four elicited stereotypes (see Table A23); (ii) willingness to engage in personal contact with a person that has opposing political views; (iii) an index variable of overall affective polarization, defined as the first principal component of all four elicited stereotypes and the willingness to engage (see Table A24). The measures are described in Section 4.2 and regression specifications are detailed in Section 3. Estimates are based on 25 repetitions of the Chernozhukov et al. (2018) double machine learning estimator with tenfold Lasso cross-fitting and different random sample splits each. Error bars represent 95% confidence intervals.

variable of affective polarization by conducting a PCA with the four negative stereotype measures and willingness to engage in personal contact. Higher values are associated with more animosity toward members of society who hold different political views. All measures are standardized within each subsample using the respective control group moments. The standard deviations are similar in CM and LM for all measures of affective polarization.

Findings. Figure 4 shows that talking with a person who holds similar political views does not seem to reduce or increase negative stereotypes about ideological out-group members. In contrast, we find that in-person conversations with a contrary-minded partner lead to a strong and significant reduction in overall negative stereotypes about people with different political views by about 27% of a standard deviation ($p < 0.001$). This is in line with the contact hypothesis and the notion that in-depth conversations across ideological divides can help individualize out-group members and alleviate prejudice and affective alienation. Appendix Figure A7 investigates each item in the stereotype index separately and finds that the effects of contrary-minded conversations are particularly pronounced for beliefs that members of ideological out-groups have low cognitive capacity and hold different values than oneself, but little effect on the belief that they lead a completely different life. When we look at the effects on attitudes toward close social contact with the ideological out-group, the results are statistically less clear-cut, but our estimates suggest that conversations in the CM sample may lead to an increase in willingness to engage by about 15% of a SD on average (p = 0.0545), whereas conversations with like-minded partners may even have a negative effect on willingness to engage with ideological out-group members (p = 0.0651).

When combining both negative stereotypes and willingness to a single index measure of affective polarization, we find strongly significant average ITT estimates of about 28% of a standard deviation in the CM sample (see Table 4, columns 1 to 3). Moreover, column 3 confirms that, when considering the pooled sample, the asymmetry in effects of like-minded versus contrary-minded conversations is statistically significant (p =0.015).

Robustness. The results are robust to using OLS with a fixed set of controls and without any controls at all (Appendix Tables A10, A12), as well as to splitting the sample into LM and CM by (mis-)alignment of latent ideological class (Appendix Table A14). Moreover, note that we did not account for baseline measures of negative stereotypes and willingness to engage with ideological out-group members in our main specification, as most participants were already aware of their partner's identity and whether they would have a conversation by the time of the baseline survey. Nevertheless, we find that the beneficial effects of meeting a contrary-minded partner remain significant when we allow the DML procedure to consider baseline responses (see Appendix Table A15).

Interpretation. Our findings are consistent with the idea that contact with ideological out-group members can help reduce systematic misperceptions and stereotypes that may arise, e.g., due to the representativeness heuristic (see, e.g., Bordalo et al., 2016; Moore-Berg et al., 2020; Bursztyn and Yang, 2022). This may be particularly relevant in our context since the thematic topics chosen by *Germany Talks* represented salient ideological divides at that time (Bordalo et al., 2020b). Interestingly, these positive effects arise despite (or because) partners talk extensively about political topics on which they disagree, which is consistent with Rossiter (2023) but contrasts with Santoro and Broockman (2022). One potential explanation is that the in-depth meeting format allows sufficient time for partners to talk about both political and non-political topics, as well as to articulate their respective

Table 4: Effect on affective polarization and social cohesion

	Beliefs and attitudes toward other members of society					
	Affective polarization index			Social cohesion index		
	(1) LM	(2) CM	(3) Pooled	(4) LM	(5) CM	(6) Pooled
Treated	0.063	-0.277***	0.014	0.129*	0.201***	0.104
	(0.080)	(0.077)	(0.072)	(0.078)	(0.078)	(0.066)
Treated × CM			-0.240**			0.135
			(0.099)			(0.094)
Contrary-minded (CM)			-0.026			-0.261***
			(0.101)			(0.095)
Double ML	Yes	Yes	Yes	Yes	Yes	Yes
No. of selected controls	83	96	303	47	118	311
Observations	756	736	1492	765	741	1506
Like-minded sample	✓		✓	✓		✓
Contrary-minded sample		✓	✓		✓	✓

Notes. The table reports ITT effects of in-person conversations on the affective polarization index (columns 1 to 3) and an index of perceived social cohesion (columns 4 to 6). Dependent variables are standardized using mean and standard deviation of control group individuals in the respective samples. Estimates are based on 25 repetitions of the Chernozhukov et al. (2018) double machine learning estimator with tenfold Lasso cross-fitting and different random sample splits each. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

viewpoints and reflect on the reasons behind their disagreements.

To put the effect magnitude in perspective, we use different benchmarks. First, we follow Lowe (2021) and compare our estimates with effects of intergroup contact from a recent meta-analysis by Paluck et al. (2019), which reports an average effect of 0.39 standard deviations across studies. Second, a recent study by Santoro and Broockman (2022) found that a conversation with an out-party voter about a predefined non-political topic increased warmth toward out-party voters by 0.34 standard deviations directly after the conversations. Similarly, Broockman and Kalla (2016) show that a ten-minute face-to-face conversation with transgender/gender non-conforming canvassers leads to an increase in tolerance by 0.45 standard deviations after three days and 0.3 standard deviations after three weeks, respectively. Several considerations need to be made when comparing our effect sizes to existing literature. First, the general level of polarization in Germany was generally lower than in the U.S. (Boxell et al., 2022). Second, although the *GT* intervention aimed to match people with large political distance, it was constrained by geographic distance and the composition of registered participants. Thus, even conversation partners in the contrary-minded sample may have been more similar on average than subjects in other studies. Effect sizes also depend on the way we split our sample. When defining conversation types by (mis-)alignment of latent ideological class (Appendix Table A14), our estimates increase to about 0.35 standard deviations in magnitude. Third, the conversations happened in person at a time and place of participants' own choosing, unobserved by researchers and free of any preimposed structure or guidelines. The median conversations lasted about 150 minutes and thus constitute a relatively intense intervention, although one-shot in nature. Fourth, the endline survey was sent out seven days after the conversation took place. Thus, the reported effects capture a degree of persistence beyond the immediate aftermath, whereas some short-run effects of the conversation may have already dissipated by the time of measurement. While we cannot observe persistence beyond that time frame, we note that Broockman and Kalla (2016) found long-lasting effects after a ten-minute conversation. The positive effect on willingness to engage in contact with the ideological out-group may further point to a potential for virtuous cycles.

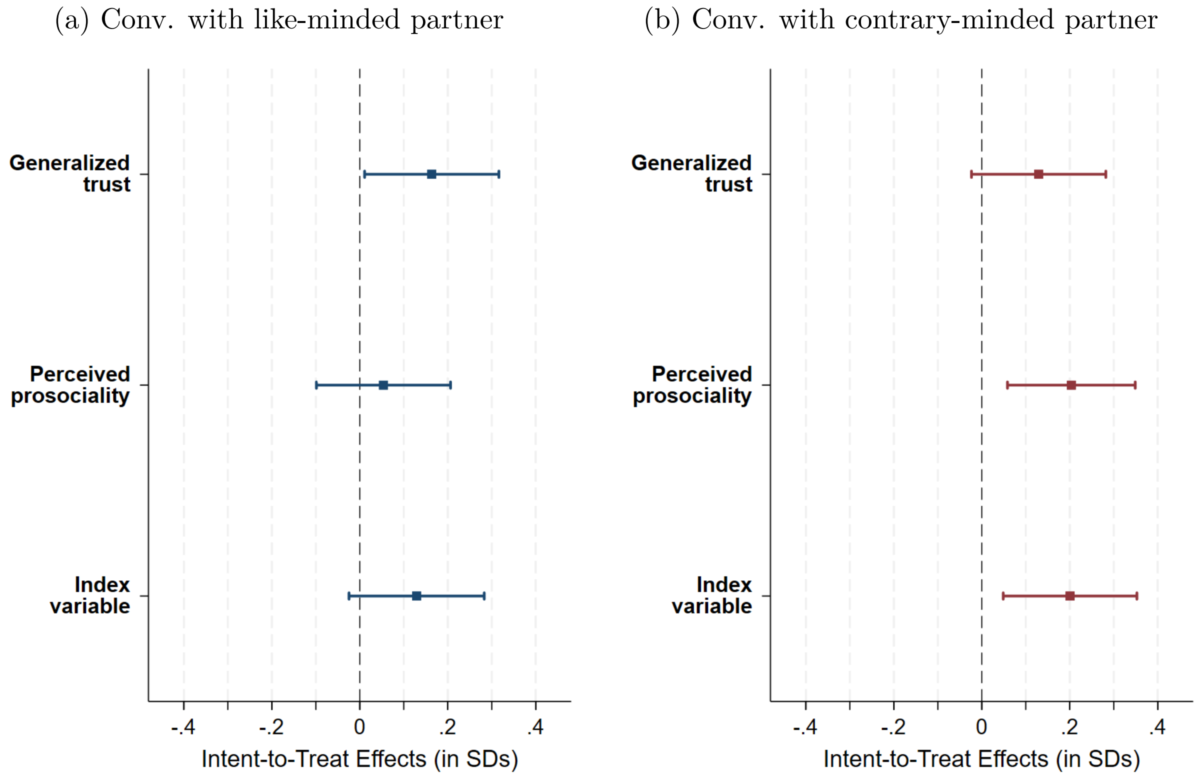
4.3 Effects on perceived social cohesion

Rising affective and ideological polarization could fundamentally undermine social cohesion by tearing society apart from within (Iyengar et al., 2019). Our finding that conversations with a contrary-minded person lead to more positive beliefs and attitudes toward out-group members is consistent with the contact hypothesis, but it may or may not translate to a more optimistic outlook about society more broadly. Related evidence by Rao (2019) finds an increase in general prosociality after contact, while Lowe (2021) observes a reduction in general trust.

Outcome measures. In the following, we will shed light on how interpersonal conversations affect beliefs about how trustworthy other members of society generally are, and how much fellow citizens generally care about the well-being of others. To assess the overall impact on perceived social cohesion, we additionally combine both trust and perceptions of prosociality into a single summary measure using equal (positive) weights. All measures are standardized within each subsample using the respective control group moments. The (control group) standard deviation is similar in CM and LM for all measures of social cohesion.

Findings. Figure 5 presents the average ITT effects on our measures of perceived social cohesion. In line with the results for affective polarization, we observe that participants who met with a contrary-minded stranger become more positive in how trustworthy and prosocial they perceive other members of society in general, although the effect is only strongly statistically significant for perceived prosociality ($p = 0.006$). Interestingly, we also find a positive effect on generalized trust after a conversation with a like-minded partner (p

Figure 5: Effect on perceptions of overall social cohesion in Germany



Notes. ITT effects on standardized measures of perceived social cohesion: (i) the perception that fellow citizens are generally trustworthy; (ii) the perception that fellow citizens care about the well-being of other; (iii) an equal-weighted index variable of the two measures. The outcome measures are described in Section 4.3 and regression specifications are detailed in Section 3. Estimates are based on 25 repetitions of the Chernozhukov et al. (2018) double machine learning estimator with tenfold Lasso cross-fitting and different random sample splits each. Error bars represent 95% confidence intervals.

= 0.036), which suggests that talking with strangers in general may have some beneficial effects on optimism toward other citizens. When we aggregate both measures into a single index variable of perceived social cohesion (see Table 4), we find a positive average effect in the CM sample of about 20% of a SD (p = 0.0096), while the estimated effect in the LM sample is only at the margin of weak significance (p = 0.0996). However, we cannot reject the null hypothesis that the effects of both types of meetings are equal.

Robustness. The results are robust to using OLS with a fixed set of controls and without any control at all (Appendix Tables A10, A12), as well as to splitting the sample into LM and CM by (mis-)alignment of latent ideological class (Appendix Table A14). Similarly as for affective polarization, we further present estimates including baseline survey measures of social cohesion, ignoring potential endogeneity concerns. Appendix Table A15 shows that the positive effects on social cohesion remain significant for like-minded conversations, but not for contrary-minded conversations. This may be driven by positive effects on perceived trustworthiness and prosociality already manifesting after the mutual confirmation of the conversation and the first written communication to arrange a meeting.

Interpretation. We find that conversations with contrary-minded individuals can have a positive impact on the perceptions of general trustworthiness and prosociality of other people in Germany. The findings are largely in line with the positive effects on affective polarization and further indicate that improvements in beliefs and attitudes extend also to an overall more optimistic view of society. This may go hand in hand with the reduction in negative stereotypes about people with opposing political views, particularly the realization that the values they hold tend to be closer to one's own than previously thought. Interestingly, we find that even like-minded conversations may have some positive effect on general trust, although the evidence is slightly weaker. This might speak to the general role of social interactions with strangers in fostering a sense of community, shared values, and trust in society – thus relating to the literature on social and civic capital (Putnam, 2000; Guiso et al., 2011; Durante et al., 2023).

5 Potential mechanisms

In this section, we aim to shed more light on potential mechanisms underlying our findings through a number of additional analyses.

5.1 Experience of the conversation

To provide suggestive evidence on *how* an in-person conversation might have affected political views as well as beliefs and attitudes toward others, we leverage endline survey data on participants' subjective impressions of the conversation – ranging from whether the partner was likeable to how fruitful the discussion was (see Section 2.4). This is to be interpreted with caution, as the control group counterfactual is unknowable and meeting experience is obviously endogenous: the way a conversation goes depends not only on the conversation partner, but also on oneself. That said, we observe strong and asymmetric associations between treatment effects and the reported meeting experience.

Political discussion versus interpersonal contact. In general, participants' responses to the seven survey items regarding how they perceived different aspects of the conversations are highly intercorrelated. Zooming into the idiosyncratic pattern of each conversation allows us to better understand which particular aspect matters most, but there is also a need to reduce dimensionality. Therefore, we conduct a "cross-demeaned" PCA and decompose the overall subjective experience into two components that emerge from the data: a first component that correlates more with perceived outcomes of the political discussion – e.g., whether views converged ($\rho = 0.715$) and whether the participant was able to convince the partner ($\rho = 0.814$) or vice versa ($\rho = 0.819$) –, and a second component that relates mostly to perceptions of how enjoyable the conversation was on an interpersonal level – e.g., whether the partner was likeable ($\rho = 0.809$) and whether the atmosphere was pleasant ($\rho = 0.558$).[17]

Findings. Table 5 presents the estimated associations between treatment effects on polarization outcomes and participants' subjective experiences of the political discussion and the interpersonal contact. We find no evidence that a positive impression of interpersonal contact is significantly correlated with outcomes on political views in either the LM or the CM sample. Similarly, we find no association between perceived discussion outcomes and actual changes in political views in the like-minded sample. However, columns (2) and (4) suggest that the perceived fruitfulness of the political discussion with a contrary-minded partner is indeed predictive of our actual outcome measures of ideological polarization. While the ITT effect of contrary-minded conversations on political views is close to zero, this average seems to mask that participants with above-average positive impressions of the discussion tend to become more moderate in their views, whereas participants who report below-average impressions even exhibit a backlash effect and become more extreme in their views. Quantitatively, a one SD change in the discussion component of overall conversation experience is associated with a 5-8% of a SD change in polarization.

In stark contrast, interpersonal contact seems to be the primary driver behind heterogeneous effects of face-to-face conversations on affective polarization and perceived social cohesion. A one standard deviation increase in positive contact experience with a contrary-minded partner is associated with a doubling of the average treatment effect on affective polarization (by 29% of a SD, $p < 0.001$), whereas less positive experiences may nullify or even reverse these effects. Interestingly, we also observe a weakly significant correlation with reduced affective polarization in the like-minded sample, maybe because partners could still disagree on potentially up to 3 out of 7 political topics in the registration questionnaire. Furthermore, there is a slightly stronger correlation of positive interpersonal experience dur-

[17] We first separate the overall experience from the specific experience pattern by calculating the mean response across the seven items (all on a 7-point Likert scale) and subtracting this overall mean value from each individual item response. The scale is reversed for "things got heated". We then run a PCA on the mean response as well as the seven "cross-demeaned" responses to individual statements. The first principal component (eigenvalue = 3.257) loads mostly on items about perceived discussion outcomes; the second component (eigenvalue = 1.187) loads mostly on statements about interpersonal contact.

Table 5: Association between meeting experience and polarization outcomes

	Distance from centrist view				Beliefs and attitudes toward others			
	Absolute centrist		Relative centrist		Affective polar.		Social cohesion	
	(1) LM	(2) CM	(3) LM	(4) CM	(5) LM	(6) CM	(7) LM	(8) CM
Treated	0.182***	0.011	0.139**	-0.029	0.085	-0.294***	0.147*	0.246***
	(0.053)	(0.046)	(0.056)	(0.030)	(0.082)	(0.079)	(0.081)	(0.081)
Treated × Positive discussion	-0.003	-0.080***	-0.008	-0.049**	0.055	-0.076	-0.029	-0.014
	(0.033)	(0.030)	(0.035)	(0.021)	(0.054)	(0.058)	(0.055)	(0.061)
Treated × Positive contact	0.017	-0.056	-0.050	-0.022	-0.102*	-0.290***	0.128***	0.074
	(0.035)	(0.034)	(0.040)	(0.024)	(0.053)	(0.055)	(0.049)	(0.059)
Double ML	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# selected controls	120	109	116	123	118	115	97	125
Observations	655	646	655	646	680	670	688	674
LM sample	✓		✓		✓		✓	
CM sample		✓		✓		✓		✓

Notes. "Positive discussion" is the first principal component extracted from a cross-demeaned PCA on perceived experience and subjective outcomes of the meeting among treated participants. "Positive contact" is the second principal component. Outcome and meeting experience variables are standardized to mean 0 and standard deviation 1 within each sample. Participants who are assigned to the treatment group but did not have a meeting after all are excluded. Estimates are based on 25 repetitions of the Chernozhukov et al. (2018) double machine learning estimator with tenfold Lasso cross-fitting and different random sample splits each. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

ing like-minded conversations with improvements in perceived social cohesion, which reflects beliefs and attitudes toward other members of society in general.

Robustness. These findings may not necessarily have a fully causal interpretation due to potential reverse causality, especially for the affective polarization and social cohesion outcomes. In particular, individuals who enter the conversation with more positive prior attitudes and beliefs may also be able to create a more pleasant experience. Therefore, Appendix Table A16 reports results that additionally control for baseline measures of affective polarization and social cohesion, respectively, thus allowing for a tighter difference-in-differences identification. Intriguingly, the associations between interpersonal contact experience and outcomes related to other-regarding beliefs and attitudes remain large and significant. Note that, in most cases, the baseline survey was sent only after the organizers established first contact between mutually accepted partners in the treatment group. Including these endogenous controls may potentially bias the estimated associations toward zero, as part of the positive (or less positive) impression may already manifest itself in the first written communication between matched partners when arranging the meeting.

Interpretation. Overall, the suggestive results presented here are consistent with contact theory and the notion that interpersonal contact among members of different (ideological) groups can reduce animosity by building familiarity with out-group members and fostering an understanding of the reasoning and emotions behind their points of views, even if the conversation partners part ways in agreement to disagree. Civility and friendliness of the exchange seem to be an important pre-requisite. However, the effects of conversations with ideological out-group members on extremeness of political views may be double-edge and go in either direction, depending on how the discussion goes. This highlights the importance of future research on determinants of political discussion outcomes. At the same time, we observe that ideological feedback loops following exposure to individuals with like-minded views seem to arise irrespective of the conversation experience. This could be explained by asymmetric cognitive responses in the social exchange of beliefs depending on congruence with one's priors, for example due to confirmation bias (Glaeser and Sunstein, 2009; Oprea and Yuksel, 2022).

5.2 Evaluating alternative explanations

There are a number of potential confounding mechanisms that could explain for our empirical findings, such as differential composition of the like-minded and contrary-minded sample, conflict avoidance when talking with contrary-minded partners, and disappointment among individuals in the control group due to not being accepted by their matched partner. In the following, we will discuss these alternative explanations and provide suggestive evidence that they are unlikely to drive our results.

Composition of the LM and CM samples. Our identification strategy aims at estimating the causal ITT effects of conversations conditional on being matched to a like-minded or a contrary-minded partner, with one key finding being the asymmetry in impacts on ideological and affective polarization. However, one challenge in interpreting these findings is that the composition of participants in the LM and CM samples also differs along other dimensions (apart from the partner type) that may influence how they respond to conversations with a generic stranger in general, in particular political orientation (see Section 2.3). This is because constraints in the available partner pool within an acceptable geographic radius lead to the GT algorithm having more difficulty generating matched pairs with high political disagreement among left-leaning participants. We conduct two types of robustness checks to alleviate concerns that the asymmetric effects of like-minded versus contrary-minded are driven by sample composition. First, we show that our results are very similar when we restrict the sample only to participants with a leftist ideological class, which eliminates the dominant discrepancy between the two samples (see Appendix Tables A17 and A18). Second, we conduct additional analyses where we split the sample by treatment status – instead of by partner match – and estimate the effect of being matched to a contrary-minded partner (compared to a like-minded partner) on polarization outcomes while controlling for a rich vector of covariates and interactions using DML. This should account for any confounding differences in observable participant characteristics between the LM and the CM sample (other than the partner type) that could drive differences in the estimated treatment effects. The results presented in Appendix Tables A19 and A20 suggest that there are robust effects of talking with a contrary-minded partner (versus talking with a like-minded partner) among treated participants, when compared to the counterfactual effect of being matched with a contrary-minded partner in the control group. The signs and magnitudes of these differences-in-differences are similar to those in Tables 3 and 4, thus suggesting that the asymmetry in impacts on polarization do not seem to be driven primarily by sample composition.

Conflict avoidance. Another alternative explanation beyond composition could be that contrary-minded partners lead their conversations in systematically different ways than like-minded partners, for example, if participants try to preserve interpersonal harmony by avoiding discussions about contentious topics on which they fundamentally disagree (see, e.g., Chen and Rohla, 2018). Our data does not support this explanation. First, meetings among contrary-minded partners were significantly *longer* than those among like-minded partners, with median durations of 150 and 120 minutes, respectively ($p < 0.01$). Second, Figure A3 shows that contrary-minded partners are *more* likely to discuss a particular topic when they have opposite positions on it in the registration questionnaire.

Disappointment. Finally, another possible confounding mechanism for the observed reduction in affective polarization could be that it is driven instead by disappointment among non-accepted participants in the control group. Specifically, participants who have accepted their match but are not accepted reciprocally may feel let down by their partner and hence update their beliefs and attitudes (negatively) on that basis, thereby distorting any comparisons with the treatment group. If this was the case, we should observe systematically higher affective polarization among control group individuals who were rejected by contrary-minded partners rather than by like-minded partners. Appendix Table A21 shows that this is not the case, thus supporting the interpretation that the observed asymmetry in treatment effects is driven by real difference in behavioral responses to talking with ideological in-group versus out-group members.[18]

[18]More specifically, Table A21 tests whether control group participants in CM are more negative than control participants in LM with regard to (1) baseline survey measures of affective polarization, and (2) time trends in affective polarization from baseline to endline survey. Note that the baseline survey was sent out over 1 week after GT informed participants of the match. First-accepters who have not heard of their partner by that time may therefore have concluded that there would be no meeting, in which case disappointment effects should be visible in baseline. However, if first-accepters were still hopeful at that time, disappointment

6 Discussion

This study exploits a quasi-experimental field setting to estimate the impact of in-person conversations on political polarization, depending on whether the partner is like-minded or contrary-minded. Thus, it serves as a proof of concept in demonstrating that interpersonal communication can be both a cause of rising polarization, but also a powerful tool to counteract the negative consequences of political polarization.

External validity. We show that in-person meetings with like-minded and contrary-minded strangers can have differential effects on affective and ideological polarization among participants who chose to voluntarily participate in a large-scale program (*Germany Talks*) that matches individuals for political conversations. As in most empirical studies, one major question concerns the generalizability of our results, which boils down to four elements: selection, attrition, naturalness, and scaling (List, 2020).

As described in Section 2.3, participants of our study are not generally representative of the overall German adult population. People who *select* into *GT* are on average richer, more educated, more urban, more likely to be male, more politically engaged, and more left-leaning in their political ideology – this is perhaps unsurprising given the nature of the program and the recruitment through (politically center-left) newspapers and news media outlets. It is ex ante unclear how our conclusions would extrapolate beyond our setting and our sample. One particular concern with our design is that we study individuals who are eager to voluntarily meet and discuss political topics with a stranger: firstly, they registered for *GT*, and secondly, we condition on the subsample of first-accepters for causal analysis. If these individuals are on average more open-minded and willing to change their opinions, one might expect the magnitude of effects in our sample to be larger compared to the general population. On the other hand, one might argue to the contrary that it is especially those who do *not* seek out contact on their own accord who would have found it particularly illuminating to do so, for example if lack of willingness to engage with out-group members is partly driven by misperceptions and stereotypes (see, e.g., Bordalo et al., 2016; Moore-Berg et al., 2020; Ruggeri et al., 2021; Bursztyn and Yang, 2022). Similarly, research in psychology demonstrates that people tend to systematically underestimate a priori how much knowledge and enjoyment they would derive from talking with strangers (e.g., Epley and Schroeder, 2014; Sandstrom and Boothby, 2021; Atir et al., 2022).[19] In any case, the selective sample

should only become manifest in the endline survey. We find no evidence for disappointment effects in the control group through either lens.

[19] Indeed, Blattner and Koenen (2023) who study the 2021 iteration of *GT* find suggestive evidence for adverse selection: the mitigating effects of (contrary-minded) conversations on affective polarization tend to be concentrated among individuals with *lower* initial interest in having the conversation.

of intrinsically motivated participants we study represents a relevant subpopulation from a policy perspective, as voluntary engagement among "early adopters" is a necessary first step for any soft approach to encourage deliberation across ideological divides, such as the *My Country Talks* initiative.

In addition to voluntary registration for *Germany Talks*, another source of selection comes from using only a subsample of participants for estimating treatment effects on individual polarization outcomes. Firstly, our identification approach conditions on the subpopulation of first-accepters, and secondly, only registered participants who completed both the baseline and the endline survey can be included in the analyses, thus leading to *attrition*. This is a common issue in empirical research that relies on survey data to collect rich individual-level information. To check how the composition of our study sample is affected by selection and attrition among the *GT* participant pool, we use data from the registration questionnaire that every participant had to complete. Table A22 shows that female and older participants tend to be more likely to be included in our sample, with some additional smaller selection across states and answers to the seven political registration questions. This is driven mostly by selective survey response rather than conditioning on the pool of first-accepters. The overall explanatory power for sample inclusion is only about 3%, suggesting that the study sample generally resembles the broader participant pool of *GT* based on demographic characteristics and political views.

The *naturalness* of the *GT* intervention is an important contribution of our study to the literature. Participants in our setting meet in person, at a place and time of their own choosing; they talk with each other privately and face-to-face, unobserved by any researchers; and their conversations follow a free and natural flow that is undisturbed by any pre-imposed structure or guideline. However, one potential threat to external validity may lie in the (lack of) naturalness of our outcome measures, as is often the case with individual-level studies on political and affective polarization. While we document significant effects of in-person conversations on self-reported survey responses, we cannot observe individuals' real-world behavior after participating in the *GT* program. Given that *GT* was initiated to counteract the perception of growing political division in Germany, self-reported responses might be systematically influenced by the perceived intent of the intervention. Note that such experimenter demand effects are typically modest (De Quidt et al., 2018) and that the study by Blattner and Koenen (2023) on the 2021 virtual *GT* program finds consistent effects on behavior in incentivized dictator and trust games, which is presumably less prone to demand effect (although also not natural). Moreover, it would be unlikely that social desirability could explain the complete pattern of results that we observe in our context, especially since participants were not aware of any division into like- or contrary-minded samples. For example, it is not clear why we would observe that conversations with contrary-minded partners do not lead to a moderation in political views despite this being presumably perceived as one of the program's main objectives, or why like-minded conversations actually lead to a reinforcement in political polarization.

Finally, *scalability* comes naturally to our context, as *Germany Talks* already constitutes a nationwide program that has since expanded to other countries as well; face-to-face social contact and in-person conversations are also a regular occurrence in our daily lives.

Concluding remarks. Our evidence supports the common notion that "echo chambers" lead to an entrenchment and widening of ideological divides in society. In principle, a healthy democracy "is designed" to handle disagreements on policies. However, it may not be able to handle circumstances in which large parts of society form extreme prejudices and immediate animosity solely based on someone holding opposing political views to one's own (e.g., Orhan, 2022). Our paper shows that positive experiences of in-person conversations with people who hold contrasting political views can improve attitudes toward contrary-minded people and the perception of social cohesion more generally. This highlights the importance of reducing obstacles to – and creating (physical) space and opportunity for – interpersonal contact and civil discussions across different political groups as an effective countermeasure against political polarization, especially in light of growing geographical segregation (Bishop, 2009; Brown and Enos, 2021). While the attention economy underlying social media and online engagement often favors negativity, outrage, and ideological extremes (e.g., Zhuravskaya et al., 2020; Rathje et al., 2021; Brady et al., 2023; Robertson et al., 2023), grass-root initiatives such as "My Country Talks", which allow people of different views and backgrounds to talk and listen to each other, provide a hopeful case study for the benefits of facilitating bona fide dialogue across different political camps.

Our study constitutes a proof of concept on the potential polarizing or moderating effects of in-person conversations among like-minded versus contrary-minded individuals. Future research needs to investigate the role of endogenous selection into in-group and out-group contact and how the effects we document generalize to other samples and settings. Furthermore, it is unclear to which extent the effects of in-person conversation on attitudes, beliefs, and political views also translate into "real" behavioral changes in everyday contexts and over longer periods. Finally, we need to better understand the dynamic co-evolution process of affective and ideological (de-)polarization. While reducing affective polarization is no immediate panacea (e.g., Voelkel et al., 2021; Broockman et al., 2023; but see also Braley et al., 2023), it may offer a starting point for triggering virtuous cycles that could lead toward more constructive deliberation across ideological groups.

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A Online Appendix

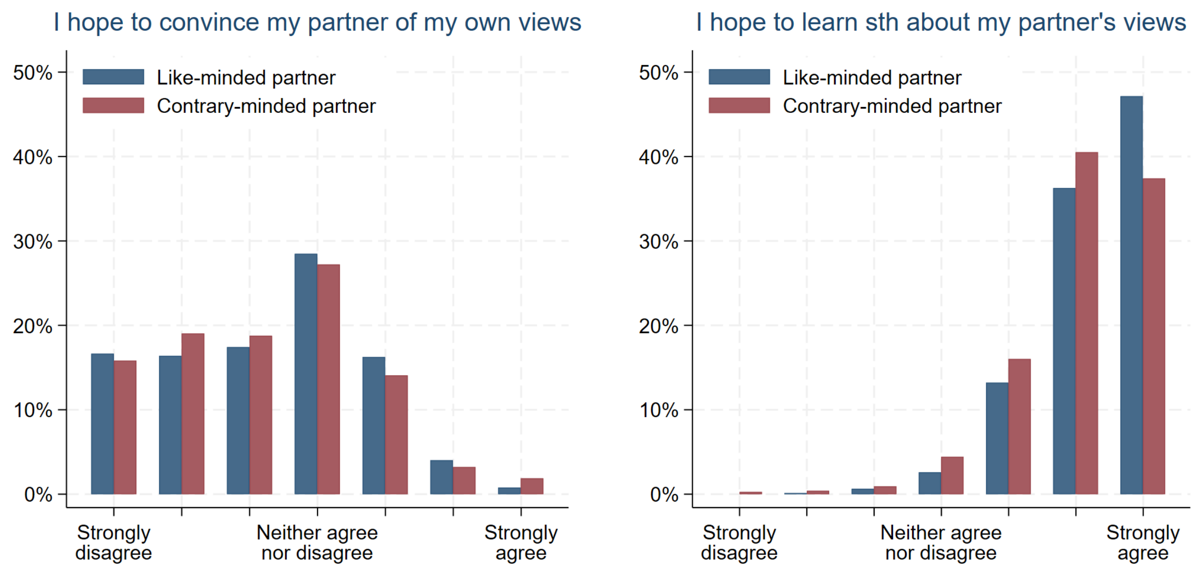
A.1 Additional Figures and Tables

A.1.1 Additional figures

Figure A1: Baseline expectations about the conversations

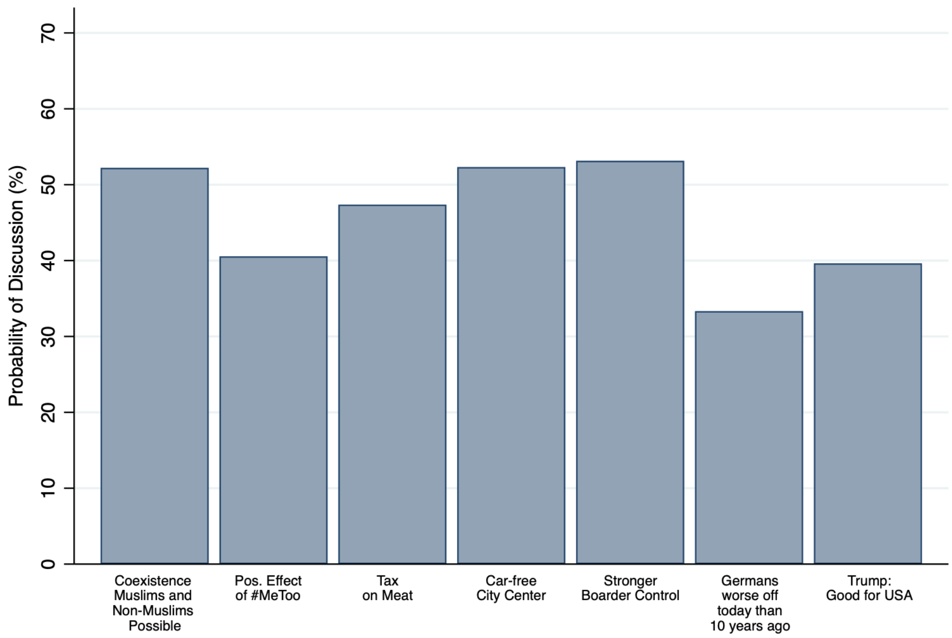
(a) Convince about own views

(b) Learn about other's views



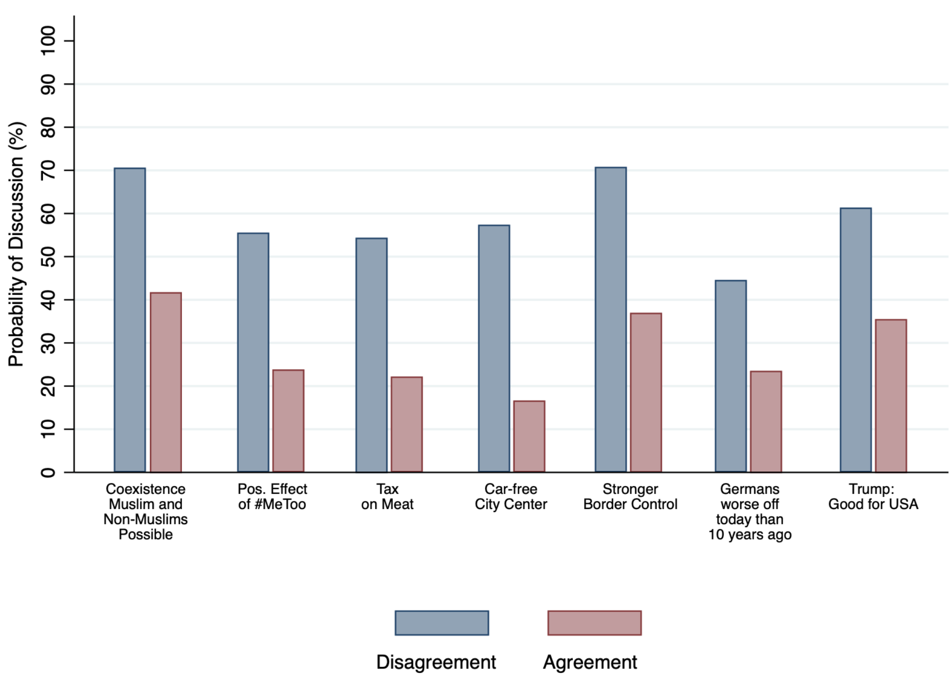
Notes. The figure plots the distribution of expectations about the conversation from the baseline survey before the conversation took place. Only participants who are part of our first-accepter sample are included. Note that the baseline survey was sent out 1 week after the matches were announced, so participants could already adjust their expectations based on information about the partner.

Figure A2: Topics of the Conversations



Notes. This figure plots the probabilities of discussion for the seven political registration questions. The y-axis of the graph denotes the frequency in %.

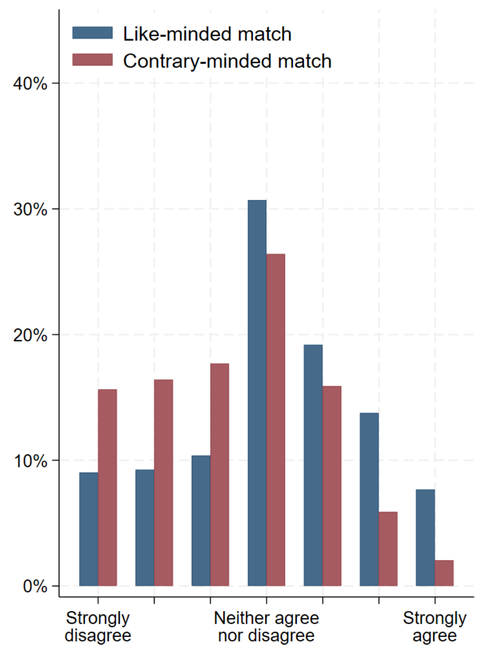
Figure A3: Conversational Topics: Agreement vs Disagreement (CM)



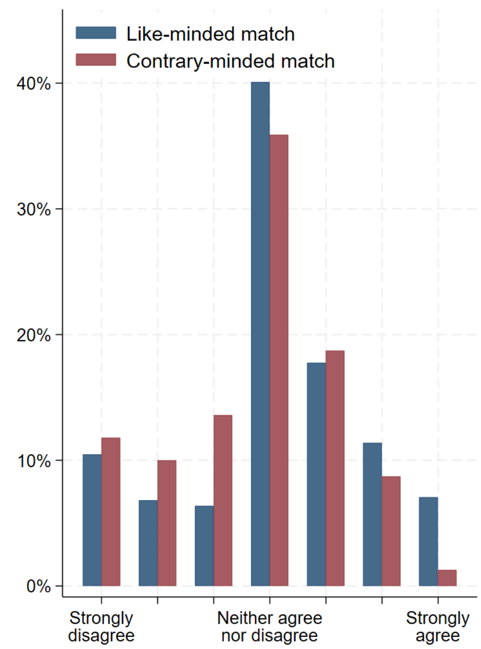
Notes. The figure plots probabilities of discussion for the seven political registration questions in the contrary-minded treatment condition, depending on whether the partners agreed or disagreed on the topic. The Y-axis indicates the share of pairs that discussed the respective topic.

Figure A4: Perceived meeting outcomes

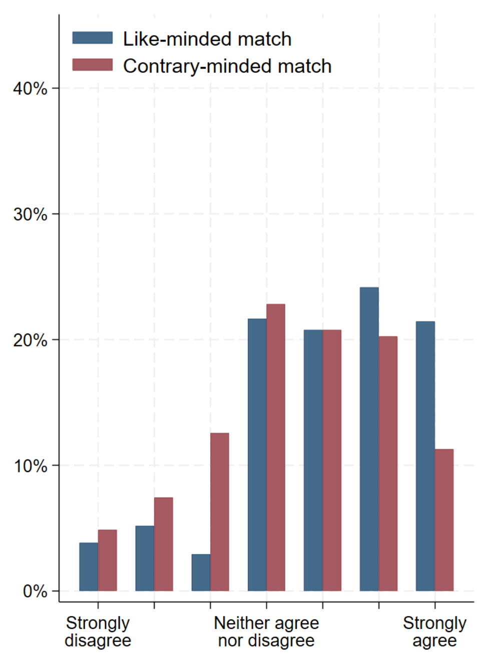
(a) Convinced by partner



(b) Convinced partner



(c) Views converged



(d) Learned about views

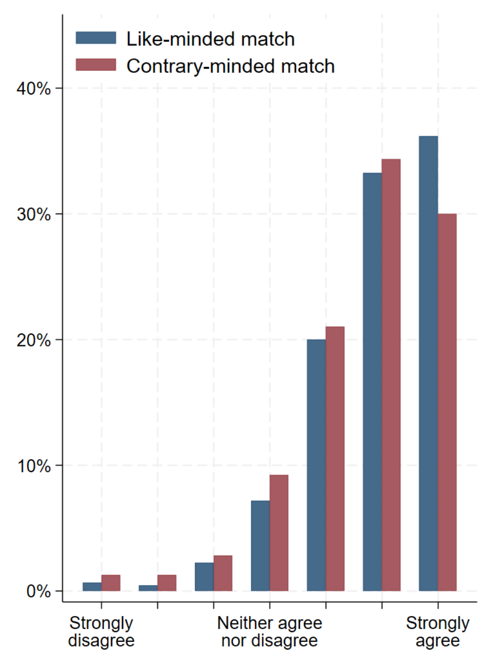
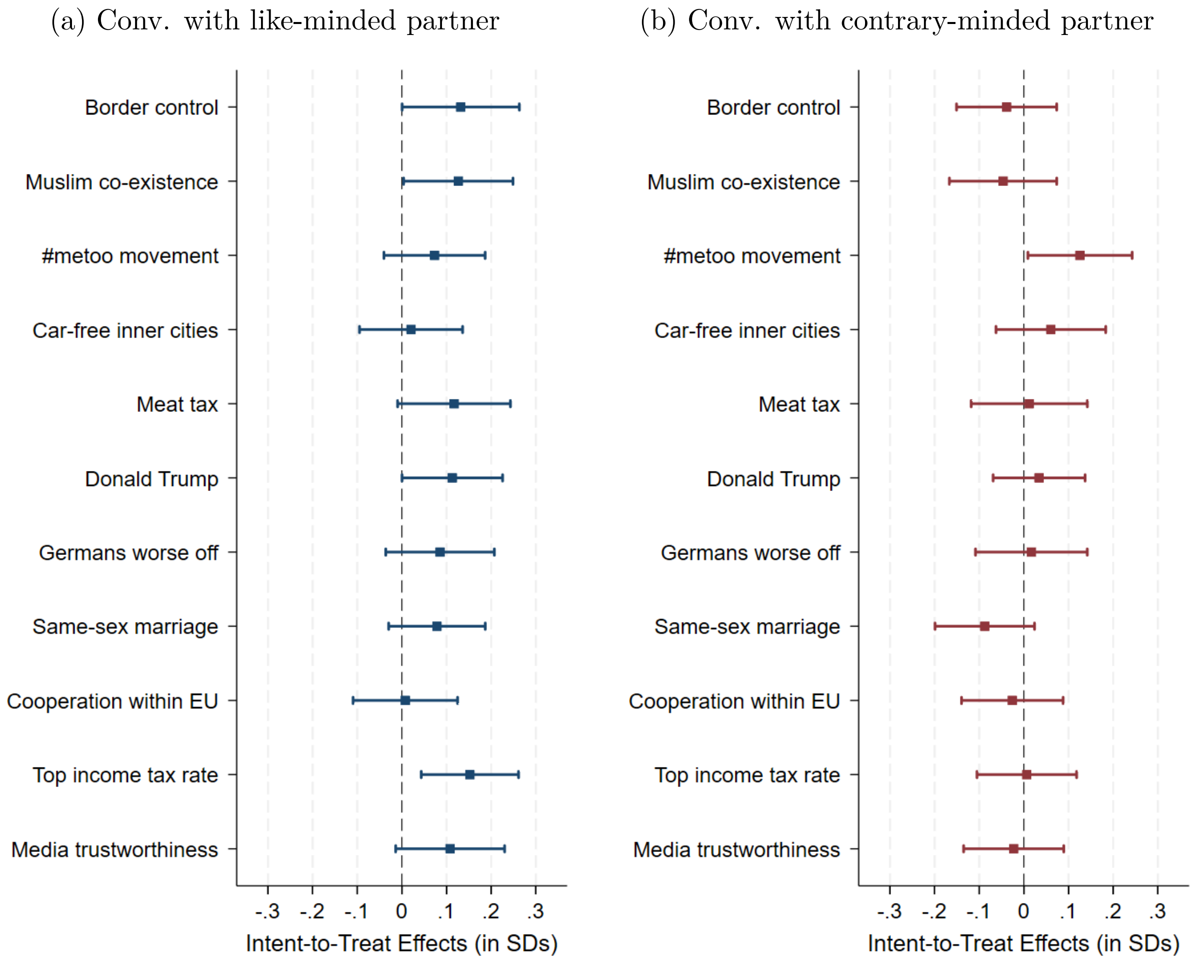
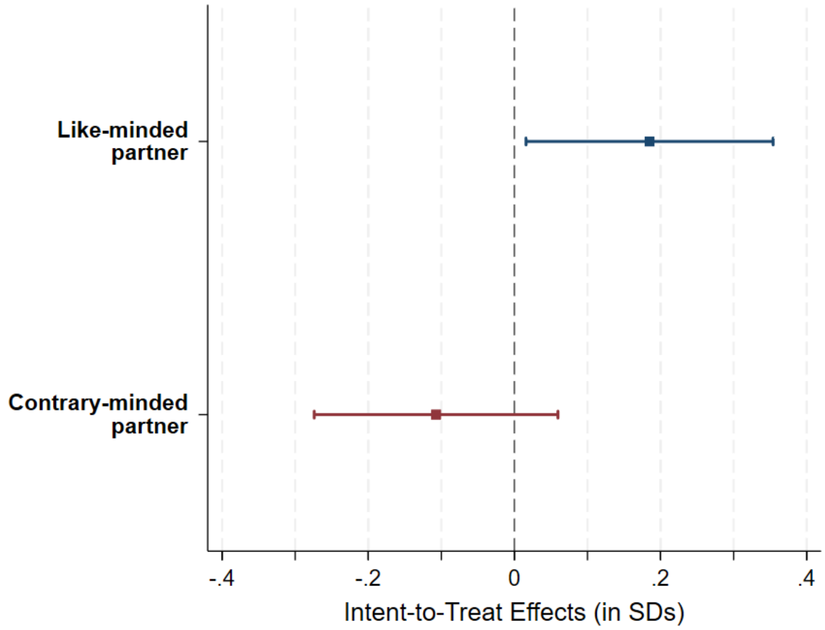


Figure A5: Effect on distance of political views from absolute center (by topic)



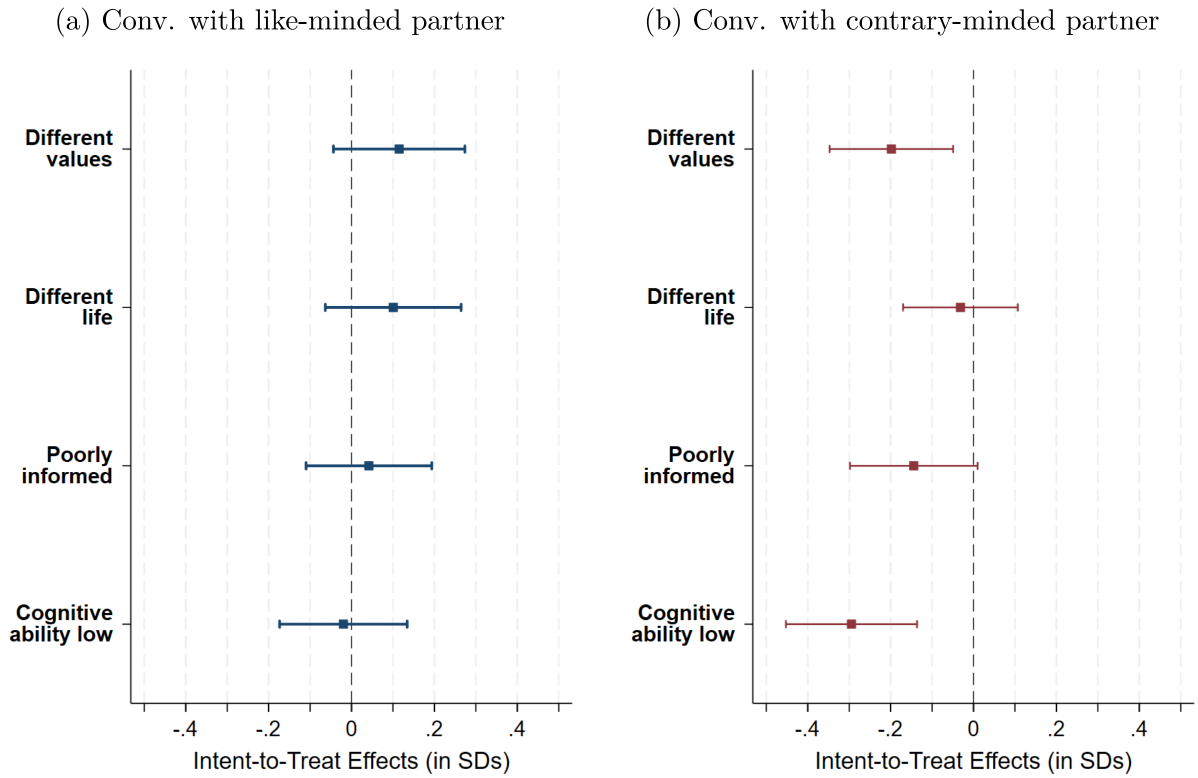
Notes. The figure shows the ITT effect of the like- and contrary-minded treatments on distance of political views from the absolute centrist view. 95% confidence intervals are included.

Figure A6: Effect on General Change of Political Opinion



Notes. This figure shows the ITT effects of the like- and contrary-minded treatments on standardized general change of the overall political opinion. A higher value denotes higher change. The general change of the overall political opinion is defined as the Euclidean Distance between the overall opinion before and after the meeting. The measure is described in Section 4.1 and regression specifications are detailed in Section 3. 95% confidence intervals are included.

Figure A7: Effect on Stereotypes (Separate)



Notes. The figure shows the ITT effect of the like- and contrary-minded treatments on standardized stereotypes. Higher values denote higher stereotypes. The first panel shows the effect on the stereotype that contrary-minded individuals are cognitively less capable. The second panel plots the effect on the stereotype that contrary-minded individuals are poorly informed. The third and fourth panel show the effects on the stereotypes that contrary-minded individuals have different moral values and live completely different lifes, respectively. The measures are described in Section 4.2 and regression specifications are detailed in Section 3.

A.1.2 Descriptive statistics and balance checks

Table A1: Summary statistics: socio-demographic characteristics

	Germany	*Study sample*		
	Population 2018 [%]	All [%]	LM [%]	CM [%]
Age				
18 - 34	24	25	27	23
35 - 54	32	37	35	39
55 or older	43	38	37	39
Gender				
Female	49	37	42	32
State				
Baden Württemberg	13	13	13	14
Bayern	16	14	14	14
Berlin	4	13	16	11
Brandenburg	3	2	2	3
Bremen	1	1	1	0
Hamburg	2	6	7	5
Hessen	8	8	8	9
Mecklenburg-Vorpommern	2	1	0	2
Niedersachsen	10	10	11	9
Nordrhein-Westfalen	22	17	16	18
Rheinland-Pfalz	5	3	3	3
Saarland	1	1	1	1
Sachsen	5	5	5	5
Sachsen-Anhalt	3	1	1	1
Schleswig-Holstein	3	4	4	3
Thüringen	3	1	0	2
Migration background				
Yes	23	10	10	10
Education				
No Education	2	0	0	0
Lower Sec. Education	24	1	1	1
Middle School	30	7	6	7
Advanced technical certificate	6	6	7	6
High School	10	17	17	17
University	27	67	68	66
Other	0	1	1	2
Income (monthly; EUR)				
0-800	19	10	11	8
800-1499	25	13	13	13
1500-2199	23	20	21	20
2200-3299	17	23	26	21
3300 or more	17	27	24	30
Observations		1,523	775	748

Notes. The table presents characteristics of the German adult population, our pooled sample, and the like-minded (LM) and contrary-minded (CM) samples. Measures for the German population are taken from the German Microcensus (age, gender, marital status), German Allbus 2018 (education, migration background, income).

Table A2: Summary statistics: political ideology

	Germany	*Study sample*		
	Population 2018 [%]	All [%]	LM [%]	CM [%]
Political spectrum left-right				
Far-left	3	4	4	3
Left	18	25	29	21
Centre-left	30	40	44	34
Centre	28	20	18	21
Centre-right	16	9	4	15
Right	3	2	0	4
Far right	1	1	0	1
Party				
Die Linke	10	14	14	12
Bündnis/90 Die Grüne	16	50	54	39
SPD	17	11	12	9
CDU/CSU	28	7	5	8
FDP	9	7	5	9
AfD	15	7	0	13
Other	5	5	3	5
Don't Vote/Don't know	31	2	1	2
Ideological Class				
Left Ideology		83	98	67
Right Ideology		17	2	33
Observations		1,523	775	748

Notes. The table presents characteristics of the German adult population, our pooled sample, and the like-minded (LM) and contrary-minded (CM) samples. Measures for the German population are taken from the CSES 2017 (left-right) and an election poll by Forsa from the week prior to *Germany Talks* (Party).

Table A3: Like-minded vs contrary-minded Partners

	Like-minded sample [%]	Contrary-minded sample [%]
Gender		
Female	38	21
Male	62	79
Age		
18 - 34	46	33
35 - 54	34	38
55 or older	21	29
Ideological Class		
Left Ideology	98	57
Right Ideology	2	43
Ideological Class: Overlap		
Same Ideological Class	97	26
Different Ideological Class	3	74

Notes. This table summarizes the characteristics of the partners in the like-minded LM (column 1) and the contrary-minded CM treatment condition (column 2). As most partners did not fill out the surveys, only age, gender and ideological (LCA) classes are available. Class membership is defined by the answers to the political registration questions. The last two rows indicate whether the two partners within one pair belong to the same class or not. The LCA is described in Section 2.4.

Table A4: Balance checks for treatment assignment (Logit)

	Dependent variable: 1 if treated			
	Like-minded (LM) (1)		Contrary-minded (CM) (2)	
Age	-0.047	(0.038)	-0.003	(0.035)
Age squared	0.000	(0.000)	0.000	(0.000)
Female	0.341**	(0.174)	0.322*	(0.192)
Migration background	0.158	(0.279)	0.332	(0.287)
College degree	0.071	(0.182)	0.272	(0.177)
Monthly income: €800 - €1500	0.214	(0.368)	-0.070	(0.350)
Monthly income: €1500 - €2200	-0.044	(0.337)	-0.081	(0.349)
Monthly income: €2200 - €3300	0.265	(0.349)	0.220	(0.363)
Monthly income: >€3300	0.266	(0.367)	-0.101	(0.364)
Monthly income: not reported	0.262	(0.454)	-0.048	(0.432)
Married	0.376	(0.500)	0.340	(0.455)
County of residence: Urban	-0.037	(0.221)	0.045	(0.203)
Religion: Christian	0.148	(0.169)	-0.036	(0.171)
Religion: Other	-0.064	(0.654)	0.737	(0.767)
Political ideology on left-right spectrum	-0.060	(0.085)	-0.009	(0.077)
Share of contrary-minded people in network	0.068	(0.076)	0.015	(0.065)
Party preference: Die Linke	-0.662*	(0.402)	-0.283	(0.436)
Party preference: Grüne	-0.286	(0.374)	-0.422	(0.376)
Party preference: SPD	-0.071	(0.446)	-0.385	(0.448)
Party preference: FDP	0.091	(0.529)	-0.390	(0.436)
Party preference: CDU/CSU	-0.801	(0.507)	0.006	(0.453)
Party preference: AfD			0.369	(0.460)
Party preference: Other party	0.039	(0.589)	-0.372	(0.479)
Civic engagement: Grassroots initiative	0.063	(0.319)	0.090	(0.321)
Civic engagement: Protesting	-0.311	(0.193)	0.158	(0.216)
Civic engagement: Party membership	0.170	(0.300)	0.311	(0.238)
Civic engagement: Worker union	0.034	(0.365)	-0.315	(0.393)
Political views: stricter border control	-0.079	(0.246)	0.308	(0.217)
Political views: #metoo movement	-0.140	(0.246)	-0.304	(0.186)
Political views: meat tax	0.221	(0.185)	0.113	(0.178)
Political views: car-free inner cities	-0.526***	(0.201)	-0.217	(0.175)
Political views: Muslim co-existence	-1.274*	(0.653)	-0.011	(0.255)
Political views: Germans worse off	-0.061	(0.298)	0.128	(0.208)
Political views: Trump good for U.S.	-1.090**	(0.462)	-0.665**	(0.268)
State dummies	Yes		Yes	
p-value for F-test of joint null	0.390		0.331	
Pseudo-R^2	0.057		0.063	
Observations	771		748	

Notes. The table reports balance checks by predicting treatment status using Logit. Column (1) reports the results for the like-minded and column (2) for the contrary-minded individuals. We report p-values for an F-Tests of joint significance of the full model. Robust standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$

Table A5: Lasso regressions on treatment status

	(1) LM	(2) CM	(3) Pooled
Selected lambda	0.0485	0.0279	0.0332
No. of nonzero coefficients	1	55	5
Out-of-sample R^2	-0.0009	0.0111	0.0074
CV mean prediction error	0.2237	0.2356	0.2230
Observations	774	748	1522

Notes. The table presents results from Lasso regression of treatment status on the vector of participant characteristics including interaction terms that we use for the main estimation procedure using DML. The penalty parameter lambda is selected through tenfold cross-validation (CV). We loop over 100 random splits of each sample and report cross-validated Lasso parameters using the split that minimizes the out-of-sample mean squared error.

Table A6: Testing for selective attrition

	Probability of dropping out after baseline			
	Like-minded sample		Contrary-minded sample	
	(1)	(2)	(3)	(4)
Treated	-0.009	-0.013	-0.040	-0.042
	(0.027)	(0.027)	(0.027)	(0.027)
Constant	0.493***		0.504***	
	(0.022)		(0.021)	
Double ML	No	Yes	No	Yes
No. of selected controls		27		37
R^2	0.000		0.002	
Observations	1511	1489	1441	1412

Notes. The dependent variable is a dummy variable equal to one if the participant filled out the baseline survey but did not complete the endline survey. Income and marital status are not controlled for because we elicited them in the endline survey. Estimates in columns (1) and (3) are based on OLS without additional controls, and estimates in columns (2) and (4) are based on 10 repetitions of the Chernozhukov et al. (2018) double machine learning estimator with tenfold Lasso cross-fitting and different random sample splits each. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$

A.1.3 Alternative measures of political ideology

Table A7: Robustness check for Table 3 using Mahalanobis distance

	Distance of political views from the centrist view					
	Absolute centrist view			Relative centrist view		
	(1) LM	(2) CM	(3) Pooled	(4) LM	(5) CM	(6) Pooled
Treated	0.139***	-0.024	0.120***	0.105*	-0.035	0.041
	(0.052)	(0.044)	(0.044)	(0.056)	(0.029)	(0.029)
Treated × CM			-0.138**			-0.089**
			(0.058)			(0.041)
Contrary-minded (CM)			0.008			0.059
			(0.055)			(0.040)
Double ML	Yes	Yes	Yes	Yes	Yes	Yes
No. of selected controls	52	68	228	59	85	223
Observations	729	710	1439	729	710	1439
LM sample	✓		✓	✓		✓
CM sample		✓	✓		✓	✓

Notes. The dependent variable is the Mahalanobis distance of political views to the centrist view, standardized to mean 0 and SD 1. Estimates are based on 10 repetitions of the Chernozhukov et al. (2018) DML estimator with tenfold Lasso cross-fitting and different random sample splits each. Robust SEs in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: Robustness check for Table 3 using Manhattan distance

	Distance of political views from the centrist view					
	Absolute centrist view			Relative centrist view		
	(1) LM	(2) CM	(3) Pooled	(4) LM	(5) CM	(6) Pooled
Treated	0.147***	-0.037	0.116**	0.129**	-0.046	0.055*
	(0.052)	(0.048)	(0.045)	(0.055)	(0.032)	(0.030)
Treated × CM			-0.135**			-0.101**
			(0.060)			(0.044)
Contrary-minded (CM)			0.017			0.095**
			(0.058)			(0.043)
Double ML	Yes	Yes	Yes	Yes	Yes	Yes
No. of selected controls	43	65	232	44	72	225
Observations	729	710	1439	729	710	1439
LM sample	✓		✓	✓		✓
CM sample		✓	✓		✓	✓

Notes. The dependent variable is the Manhattan distance of political views to the centrist view, standardized to mean 0 and SD 1. Estimates are based on 25 repetitions of the Chernozhukov et al. (2018) DML estimator with tenfold Lasso cross-fitting and different random sample splits each. Robust SEs in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.1.4 Using OLS instead of double machine learning

Table A9: Robustness check for Table 3 using OLS and fixed covariates

	Distance of political views from the centrist view					
	Absolute centrist view			Relative centrist view		
	(1) LM	(2) CM	(3) Pooled	(4) LM	(5) CM	(6) Pooled
Treated	0.161***	0.028	0.147***	0.151***	-0.023	0.087**
	(0.056)	(0.054)	(0.050)	(0.057)	(0.034)	(0.034)
Treated × CM			-0.132*			-0.092*
			(0.072)			(0.049)
Contrary-minded(CM)			0.057			0.052
			(0.058)			(0.040)
Baseline values	Yes	Yes	Yes	Yes	Yes	Yes
Participant characteristics	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.639	0.701	0.650	0.578	0.902	0.839
Observations	720	689	1421	720	689	1421

Notes. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A10: Robustness check for Table 4 using OLS and fixed covariates

	Beliefs and attitudes toward other members of society					
	Affective polarization index			Social cohesion index		
	(1) LM	(2) CM	(3) Pooled	(4) LM	(5) CM	(6) Pooled
Treated	0.112	-0.324***	0.099	0.106	0.267***	0.119*
	(0.082)	(0.088)	(0.078)	(0.085)	(0.076)	(0.072)
Treated × CM			-0.416***			0.160
			(0.112)			(0.104)
Contrary-minded (CM)			0.112			-0.130
			(0.090)			(0.083)
Participant characteristics	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.221	0.279	0.201	0.173	0.422	0.287
Observations	746	715	1473	755	721	1488

Notes. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.1.5 Estimating ITT effects without control variables

Table A11: Robustness check for Table 3 using OLS without additional controls

	Distance of political views from the centrist view					
	Absolute centrist view			Relative centrist view		
	(1) LM	(2) CM	(3) Pooled	(4) LM	(5) CM	(6) Pooled
Treated	0.159***	0.006	0.150***	0.124**	-0.026	0.082**
	(0.051)	(0.046)	(0.048)	(0.054)	(0.030)	(0.033)
Treated × CM			-0.144**			-0.101**
			(0.068)			(0.049)
Contrary-minded(CM)			0.053			0.135***
			(0.053)			(0.037)
Baseline values	Yes	Yes	Yes	Yes	Yes	Yes
Participant characteristics	No	No	No	No	No	No
R^2	0.711	0.809	0.707	0.680	0.937	0.864
Observations	606	553	1288	606	553	1288

Notes. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A12: Robustness check for Table 4 using OLS without additional controls

	Beliefs and attitudes toward other members of society					
	Affective polarization index			Social cohesion index		
	(1) LM	(2) CM	(3) Pooled	(4) LM	(5) CM	(6) Pooled
Treated	0.062	-0.288***	0.061	0.115	0.129*	0.101
	(0.079)	(0.078)	(0.077)	(0.078)	(0.078)	(0.069)
Treated × CM			-0.354***			0.035
			(0.111)			(0.107)
Contrary-minded (CM)			-0.011			-0.389***
			(0.086)			(0.083)
Participant characteristics	No	No	No	No	No	No
R^2	0.001	0.018	0.021	0.003	0.004	0.036
Observations	757	736	1493	766	741	1507

Notes. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.1.6 Splitting by ideological class

Table A13: Robustness check for Table 3 using ideological class to define LM and CM

	Distance of political views from the centrist view					
	Absolute centrist view			Relative centrist view		
	(1) LM	(2) CM	(3) Pooled	(4) LM	(5) CM	(6) Pooled
Treated	0.124***	0.014	0.107***	0.117**	-0.013	0.059**
	(0.046)	(0.052)	(0.042)	(0.049)	(0.039)	(0.029)
Treated × CM			-0.069			-0.107**
			(0.064)			(0.048)
Contrary-minded (CM)			0.009			0.140***
			(0.065)			(0.047)
Double ML	Yes	Yes	Yes	Yes	Yes	Yes
No. of selected controls	48	71	248	42	77	229
Observations	889	550	1439	889	550	1439
LM sample	✓		✓	✓		✓
CM sample		✓	✓		✓	✓

Notes. Robustness checks using political class obtained from running a LCA on the seven political registration questions. Estimates are based on 10 repetitions of the Chernozhukov et al. (2018) DML estimator with tenfold Lasso cross-fitting and different random sample splits each. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A14: Robustness check for Table 4 using ideological class to define LM and CM

	Beliefs and attitudes toward other members of society					
	Affective polarization index			Social cohesion index		
	(1) LM	(2) CM	(3) Pooled	(4) LM	(5) CM	(6) Pooled
Treated	0.033	-0.335***	0.014	0.122*	0.181**	0.131**
	(0.071)	(0.089)	(0.067)	(0.069)	(0.090)	(0.061)
Treated × CM			-0.300***			0.112
			(0.103)			(0.104)
Contrary-minded (CM)			0.032			-0.208**
			(0.110)			(0.106)
Double ML	Yes	Yes	Yes	Yes	Yes	Yes
No. of selected controls	48	85	246	29	79	252
Observations	924	568	1492	934	572	1506
LM sample	✓		✓	✓		✓
CM sample		✓	✓		✓	✓

Notes. Robustness checks using political class obtained from running a LCA on the seven political registration questions. Estimates are based on 10 repetitions of the Chernozhukov et al. (2018) DML estimator with tenfold Lasso cross-fitting and different random sample splits each. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.1.7 Including measures from baseline survey

Table A15: Robustness check for Table 4 including baseline survey controls

	Beliefs and attitudes toward other members of society					
	Affective polarization index			Social cohesion index		
	(1) LM	(2) CM	(3) Pooled	(4) LM	(5) CM	(6) Pooled
Treated	0.111*	-0.126**	0.102*	0.122**	0.083	0.095*
	(0.061)	(0.058)	(0.054)	(0.060)	(0.059)	(0.050)
Treated × CM			-0.221***			0.023
			(0.074)			(0.071)
Contrary-minded (CM)			0.051			-0.142**
			(0.075)			(0.069)
Double ML	Yes	Yes	Yes	Yes	Yes	Yes
No. of selected controls	62	98	271	47	99	286
Observations	744	730	1474	761	738	1499
LM sample	✓		✓	✓		✓
CM sample		✓	✓		✓	✓

Notes. Includes baseline survey measure of outcome variables in the control vector, but is otherwise identical to Table 4. Estimates are based on 10 repetitions of the Chernozhukov et al. (2018) double machine learning estimator with tenfold Lasso cross-fitting and different random sample splits each. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A16: Robustness checks for Table 5 with baseline survey controls

	Beliefs and attitudes about others			
	Affective polarization		Social cohesion	
	LM	CM	LM	CM
Treated	0.148**	-0.153**	0.143**	0.097
	(0.062)	(0.062)	(0.062)	(0.061)
Treated × Pos. discussion experience	0.049	-0.031	0.036	0.011
	(0.046)	(0.049)	(0.039)	(0.042)
Treated × Pos. interpersonal contact	-0.074*	-0.190***	0.083*	0.056
	(0.043)	(0.041)	(0.043)	(0.042)
Double ML	Yes	Yes	Yes	Yes
No. selected controls	108	103	77	119
Observations	671	664	685	671
LM sample	✓		✓	
CM sample		✓		✓

Notes. Includes baseline values as control variables. Estimates are based on 10 repetitions of the Chernozhukov et al. (2018) double machine learning estimator with tenfold Lasso cross-fitting and different random sample splits each. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.1.8 Restricting only to participants with leftist ideology

Table A17: Robustness check for Table 3 including only left-leaning participants

	Distance of political views from the centrist view					
	Absolute centrist view			Relative centrist view		
	(1) LM	(2) CM	(3) Pooled	(4) LM	(5) CM	(6) Pooled
Treated	0.159***	-0.011	0.132***	0.121**	0.003	0.072**
	(0.051)	(0.052)	(0.045)	(0.054)	(0.034)	(0.030)
Treated × CM			-0.123*			-0.065
			(0.066)			(0.044)
Contrary-minded (CM)			0.035			0.050
			(0.066)			(0.043)
Double ML	Yes	Yes	Yes	Yes	Yes	Yes
No. of selected controls	68	68	238	49	86	236
Observations	716	477	1193	716	477	1193
LM sample	✓		✓	✓		✓
CM sample		✓	✓		✓	✓

Notes. Only participants with leftist ideology based on the LCA (see Section A.3) are included. Estimates are based on 10 repetitions of the Chernozhukov et al. (2018) double machine learning estimator with tenfold Lasso cross-fitting and different random sample splits each. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A18: Robustness check for Table 4 including only left-leaning participants

	Beliefs and attitudes toward other members of society					
	Affective polarization index			Social cohesion index		
	(1) LM	(2) CM	(3) Pooled	(4) LM	(5) CM	(6) Pooled
Treated	0.081	-0.265***	0.040	0.128	0.155*	0.136**
	(0.079)	(0.096)	(0.071)	(0.079)	(0.080)	(0.066)
Contrary-minded (CM)			0.016			-0.116
			(0.102)			(0.098)
Treated × CM			-0.240**			0.053
			(0.103)			(0.092)
Double ML	Yes	Yes	Yes	Yes	Yes	Yes
No. of selected controls	49	70	261	28	75	258
Observations	743	497	1240	752	499	1251
LM sample	✓		✓	✓		✓
CM sample		✓	✓		✓	✓

Notes. Only participants with leftist ideology based on the LCA (see Section A.3) are included. Estimates are based on 10 repetitions of the Chernozhukov et al. (2018) double machine learning estimator with tenfold Lasso cross-fitting and different random sample splits each. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.1.9 Effect of partner type conditional on treatment status

Table A19: Effect of partner type on ideological polarization

	Distance of political views from the centrist view			
	Absolute centrist view		Relative centrist view	
	(1) Control	(2) Treated	(3) Control	(4) Treated
Contrary-minded(CM)	0.053	-0.087*	0.093**	-0.005
	(0.059)	(0.049)	(0.040)	(0.036)
Double ML	Yes	Yes	Yes	Yes
No. of selected controls	88	155	112	140
Observations	521	918	521	918
Control sample	✓		✓	
Treated sample		✓		✓

Notes. Estimates are based on 10 repetitions of the Chernozhukov et al. (2018) double machine learning estimator with tenfold Lasso cross-fitting and different random sample splits each. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A20: Effect of partner type on affective polarization and social cohesion

	Beliefs and attitudes toward others			
	Affective pol. index		Social coh. index	
	(1) Control	(2) Treated	(3) Control	(4) Treated
Contrary-minded (CM)	0.052	-0.189***	-0.151**	-0.165***
	(0.064)	(0.063)	(0.067)	(0.056)
Double ML	Yes	Yes	Yes	Yes
No. of selected controls	97	145	116	139
Observations	536	938	542	957
Control sample	✓		✓	
Treated sample		✓		✓

Notes. Control vector includes respective baseline survey values of the outcome measures. Estimates are based on 10 repetitions of the Chernozhukov et al. (2018) double machine learning estimator with tenfold Lasso cross-fitting and different random sample splits each. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.1.10 Testing for disappointment in the Control groups

Table A21: Testing for effects of LM versus CM in the control group

	Affective polarization measures					
	Stereoptypes		Willingness		Index	
	(1) Base	(2) Change	(3) Base	(4) Change	(5) Base	(6) Change
Contrary-minded (CM)	0.010	0.062	0.275*	-0.044	-0.068	0.073
	(0.143)	(0.076)	(0.153)	(0.085)	(0.150)	(0.075)
Double ML	Yes	Yes	Yes	Yes	Yes	Yes
No. of selected controls	109	91	111	112	99	88
Observations	549	537	552	545	548	536
Baseline survey	✓		✓		✓	
Endline survey		✓		✓		✓

Notes. The table tests for differences in baseline survey responses and time trends from baseline to endline among control group participants. *CM* denotes whether a person was matched to a like- or a contrary-minded partner. Estimates are based on 10 repetitions of the Chernozhukov et al. (2018) double machine learning estimator with tenfold Lasso cross-fitting and different random sample splits each. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$

A.1.11 Attrition

Table A22: Sample selection from population of registered *GT* participants

			Decomposition of overall selection			
	(1) Overall selection		(2) First-accepter (FA)		(3) Selection cond. on FA	
Age 25 - 34	0.007	(0.007)	0.009	(0.012)	0.006	(0.013)
Age 35 - 44	0.039***	(0.008)	0.009	(0.013)	0.065***	(0.015)
Age 45 - 54	0.079***	(0.008)	0.048***	(0.013)	0.101***	(0.015)
Age 55 - 64	0.129***	(0.010)	0.064***	(0.013)	0.168***	(0.017)
Age 65 or older	0.160***	(0.012)	0.037**	(0.015)	0.222***	(0.022)
Female	0.049***	(0.006)	0.013	(0.008)	0.058***	(0.010)
Urban	-0.002	(0.007)	0.011	(0.009)	-0.026**	(0.013)
Political views: border controls	-0.016**	(0.007)	-0.015	(0.009)	-0.019	(0.012)
Political views: #metoo movement	0.011*	(0.006)	0.052***	(0.009)	0.009	(0.011)
Political views: meat tax	-0.020***	(0.006)	-0.028***	(0.008)	-0.012	(0.010)
Political views: car-free inner cities	-0.001	(0.006)	0.003	(0.008)	-0.007	(0.010)
Political views: Muslim co-existence	0.015*	(0.008)	0.042***	(0.012)	-0.005	(0.016)
Political views: Germans worse off	-0.023***	(0.007)	-0.038***	(0.010)	-0.013	(0.013)
Political views: Trump good for US	0.014	(0.009)	0.047***	(0.013)	0.020	(0.017)
State: Bayern	-0.017*	(0.010)	-0.002	(0.014)	-0.019	(0.018)
State: Berlin	-0.022**	(0.010)	0.005	(0.014)	-0.023	(0.018)
State: Brandenburg	-0.008	(0.019)	-0.025	(0.026)	-0.030	(0.036)
State: Bremen	-0.041*	(0.022)	0.011	(0.036)	-0.090**	(0.035)
State: Hamburg	-0.029**	(0.012)	0.008	(0.018)	-0.020	(0.023)
State: Hessen	-0.004	(0.012)	-0.006	(0.016)	0.004	(0.022)
State: Mecklenburg-Vorpommern	-0.015	(0.026)	-0.024	(0.035)	-0.039	(0.047)
State: Niedersachsen	0.013	(0.012)	-0.008	(0.016)	0.047**	(0.023)
State: Nordrhein-Westfalen	-0.020**	(0.009)	-0.009	(0.013)	-0.017	(0.017)
State: Rheinland-Pfalz	-0.006	(0.016)	-0.009	(0.022)	-0.032	(0.029)
State: Saarland	-0.034	(0.029)	0.005	(0.045)	-0.018	(0.056)
State: Sachsen	0.004	(0.014)	0.009	(0.019)	0.019	(0.025)
State: Sachsen-Anhalt	0.009	(0.025)	0.000	(0.034)	-0.051	(0.041)
State: Schleswig-Holstein	-0.005	(0.017)	-0.002	(0.023)	-0.008	(0.031)
State: Thüringen	-0.035*	(0.020)	0.008	(0.031)	-0.069*	(0.038)
Constant	0.082***	(0.014)	0.309***	(0.020)	0.151***	(0.026)
Mean	0.138		0.393		0.202	
R^2	0.030		0.008		0.044	
Observations	19072		19072		7515	

Notes. Table presents results from regressing response to our surveys on characteristics from the registration questionnaire based on linear probability models. The omitted state category is Baden-Württemberg. Column (1) and (2) includes all registered participants and column (3) includes only participants who accepted their partner first (first-accepter). Robust SEs in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.2 Additional Details on Germany Talks and Surveys

A.2.1 Participating media

These news outlets were DIE ZEIT, Süddeutsche Zeitung and SZ.de, tagesschau.de and Tagesthemen (ARD aktuell), Deutsche Presse-Agentur, Der Spiegel, Chrismon and evangelisch.de, Schwäbische Zeitung, Die Südwest-Presse, Der Tagesspiegel, t-online.de, and Landeszeitung Lüneburg. The majority of the news outlets are traditional print media with online appearances. For example, DIE ZEIT is the largest weekly newspaper and Süddeutsche Zeitung is the second-largest daily newspaper in Germany. Both also cover the internet and broadcast media. t-online.de is a pure online news outlet. Tagesthemen is a daily news show in the evening on ARD, one of the two major German public television channels. On 16/08/2018 Tagesthemen showed a clip inviting viewers to participate in the program.[20] tagesschau.de is the online appearance of ARD. According to Pew Research Center (2018), ARD is the main news source for many Germans across the political spectrum. The political orientation of the larger partners is center/center-left. Pew Research Center (2018) show that ARD, Der Spiegel, and Süddeutsche Zeitung are placed on the middle of the left-right spectrum. Freitag et al. (2021) measure the political position of news outlets by politicians' sharing behavior and conclude that DIE ZEIT and Der Spiegel are positioned on the left of the political spectrum, while ARD and Süddeutsche Zeitung are positioned on the center-left.

A.3 Measures

Our analysis relies on two datasets: data from the intervention *Germany Talks* and self-reported survey data. The primary dataset comprises 19,141 registered participants and includes age, gender, zip code, answers to the seven political registration questions and the matched participant. The latter dataset comprises information elicited in the baseline or the endline survey.

A.3.1 Controls

In our analysis, we condition on a variety of control dummies that stem from both datasets, namely the *Germany Talks* and the survey dataset. In the baseline survey, we gathered information about participants' demographics such as education, migration background, and religion, the political heterogeneity of their social environments, i.e. how many politically contrary-minded people they have in their social environment, and their political preferences,

[20]The clip is available at the following link (in German): Link.

which includes a position on a political self-classification and the party they would vote for. In the endline survey, we elicited income and marital status. The following paragraphs list the relevant controls and how we construct them.

First, we consider basic information (hard facts) about the participant that we observe (age intervals, gender, region at NUTS level, combinations of answers to political registration questions) and proxies for surname (migration background, and education and income). More precisely, we divide age into following six intervals: 18-25, 26-35, 36-45, 46-55, 56-65, and 65+. We construct Gender as a binary variable indicating whether a person identifies as male, female / non-binary. Instead of including 1,531 five-digit zip codes in our analysis, we construct dummies based on the NUTS to increase power. NUTS (level 3) is a geocode standard that is developed and regulated by the European Union and divides Germany into 401 regions. We include all combinations of the seven binary political registration questions to control for policy view patterns. From our baseline survey, we include variables for the participants' education, income, and migration background. Education is an ordinal variable with seven categories from "No school leaving certificate" to "Ph.D.". We include dummies for each category. Migration background is a binary dummy where we define a person with a migration background as someone who either was not born in Germany or has parents who were born in a different country. Income is an ordinal variable that captures the net income per month of the respondents. It contains five categories, from "0-800 Euro" to "3300+ EUR" and an option for participants who do not know their monthly income. All variables additionally have a "Not specified" category.

An additional set of controls accounts for the fact that the answers to the open questions were unobserved by capturing potentially visible information. We did not receive that information (and the surname) from the organizers of *Germany Talks* due to data protection. Thus, we use proxies to capture potential topics as well as possible. This comprises *dummies* for each category of the measures party preference, political self-classification, political engagement, religion, religiousness, marital status, and the number of politically contrary-minded people in their social environment. Party preference indicates the party that the respondents would vote for. It is a nominal variable with nine categories including all five parties represented in the 19th Bundestag (German parliament) and the categories "Other party", "I don't know", and "I do not vote". Political self-classification is an ordinal variable with seven values from "Very liberal" to "Very conservative". Political engagement contains different forms of political engagement that participants have been part of or not: "Participation in civic initiatives", "Attending demonstrations", "Being an active member of a party", and "Being an active member of a trade union". Religion is a nominal variable indicating religious affiliation (seven categories). Religiousness is an ordinal variable eliciting how often participants visit a place of worship. It has six categories from "Never" to "More than once per week". Marital status dummies are "Single", "Divorced", "Widowed", "Registered partnership", "Married and living separately", "Married and living with a spouse". The number of contrary-minded people in the participants' social environment contains seven categories from "None" to "All". For all variables, we add a dummy indicating a missing value.

A.3.2 Outcome Measures

Outcome measures were elicited in the endline survey. Only in the case of political views did we also use values from the baseline survey to construct our measures. All outcome measures are standardized by subtracting the (respective) control group mean and dividing by the control group standard deviation.

Political Views Participants were asked to state the extent to which they agree with different political statements on a seven-point Likert scale. Apart from the transformation from questions into statements and the change of scales, the first seven of the eleven statements were identical to the political registration questions. In addition to the seven questions, we elicited four other, more general political attitudes (see Table 2 for an overview). Based on these attitudes, we create outcome measures for our analysis. The underlying idea is to take all eleven attitudes together and interpret the eleven-dimensional vector as the overall political opinion. In contrast to the measures of affective polarization and perception of social cohesion, we use data from the baseline survey as political views are not as easily affected by either learning the treatment condition (like- or contrary-minded partner) or first email contact with the partner. Importantly, looking at individual changes enables us to conduct a more precise analysis.

Ideological Polarization: Extreme Views in Terms of Absolute (Dis-)Agreement We construct two measures of ideological polarization. The first measure indicates the extent to which a person shows stronger (dis-)agreement with the topics after the meeting. More precisely, we construct one measure that indicates the distance to midpoint of our scale (a vector of 3s), denoting neither disagreement nor agreement. The measure is defined as follows:

$$ExtremeViewsAbsolute_i = \left(\sum_{s=1}^{11} (Y_{sit} - 3)^2 \right)^{0.5} ,$$

where Y_{sit} denotes individual i's level of agreement with statement s in the endline (t=2) or the baseline (t=1) survey. The eleven statements are the political attitudes from Table 2. Thus, $ExtremeViewsAbsolute_i$ indicates the Euclidean distance to the midpoint of our scale. By construction, the variable is always non-negative with larger values denoting more extreme opinions, i.e. more extreme disagreement or agreement on the topics.

Ideological Polarization: Extreme Views Relative to Population The second measure of ideological polarization reflects the extent to which an individual's overall opinion aligns with the average overall opinion in the respective sample (treatment condition):

$$ExtremeViewsRelative_i = \left(\sum_{s=1}^{11} (Y_{sit} - \overline{Y}_{s1c})^2 \right)^{0.5},$$

where Y_{sit} denotes individual i's level of agreement with statement s in the endline (t=2) and the baseline (t=1) survey. The eleven statements are the political attitudes from Table 2. $\overline{Y}_{s1c}$ is the average level of agreement with statement s of all participants in the treatment condition c in the baseline survey. In sum, $ExtremeViewsRelative_i$ reflects the distance to the average pre-meeting opinion in t.

General Change of Political Opinion To measure the general adjustment of the political opinion, we construct a measure that disregards any direction but focuses on the mere amount of change. More precisely, we define general change as the Euclidean distance between the endline and baseline survey:

$$GeneralChange = \left(\sum_{a=1}^{11} (Y_{si2} - Y_{si1})^2 \right)^{0.5},$$

where Y_{asit} denotes individual i's level of agreement with statement s in the endline (t=2) and baseline (t=1) survey. The eleven statements are the political attitudes from Table 2.

Affective Polarization To study how the conversations' affected stereotypes about individuals with contrasting political views and participants' willingness to have personal contact with these individuals, participants had to picture a person that gave opposing answers to the seven political registration questions. We then elicited participants' beliefs about this person by asking them the extent to which they agree with different statements about the contrary-minded person on a seven-point Likert scale. Importantly, we did not elicit beliefs and attitudes toward the matched partner but some generic person who holds opposing views. The elicited stereotypes were communicated by previous participants of *Germany Talks.*

Stereotypes We elicited four stereotypes, reflecting the beliefs that contrary-minded persons have low cognitive abilities, are poorly informed, have different moral values and lead a different life. Table 2 shows the exact wordings. We condense these questions by conducting a PCA. We use the first principal component as our overall *stereotype* measure. A higher value of our *Stereotypes* measure is associated with larger stereotypes about contrary-minded individuals. Table A23 provides the loadings of the first principle component.

Willingness to Engage in Personal Contact We elicited participants' *willingness to engage in personal contact* by asking participants to state their level of agreement to the statement that they do not want to have a person with opposing views in their social environment. For our analysis, we reversed the scale (see Table 2 for the exact wording).

Table A23: PCA: Loadings Stereotypes on Principal Component

Stereotype	Loadings
Different Way of Life	0.36
Different Moral Values	0.33
Low Cognitive Abilities	0.61
Poorly Informed	0.62

Notes. The table presents the loadings of the principal component analysis of all four stereotypes on the first principal component. The first component is the linear combination of the four stereotypes with the respective loadings as weights.

Table A24: PCA: Loadings Stereotypes and Willingness to Engage in Personal Contact on First Principal Component

Stereotype	Loadings
Different Way of Life	0.34
Different Moral Values	0.32
Low Cognitive Abilities	0.54
Poorly Informed	0.55
Willingness to Engage in Personal Contact	-0.43

Notes. The table presents the loadings of the principal component analysis of all four stereotypes and willingness to engage in personal contact on the first principal component which denotes our measure for overall affective polarization. The first component is the linear combination of the four stereotypes and willingness with the respective loadings as weights. The loadings are consistent with an interpretation of the component as an overall affective polarization measure as the signs of the loadings are positive for stereotypes and negative for willingness.

Perception of Social Cohesion To assess the effect on participants' perceptions of social cohesion in Germany, we elicited two beliefs: first, we asked how trustworthy the fellow citizens in Germany are (*Perception of General Trustworthiness*); and second, we measured participants' *Perception of General prosociality* by asking the extent to which German citizens generally care about the well-being of others. The two questions are listed in Table 2.

A.3.3 Latent Cluster Analysis

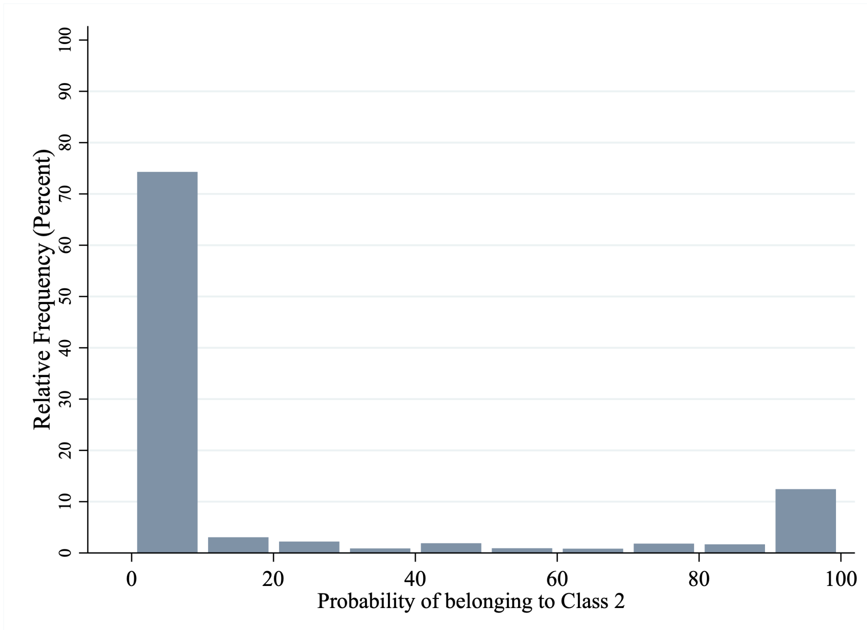
We investigated correlational patterns of the answers to the political registration questions. The organizers of *Germany Talks* carefully selected them in a way that there is typically a more "left" and a more "right" answer.[21] Thus, we should expect that one group gathers around left answers while another group chooses predominantly right answers if there are actually members of the two distinct camps within our sample. To check this, we use latent class analysis. LCA is related to factor analysis as both explore the relationship among variables. However, in contrast to FA, LCA assumes a categorical latent variable with a multinomial distribution instead of a continuous normal-distributed variable. This method does not demand any a priori assumptions about the correlations between the questions (i.e. which answers should belong in which group). Instead, it takes the data and checks whether there are latent classes whose members have specific answer patterns. LCA endogenously creates classes with specific answer patterns and assigns each participant a likelihood of membership in each class. Applying it to all registered participants, we see a bipolar distribution, i.e. participants belong to either one class or the other with a high probability (see Figure A8). Assigning participants to classes according to the probabilities, we find a large group to which 82% and a small group to which 18% of the participants belong. The answer patterns of the two groups - shown in Figure A9 - confirm the hypothesized distinction into a (large) ideologically left and a (small) ideologically right group. Membership in the left group predicts agreement with more liberal notions and clear disagreement with more conservative viewpoints. Likewise, members of the right group show a rather conservative answer pattern.[22] A t-test using self-stated left-right classification confirms the interpretation with the members of the large group being significantly more left ($p<0.01$). To further support this

[21] There are questions like "Should Germany increase its border control?", which represent typical left vs right topics, in this case migration. Other questions such as "Is Donald Trump good for the USA?" reflect less classic left-right topics, but nevertheless yield predictions about what conservatives and liberals would answer.

[22] For example, membership in the left group predicts disagreement with the demand of stricter border control, and agreement with the notion that #metoo had some positive effects. Membership in the right group predicts agreement with stricter border control but otherwise shows a less differentiating pattern. This is unsurprising as many of the conservative answer options are rather extreme opinions. For example,

finding, Table A25 reassuringly shows that we find nearly identical groups if we use k-means clustering instead of LCA. Focusing on the LM and CM samples, it is representative of all registered participants in terms of class membership (83% and 17%). Our sample comprises a majority of left- and a minority of right-leaning participants.

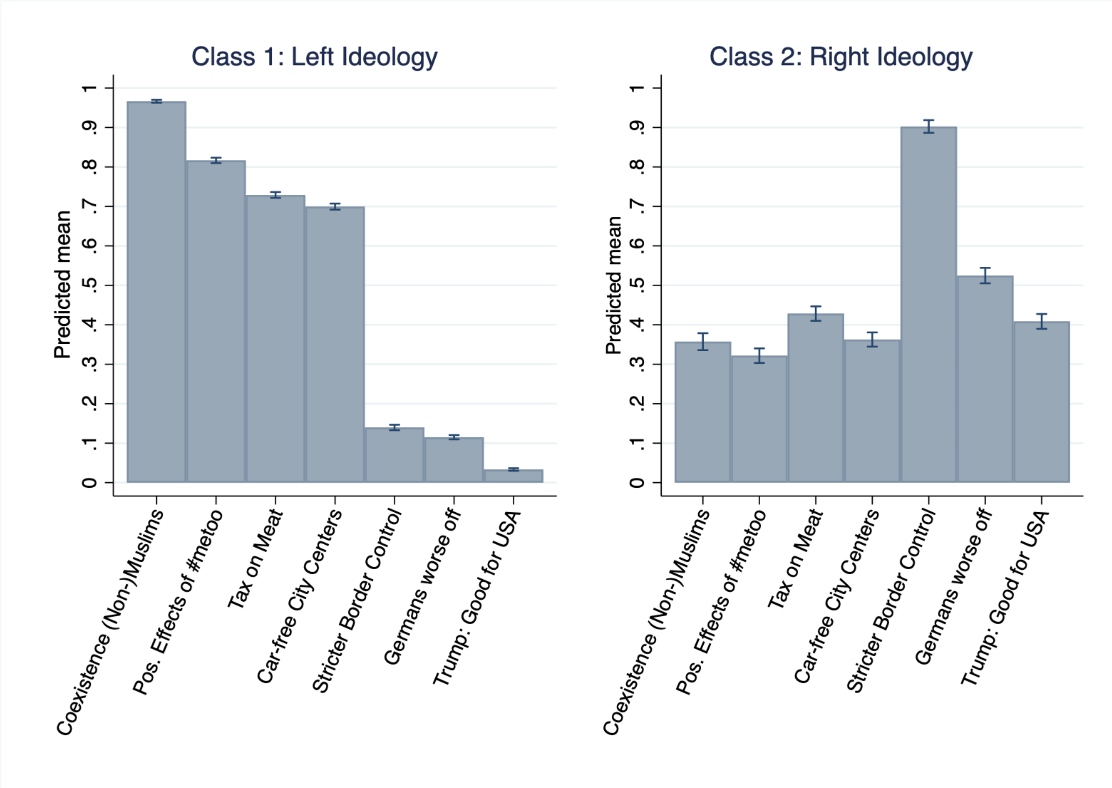
Figure A8: LCA: Likelihood of Class 1 Membership



Notes. The Figure plots the distribution of probabilities to belong to class 1 from the Latent Class Analysis. The LCA is described in Section 2.4.

disagreement with the statement that the #metoo movement and the debate about sexual harassment had *some* positive effects arguably reflects a far-right position.

Figure A9: LCA: Conditional Likelihood of Agreement



Notes. The Figure plots the probabilities of agreeing to the binary political registration questions conditional on LCA class membership. The LCA is described in Section 2.4. Error bars reflect 95% confidence intervals.

Table A25: Membership of Participants of *Germany Talks* to "Left" and "Right" Class

	Class 1: Left Ideology (kmeans)	Class 2: Right Ideology (kmeans)
Class 1: Left Ideology (LCA)	15,721	0
Class 2: Right Ideology (LCA)	377	2997

Notes. This table shows the number of participants of *Germany Talks* who belong to either the "left" or the "right" class, identified by LCA (rows) and k-means clustering (columns), respectively. The LCA is discussed in Section 2.4.