

# Interacting as Equals: How Contact Can Promote Tolerance Among Opposing Partisans

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## Abstract

In many contemporary democracies, political polarization increasingly involves deep-seated intolerance of opposing partisans. The decades-old contact hypothesis suggests that cross-partisan interactions might reduce intolerance if individuals interact with equal social status. We test this idea by implementing collaborative contact between more than one thousand pairs of citizens with opposing partisan sympathies, using the online medium to credibly randomize participants' relative social status within the interaction. Interacting under both equal and unequal status enhanced tolerant behavior immediately after contact; however, three weeks later, only the salutary effects of *equal* contact endured. These results demonstrate that a simple, scalable intervention that puts people on equal footing can reduce partisan intolerance and make online contact into a prosocial force.

# Introduction

In recent years, many countries have experienced partisan polarization severe enough to undermine trust in institutions and threaten the stability of democracy (Carlin and Love, 2018; Iyengar et al., 2019; Finkel et al., 2020; Baldassarri and Page, 2021). In such environments, political cleavages can align with preexisting social cleavages, including social status. Partisan political divides that extend into society are often called “affective polarization,” a particularly insidious form of animus that decreases cross-partisan interaction and exacerbates mutual intolerance (Mason, 2018; Iyengar et al., 2019). Could creating opportunities for people from opposing political camps to interact under conditions of equal status increase tolerance?

Social status lies at the center of the well-known contact hypothesis. Allport (1954) argued that cross-group collaboration under conditions that endow participants with equal status can increase intergroup tolerance by enhancing perceived commonality and interpersonal closeness between group members (ch. 30). In Allport’s words, “whatever makes for equal status relationships and for more intimate acquaintance is likely to make for increased tolerance” (p. 498). In contrast, contact under unequal status is less likely to bring dissonant groups closer and lacks the beneficial effects on intergroup tolerance (Allport, 1954; Pettigrew and Tropp, 2008; Enos, 2014). Yet the hypothesized role of status equality within the contact interaction lacks experimental validation, and observational research has yielded mixed findings (Pettigrew and Tropp, 2006; Paluck, Green and Green, 2019; Paluck et al., 2021).

We test Allport’s (1954) equal-status hypothesis by inducing contact between citizens with opposing partisan sympathies while experimentally varying their social status in the contact situation. Paired participants collaborated online on non-political tasks for ten minutes. The first task asked participants to decide whether fellow citizens in general value friendship or professional success more highly; the second task consisted of trivia questions about popular culture. Paired participants were provided with a chat window and encouraged to communicate with each other while completing these tasks. For our main analysis, we manipulated the participants’ relative status within the interaction (Fast, Halevy and Galinsky, 2012). In the equal-status condition, participants were informed that their respective answers to the tasks counted equally toward pair-level rewards. In the unequal-status condition, one participant was designated Leader and the other Follower, with only the Leader’s answers determining pair-level rewards.

We tested for both immediate and long-term effects of contact on tolerant behaviors.

Consistent with some prior nonexperimental research, our experiment shows that, immediately after treatment, all forms of intergroup contact enhanced tolerant behavior towards opposing partisans (Pettigrew and Tropp, 2006). However, three weeks after treatment, only the experience of intergroup contact under status equality continued to enhance tolerant behavior towards opposing partisans, whereas the salutary effects of interacting under status inequality had disappeared (Supplementary Materials Table T-13). We focus on these longer-term effects because they are less frequently measured in experimental research and are more relevant for policy.

Three weeks after contact, we find that participants assigned to the equal-status condition were willing to share 24% more of their own cash points with an anonymous study participant of opposing partisanship in a dictator game, compared to those in the no-contact control group ( $p=0.010$ ). Participants in the equal-status condition were also 5 percentage points (pp) more willing to accept an invitation to a future 30-minute meeting to discuss the country’s problems with a group of people that they were told would include opposing partisans ( $p=0.009$ ). A standardized index combining these two measures of tolerant behavior was .17 standard deviations greater under equal status contact compared to no contact ( $p<0.001$ ), and .13 standard deviations greater compared to unequal contact ( $p=0.009$ ). In contrast, contact under unequal status did not improve tolerant behavior for either Leaders or Followers compared to no contact. Analysis of chat content suggests that the overall quality of interaction was lower between participants assigned to the inequality condition in relation to the equal-status one.

Moving beyond the contact hypothesis, we additionally test whether knowledge of real-world socioeconomic status (SES) moderates the effect of equal-status contact. Staging intergroup contact online allows us to suppress—or reveal—real-world status cues that would be readily perceived in person (Desmichel and Rucker, 2022). Three weeks after contact, the effect of equal status contact remained unchanged when informing participants about their paired partners’ real-world socioeconomic status.

Our primary contribution is to build on prior research on the contact hypothesis. We experimentally manipulate participants’ status during contact and show that even brief collaborative interactions can have enduring positive effects. Prior experimental studies demonstrate that prolonged and intense contact between ethnic or religious groups, for instance in sports leagues, can lessen discriminatory behaviors (Scacco and Warren, 2018; Mousa, 2020; Bursztyn et al., 2021; Lowe, 2021). Shorter interventions have also reduced some kinds of nonpolitical prejudice (Broockman and Kalla, 2016), but intergroup contact experiments

that focus specifically on political prejudice have yielded mixed findings about effect persistence beyond treatment day (Rossiter, 2023; Rossiter and Carlson, 2023; Santoro and Broockman, 2022). In contrast with the present study, these and related approaches do not induce variation in participants' relative status while interacting (Paluck, Green and Green, 2019).

Our study also contributes to research on affective polarization. Affective polarization is traditionally defined as the tendency of people to view opposing partisans negatively and copartisans positively (Iyengar and Westwood, 2015; Iyengar et al., 2019). Intolerant behavior, our main dependent variable, is a key dimension of animosity toward outpartisans (Iyengar et al., 2019; Hartman et al., 2022). We demonstrate the importance of equal status as a condition for interpersonal contact to diminish partisan animosity. Additionally, we provide proof of concept that online spaces for cross-partisan contact that put people on equal footing can diminish partisan animosity in an affectively polarized environment.

Our research demonstrates the reduction of intolerant behaviors by implementing actual contact, an approach that differs from interventions that do not use contact, but instead provide corrective information about outpartisans (Voelkel, Ren and Brandt, 2021), give participants the opportunity to observe warm relations among opposing elites (Huddy and Yair, 2021; Voelkel et al., 2023), or prime self-affirmation, empathy, and other feelings (Levendusky, 2018*b*; Santos et al., 2022; Voelkel et al., 2023). Actual cross-partisan contact in our study also differs from interventions that simulate it by describing such contact in survey vignettes (Wojcieszak and Warner, 2020), simulating discussion environments (Voelkel, Ren and Brandt, 2021), or having participants meditate and imagine contact (Simonsson, Narayanan and Marks, 2022).

A final contribution is to extend the study of intergroup contact and affective polarization to a new location. We fielded our experiment in Mexico, where affective polarization has risen sharply in recent years (Moreno, 2020; Castro Cornejo, 2022). The party system now features two poles, one in support of the incumbent party, Movimiento Regeneración Nacional (MORENA), and another that combines previous adversaries in opposition (Castro Cornejo, 2023). In 2021, the mean difference between inparty and outparty warmth on a 10-point feeling thermometer was 5.2, equalling the level of polarization in the United States in 2020. Partisan groups in Mexico divide over core political issues, including assessments of democracy and electoral integrity (Hernández-Huerta and Cantú, 2022) and they harbor deep social animus toward one and other. Opposing partisans also segregate into distinct social networks (ITESO, 2019), view each other as dishonest, intolerant, and unpatriotic,

and increasingly identify with different social classes (Sánchez-Talanquer and Greene, 2021; Moreno, 2022). These divides are stoked regularly during President López Obrador’s highly-viewed daily morning press conferences that often include diatribes against the “immoral” elites and the middle class, the media, and opposition parties and partisans (Sarsfield, 2023). Partisan sympathies in Mexico increasingly operate like a macrosocial identity, like they do in the United States, resulting in affective polarization marked by deep-seated intolerance that extends from political interactions to non-political social settings (Mason, 2018; Carlin and Love, 2018). For instance, in a nationally representative survey, 79.8% of pro-MORENA respondents said they would accept having a MORENA sympathizer live in their house compared to only 38.3% of anti-MORENA respondents. (For details on all figures cited in this paragraph, see Supplementary Materials S-2.7 and Figure F-8.)

## Results

**Research design and sample.** Ours is among the largest experimental studies of inter-group contact to date. From an existing group of 150,000 survey respondents in Mexico, we invited 3,120 individuals to join the study, in batches of several hundred, at specific dates and times. Upon connecting, they were asked a question that we used to form pairs of citizens with opposing partisan sympathies (described below). We formed such pairs in the background while participants completed the remainder of a baseline survey. We then assigned pair-level treatments randomly, using blocking to improve statistical power (see Methods section below for details on the sample recruitment and randomization procedure). Pairs within a block were randomly assigned to equal status during contact ( $E$ : 780 pairs), unequal status contact ( $U$ : 390 pairs with random assignment of pair members to Leader ( $U_L$ ) or Follower ( $U_F$ )), or no-contact control ( $C$ : 390 pairs). To probe robustness of equal-status contact to information about real-world status differences, we exposed a random subset of pairs in the equal status condition  $E$  to information about their pair partner’s socioeconomic status ( $E_S$ : 390 pairs). The remaining pairs were not exposed to such information ( $E_N$ : 390 pairs).

Our main analysis sample consists of the 2,454 individuals (79% of those invited to the study) that remained after dropping those who did not complete the study and their paired partners. Attrition is statistically indistinguishable across treatment arms assigned to contact ( $E_S$ ,  $E_N$ ,  $U_F$ , and  $U_L$ ). Moreover, all experimental conditions including the no-contact control are well balanced on pre-treatment covariates including turning out to vote

in the 2018 presidential election, political interest, party identification, age, sex, and SES among others (Supplementary Materials S-1.1 Balance).

Participants were asked to complete an endline survey directly after the intervention and a follow-up survey approximately three weeks later. Everyone received a participation fee upon completing the study. Additional incentives were provided within the study conditional on participant responses (see Supplementary Materials S-2.1 Ethical Considerations). All incentives were provided at the end of the study, no deception was used, and all protocols obtained IRB approval from the University of Texas at Austin and ITAM in Mexico City. We pre-registered the trial at the Social Science Registry (<https://www.socialscienceregistry.org/trials/8143>).

**Pairing.** We divided participants into pro- and anti-MORENA groups by asking them which party they would vote for if presidential elections were held today. We use this measure for the following reasons. First, research in other new democracies shows that vote choice performs better at measuring partisan sympathies than traditional party identification questions that rely on prolonged exposure to the same party labels (Brader and Tucker, 2001; Dinas, 2014; Baker, Ames and Rennó, 2020, p.53). At the time of our study in 2021, MORENA had competed in just one presidential election in 2018. Second, although the two classification approaches (by vote choice vs. party identification) create substantively similar groups, the large proportion of respondents who do not identify with any party means that the vote choice measure captures the pro- versus anti-MORENA cleavage for a much larger proportion of our sample. Supplementary Materials Figure F-9 shows, using a separate 2018 survey, that views on core political issues such as evaluations of corruption, incumbent performance, and the economy are virtually identical when classifying voters according to vote choice or partisan identification.

**Pair-level intervention.** After informing paired participants of their partner’s partisanship (and, in the inequality condition, of whether they were designated Leader or Follower), we asked members of a pair to complete nonpolitical tasks. Like other studies, we designed our tasks to enhance perceived commonality and foster collaboration. In the first task, participants were asked to decide whether Mexicans in general value friendship or professional success more highly. This task was designed to generate a connection quickly in the tradition of the “fast friends” studies in social psychology (Aron et al., 1997) and prime participants’ superordinate identity of being Mexican to enhance perceived commonality (Levendusky, 2018a). The second task further encouraged collaboration by asking participants to answer

three trivia questions about Mexican popular culture. Light-hearted games like this one can increase positive attitudes toward outgroup members (Rajadesingan et al., 2023). The tasks were accessible to participants independent of education, income, age, gender, and political views. During the contact interaction, we held constant across contact treatment arms the presence of common goals and the incentive to collaborate, both of which Allport (1954) hypothesized, and subsequent studies have shown, to enhance the effectiveness of intergroup contact (Lowe, 2021; Pettigrew and Tropp, 2006). A full description of the tasks appears in the Supplementary Materials (S-2.3 Chat Instructions and S-2.4 Control Instructions).

Paired participants were invited to communicate in an open-ended manner with their partner while completing the tasks. Communication took place in an anonymous text-chat window on the same screen, powered by Chatter, a purpose-built application (Rossiter, 2023). Figure 1 provides a flavor for the interface and the interaction by showing screenshots of the first task and the first few real messages of a chat between participants assigned to contact under status equality. Each member of the pair entered responses to the tasks' questions individually and their responses could not be observed by their partner in any treatment condition. We informed participants that both members of the pair would qualify for entry into drawings, one for each task, if they provided answers to the values question and at least two of three correct answers to the trivia questions. The reward for the values task was the use of responses in teaching about Mexicans' values in universities in the United States and Mexico. The reward for the trivia task was a lump-sum of cash points, roughly equivalent to 315 Mexican pesos or about US\$15, that could be exchanged for goods in an online store.

We manipulated relative status by informing participants about their experimentally assigned status at the beginning of the first task and reminding them of it before the second task. In the *equal status* treatment, participants were told that their respective answers would count equally. Specifically, one set of answers would be selected at random with equal probability to determine pair-level rewards. In the *unequal status* treatment, participants were told that one member of the pair was randomly designated the 'Leader' and the other member the 'Follower,' and only the Leader's answers would count for determining pair-level rewards. Individuals in the control condition completed the tasks and accrued rewards individually, without interpersonal contact. Collaboration between pair members lasted ten minutes. Personal identifying information was never displayed. In the unequal-status condition, chat handles read "Leader" and "Follower."

Our approach builds on recent work to manipulate equal status during intergroup contact, recognized as a difficult endeavor (Riordan, 1978; Busby, 2021). Lowe (2021) experimentally

manipulates payment equality in an Indian cricket league, and finds that it does not undermine the beneficial effects of contact. Our design differs in that it directly manipulates relative status while maintaining payment equality across pair members, thereby holding constant incentives to collaborate. We discuss the complementary relationship between Lowe's and our approach and results in the Supplementary Materials (S-1.4 Main Results).

Figure 1: Chatter interface: sample chat

Please **take 2-3 minutes** to chat about which of the following values are more important to Mexicans in general (not just to you):

- "Having money and being successful at work" or
- "Having meaningful friendships"

**Take this opportunity to get to know the other person by exchanging a few messages with them.** Write in the boxed area at the bottom of this screen.

When you have finished chatting, choose your response:

**Your responses and the other person's will count equally.**

Please coordinate with the other person to move to the next screen at the same time.

Next page

**userY4B8J:** Success improves the quality of life

I agree with that

**userY4B8J:** If we, as Mexicans, don't pursue our goals, our corrupt politicians aren't going to do it for us

I think intelligence and how you use it is important

**userY4B8J:** I think intelligence together with values, and that's what we need to teach our children

Yes, exactly. But it seems like values are no longer taught in school, much less at home

**userY4B8J:** Exactly. These days, values aren't a priority and that's why our society is so divided and indifferent about what's happening

Write reply...

You see a person living on the street and just walk by. I feel a little guilty, but right now I can't help

Send

Time until finished: 0:00:04:24

Notes: This example pertains to the first task. Chat contents display part of a real conversation of a pair assigned to equal status. The instructions above the chat window were visible to participants during chat. Instructions and chat contents shown here were translated from Spanish by the authors.

**Main outcome variables.** We measured tolerance using incentivized behaviors. *Sharing* was measured through a dictator game where participants could choose to donate cash points, exchangeable for goods at an online store, to an anonymous participant with opposing partisan sympathies. *Willingness to dialogue* was measured as the response to an invitation to take part in a future online meeting with other participants, which we indicated would include opposing partisans and last 30 minutes. We aggregated the two measures into a standardized additive index for expository ease, to improve statistical power by reducing measurement error, and to mitigate issues with multiple-testing (Anderson, 2008).

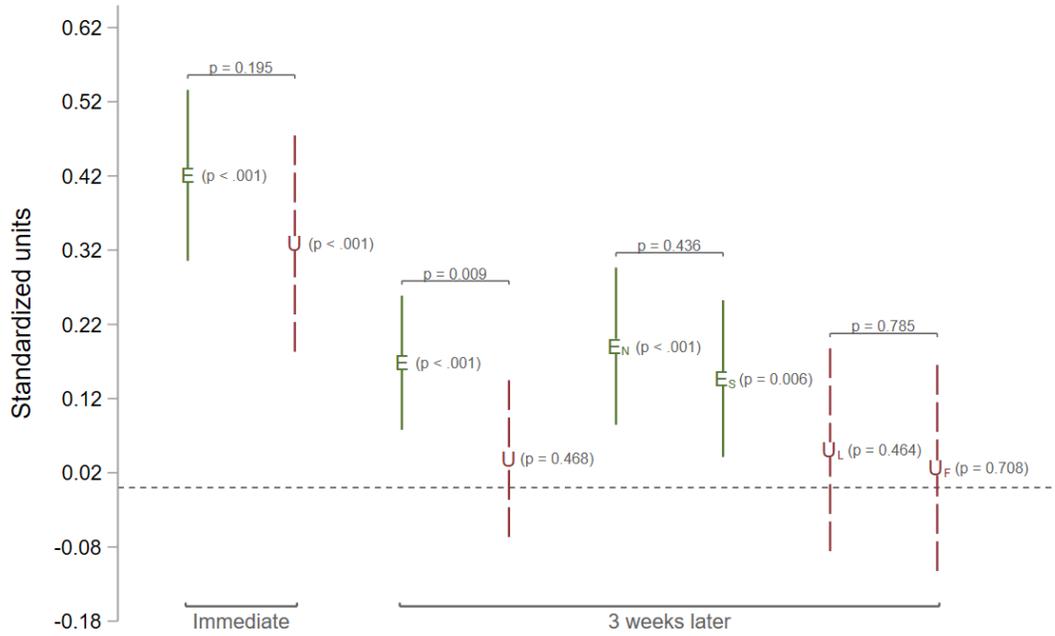
We selected these measures for four reasons. First, we sought to measure tolerance because it is central to Allport’s predictions about the beneficial effects of contact (Allport, 1954). Second, reducing intolerance is a key goal of interventions designed to ameliorate affective polarization, and behavioral measures are increasingly used for this purpose (Carlin and Love, 2013; Iyengar and Westwood, 2015; Iyengar et al., 2019; Finkel et al., 2020; Baldassarri and Page, 2021). Third, the specific tolerant behaviors we measure are vital to democracy. Democratic theorists view willingness to dialogue across partisan lines as key to problem solving and to elucidating a society’s priorities (Habermas, 1991; Barber, 2003). Additionally, sharing with out-group members, for example via taxation and redistribution, implies that people perceive the interests of others as legitimate (Alesina and Giuliano, 2011). Finally, incentivized behaviors are less susceptible to social desirability biases and experimenter demand effects than unincentivized survey questions because they make tolerant behavior costly (Bauer, Chytilová and Miguel, 2020).

**Estimates.** Participants were attentive to the experience of interpersonal contact. Three weeks after treatment, 88% of participants assigned to a contact condition recalled having chatted; only 10% of those assigned to the no-contact control reported (erroneously) having chatted (Supplementary Materials T-12).

Figure 2 displays intent-to-treat effects immediately after treatment and three weeks later (Supplementary Materials T-13 and T-17). The figure shows that, immediately after treatment, assignment to inter-group contact under equal status increased the tolerant behavior index by  $0.42\sigma$  compared to no contact, and unequal status contact increased it by  $0.33\sigma$  (leftmost two estimates; the  $p$ -value of the difference is 0.20). Three weeks later, assignment to equal-status contact increased the tolerant behavior index by  $0.17\sigma$  compared to no contact (third estimate from the left,  $p < .001$ ), while contact under unequal status did not (fourth estimate from the left,  $0.04\sigma$ ,  $p = 0.47$ ). At the three

week mark, moreover, the effect of assignment to equal status contact was larger than that of contact under unequal status by  $0.13\sigma$  ( $p = .009$ ). Figure 2, rightmost estimates show that tolerant behavior did not improve for either Leaders or Followers in the unequal status condition after three weeks. One implication of these findings is that, contrary to Allport's conjecture, equality may not be necessary to increase tolerance in the short run; however, consistent with his conjecture, equality may be necessary for the effect on tolerance to persist. We view the longer term results as more policy-relevant and novel (Paluck, Green and Green, 2019), and potentially less prone to experimenter demand effects.

Figure 2: Index of Tolerant Behavior Three Weeks After Contact



Note: Point estimates of intent-to-treat effects are represented by treatment assignment indicators:  $E$ =equal status,  $U$ =unequal status,  $E_N$ =equal status without revealing SES,  $E_S$ =equal status with SES revealed,  $U_L$ =unequal status, assigned as Leader,  $U_F$ =unequal status, assigned as Follower. Bars represent 95% confidence intervals. The vertical axis is measured in standard deviations compared to the no-contact control condition. Inline p-values compare each treatment arm to the no-contact control condition. Horizontal p-values correspond to difference-of-means tests between adjacent estimates. Source: Table T-13 in the Supplementary Materials.

Participants in equal-status contact experienced higher quality interactions with opposing partisans. The number of phrases expressing agreement, such as “you are right,” “yes,” and “I agree”, were 10% more common under equal vs. unequal-status assignment ( $p = .036$ ), although there is no difference when normalizing the number of agreement words by total words. The number of words in chat was more evenly distributed across members of a pair assigned to equal status, compared to unequal status ( $p = .016$ ) (Supplementary Materials Table T-20).

The literature on the contact hypothesis in social psychology identifies three major categories of mechanisms through which high-quality contact might increase tolerance: learning that the outgroup is more similar to the ingroup than one thought, reduced anxiety about the outgroup, and perspective taking or empathy (Pettigrew and Tropp, 2008). While it is beyond the scope of our analysis to pin down a precise mechanism, our findings might help guide future work. Differences between the two treatment groups do not reach statistical significance, but participants assigned to equality found it more palatable to imagine chatting with an outparty stranger while waiting in line for a routine task (anxiety reduction) and their opinions of a typical outparty voter were more positive than those assigned to inequality (anxiety reduction and perspective taking) (Supplementary Materials Table T-15). In contrast, the point estimates relevant to the learning hypothesis are in the positive direction for beliefs about shared values but in the negative direction for perceptions of outgroup honesty and intelligence among those assigned to equal vs. unequal contact (Supplementary Materials Table T-16).

We probed the robustness of our main findings by revealing participants’ real-world socioeconomic status to a random subset of pairs assigned to equal-status contact (treatment arm  $E_S$ ). We elicited SES information, prior to contact, by asking participants to choose, among five sets of images of house facades, kitchens, and bedrooms corresponding to different socioeconomic strata, those that best represented their own homes (Supplementary Materials Figure F-7). Exposure to real-world SES information increased participants’ ability to correctly predict their paired partner’s SES by 19% ( $p = 0.020$ ) (Supplementary Materials Table T-11).

Introducing information on real-world SES could in principle reinforce partisan animus (e.g., when pair members have unequal SES), undercutting the potential for contact to enhance tolerant behavior. The beneficial effect of equal status contact, however, proved robust to the revelation of SES information. Figure 2 shows that the effects of  $E_N$  and  $E_S$ , compared to  $C$ , are statistically indistinguishable. The effect of equal-status assignment also

did not change significantly when separately examining individuals whose real-world SES was higher, equal, or lower than their partner’s (Supplementary Materials Tables T-14 and T-22). An alternative interpretation of this finding is that the procedure we used was not effective at memorably conveying information about a partners’ SES.

Finally, we consider whether the difference in outcomes between the equal vs. unequal status conditions might be driven by the displeasure of participants assigned to be Followers. To study this possibility, we tested for differences between Leaders and Followers in willingness to complete the followup survey three weeks after treatment, as well as in dictator-game donations to outparty participants. Inconsistent with the displeasure alternative explanation, we find no differences in these variables (Supplementary Materials Tables T-2 and T-13).

We report results for feeling thermometers and other attitudinal outcomes in the Supplementary Materials (S-2.9 Outcome Variables). Like other studies of intergroup contact (Paluck, Green and Green, 2019; Scacco and Warren, 2018; Mousa, 2020; Lowe, 2021), we find positive effects on tolerant behavior without consistent change in related attitudes at the three week mark (Supplementary Materials T-15 and T-17).

## Discussion

Intensifying partisan polarization in many countries is straining democracy’s moorings. Sympathizers of opposing parties frequently self-sort into different neighborhoods, absorb news from different sources, and participate in different online social circles. When cross-partisan contact does occur, it is often brief and bitter, with intolerance exacerbated by social status differences. Nearly 200 years ago, Alexis de Tocqueville wrote that democracy thrives when citizens interact in the public square as equals (de Tocqueville, 2015). Our design put a modern version of de Tocqueville’s idea—and a decades-old conjecture that is central to contact theory—to the test by experimentally manipulating status within the interaction.

As it turns out, relative status in the interaction plays a key role in moderating the effects of intergroup contact on tolerant behaviors, consistent with Allport’s conjecture. When we place people in a situation of status equality, tolerant behaviors rise meaningfully and durably because of interpersonal contact. We find that revealing or withholding real-world SES makes no difference to the salutary effect of contact under equality.

We designed several features of our experiment to closely mimic online contact in everyday life. The chat app looks like other online interfaces, worked seamlessly across devices

that participants routinely use such as computers, tablets, and cell phones, and permitted participants to engage in chat wherever they preferred. In addition, we asked respondents to engage in collaborative tasks associated with natural topics of conversation. We thus believe that our intervention could be replicated and achieve similar results when staged online among other affectively polarized populations.

Our findings suggest practical ways of increasing mutual tolerance among opposing partisans. A version of intensive and costly in-person deliberation has recently been shown to improve cross-partisan understanding (Fishkin et al., 2021). We believe that online spaces for cross-partisan contact that put people on equal footing can generate prosocial and democracy-supporting behavior affordably and at large scale using a medium that is increasingly popular for political speech. Our results indicate that such spaces require only mild curation: conversations need not be orchestrated around political topics nor do opposing partisans have to be steered away from disagreement. Even under severe political polarization, enhancing tolerance of out-groups is within reach.

## Methods

**Sample recruitment.** We recruited participants from within Netquest’s survey panel in Mexico. Netquest, a private firm, has about 1.5 million panelists worldwide, and about 196,000 across Mexico. Invitations were issued by e-mail, in batches of several hundreds, to connect at a pre-specified time and date to conduct the actual study (further details on the logistics are provided further below). A very small fraction of panelists who reported not being registered to vote were screened out. We asked Netquest to make a special effort to recruit across the socioeconomic spectrum since lower-income deciles are under-represented in their pool of panelists. Our sample was slightly younger and more educated than the 2020 census population of adults, which is unsurprising for an online study (Supplementary Materials Table T-6).

**Randomization procedure.** Our experimental design employs blocked cluster randomization. Clusters consist of pairs of participants with opposing partisan sympathies. Treatment is assigned at the pair level. Information on participants’ partisan sympathies was gleaned from a survey question that asked which party the participant would vote for if the election for president were held today. If participants selected “MORENA,” we deemed them pro-incumbent. If participants selected any other political party, we deemed them

anti-incumbent. All pairs consisted of one pro- and one anti-incumbent individual thus defined.

To increase statistical power, we used information about participants gathered from a pre-treatment screening survey to create pairs and organize them into blocks. The variables we used for blocking include feeling thermometers toward the three largest political parties (PRI, PAN, and MORENA) and towards the incumbent president, as well as socioeconomic status. Socioeconomic status was elicited by asking respondents to choose, from among five sets of images of house facades, kitchens, and bedrooms corresponding to different socioeconomic strata, the set of images that best represented their own homes (Supplementary Materials Figure F-7). We validate these measures and provide more details on the composition of pro- and anti-incumbent partisan sympathizers in Supplementary Materials S-2.5 (Tables T-7, T-8, and T-10).

The algorithm we used to create blocked partnerships is illustrated in Figure F-1 in the Supplementary Materials. First, we divided participants into pro- and anti-incumbent partisan sympathizers based on the vote-choice question referenced above. Second, within these partisan groups, using an optimal Greedy algorithm, we selected four participants to form clusters of similar people by minimizing Mahalanobis distance (computed using feeling thermometers toward people who identify with the three largest parties, feelings toward AMLO, and the picture-based measure of SES). We used the `blockTools` software to implement this step (Moore, 2012). Third, we randomly assigned the sets of four similar partisan sympathizers to an analogous set of four cross-partisans. This creates a block. Fourth, within each block, we randomly assigned each participant to a cross-partisan partner, thus forming four pairs. We also did this for the control group, even though the control condition entailed no interaction between members of a pair. Within each block, we then randomly assigned pairs to experimental conditions ( $C, U, E_N, E_S$ ). For pairs assigned to the  $U$  condition, we additionally randomly assigned participants to be either the Leader ( $U_L$ ) or the Follower ( $U_F$ ).

Randomization achieved balance. Table T-3 in the Supplementary Materials shows balance across treatment conditions on blocking covariates as well as other important covariates measured prior to treatment. We also assess balance in the main sample, which includes only participants that completed the followup survey (fielded approximately 3 weeks after treatment). Table T-5 shows there is no differential attrition between equal ( $E$ ) vs. unequal status ( $U$ ) treatment arms. Attrition is slightly greater for participants assigned to the contact conditions compared to the no-contact control  $C$  (about 3% for  $E$  and 1% for  $U$ ), but

all treatment arms and the no-contact control are balanced on observables (Supplementary Materials S-1.2 Attrition). Finally, the sample that completed the survey fielded immediately after treatment is also balanced on pretreatment covariates (Supplementary Materials Table T-4).

**Online conversation logistics.** This section describes the procedure we employed to coordinate pair-wise online contact sessions.

The chat portion of the experiment took place on August 24-26, 2021 and August 30-September 1, 2021. On each of these six days, at a pre-specified time of day, participants followed a link that directed them to the study. They completed a baseline survey, were randomized to partners and to treatments as described above, completed the synchronous chat (except for those in pairs assigned to the no-contact control condition, who did not chat) and associated tasks, and took the endline survey, all within 40 minutes.

The day before each study day, we sent out an invitation email that included a link to a short screening survey to several hundred panelists. That survey alerted them to the fact that they might receive an invitation the next morning to join the study in the afternoon. We issued invitations only to those who stated that they were eligible to vote and available at the time of the study. We aimed to invite about one half MORENA supporters and one half supporters of other political parties, since anyone who logged on but could not be paired up could not participate in the study (recall that treatment assignment was conducted at the pair level). These dropped participants would still have spent time answering the baseline questionnaire and would have been paid a participation fee, wasting project resources.

The invitation email sent the day before the study that contained the link for the screening survey also included a link for the main study. Panelists were asked to log on at a specific time in the afternoon, to participate in the study. They were informed that they would not be able to participate if they logged on more than five minutes after the appointed time. Upon clicking on the link, participants were directed to the baseline survey, which was programmed on Qualtrics. At the start of the survey, participants were asked the questions that would later be used by our algorithm as blocking covariates, other than the vote choice question that we had asked the day before in the screening survey. Then, while participants completed the remainder of the baseline survey, we downloaded the blocking covariate data from Qualtrics to feed it to the algorithm that would implement our experimental design (i.e., blocking, pairing participants, and randomizing treatments). Simultaneously, we used this window of time to create online chatrooms for each pair. To do this, we utilized the Chatter

software, which has an API that researchers can use to create users, create chatrooms with specific instructions (i.e., our treatments), and assign users to chatrooms.

Fewer than 3% of participants that logged on were dropped from the study because they did not complete the pairing questions that appeared toward the start of the baseline survey within 13 minutes of the appointed start time (i.e., eight minutes after the end of the five-minute grace period) or, in a very few cases, because they failed the simple attention check question in the baseline survey.

At the 15 minute mark after the appointed time, participants could advance to their pre-assigned chatroom with their pre-assigned partner (participants who completed the baseline before that moment or before their partner arrived were asked to wait so that both members of a pair would begin the chat section of the study simultaneously).

Participants then chatted and completed the tasks outlined in the “Treatment Conditions” section (S-2.2, Supplementary Materials) while a visible eight-minute timer counted down from the moment that both members of the pair entered the chatroom. When the timer ran out, participants could advance to the endline survey, also programmed on Qualtrics.

Approximately three weeks later, all participants who were assigned to a pair at baseline were invited to complete a followup survey. The followup survey was in the field during September 17-24, 2021, with 75% of participants completing the survey the day it was distributed. Therefore, a majority of participants completed the followup 2.5 to 3.5 weeks after treatment.

**Estimation.** Our main results are estimates of intent-to-treat (ITT) effects based on the following equation:

$$Y_{i,p(i),k(p)} = \alpha + \beta_E E_{i,p(i)} + \beta_U U_{i,p(i)} + \gamma X_{p(i)} + \theta_{k(i)} + \varepsilon_{i,p(i),k(p)}, \quad (1)$$

where  $i$  indexes individuals,  $p$  indexes the pair to which the individual belongs, and  $k$  indexes the block to which the pair  $p$  belongs (reflecting the block randomization).  $Y_{i,p(i),k(p)}$  is the outcome variable of interest for individual  $i$  in block  $k$  (for instance the tolerant behavior index).  $E_{p(i)}$  is an indicator for assignment of pair  $p$  to the equal-status condition, and  $U_{p(i)}$  indicates assignment to unequal status. The control group  $C_{p(i)}$  is the omitted category, therefore, average outcomes for the control group are captured by  $\alpha$ .

Block dummies  $\theta_{k(i)}$  reflect the blocked randomization design.  $X_{p(i)}$  is a matrix of pre-treatment covariates, included to increase statistical power. These covariates include the outcome variable  $Y_{i,p(i),k(p)}$  measured at baseline and an indicator variable ( $Morena_i$ ) that

takes the value of 1 if individual  $i$  is a MORENA supporter. Standard errors are robust and clustered at the pair level.

Coefficient  $\beta_E$  estimates the intent-to-treat effect of pair-level assignment to equal-status contact ( $E$ ) compared to no contact ( $C$ ). Similarly,  $\beta_U$  estimates the intent-to-treat effect of assignment to unequal-status contact ( $U$ ) compared to no contact ( $C$ ). While the effects of contact vs. no contact are of interest and constitute the focus of most of the relevant empirical literature, our primary focus is testing whether, *conditional on contact taking place*, contact under equal status is more effective than contact under unequal status. This requires testing  $\beta_E$  against  $\beta_U$ . We perform these hypotheses tests and report the p-values in the regression tables.

We run additional specifications to estimate the effects of the randomizations we performed within the  $E$  and  $U$  conditions. These specifications are represented by equation 2.

$$Y_{i,p(i),k(p)} = \alpha + \beta_{E_N} E_{N i,p(i)} + \beta_{E_S} E_{S i,p(i)} + \beta_{U_L} U_{L i,p(i)} + \beta_{U_F} U_{F i,p(i)} + \gamma X_{p(i)} + \theta_{k(i)} + \varepsilon_{i,p(i),k(p)}, \quad (2)$$

As explained in the body of the article, a random subset of pairs assigned to  $E$  were assigned to information-revelation about their partner’s socioeconomic status (SES) ( $E_S$ ). In that condition, each individual in the pair learned which of the five sets of pictures (of house rooms and facades) was selected by their partner. In the rest of the individuals in pairs assigned to  $E$  (denoted  $E_N$ ) were not provided with information about their partner’s SES. Moreover, individuals within a pair assigned to  $U$  were respectively randomized into the roles Leader ( $U_L$ ) or Follower ( $U_F$ ). The variables  $E_{N i,p(i)}$ ,  $E_{S i,p(i)}$ ,  $U_{L i,p(i)}$  and  $U_{F i,p(i)}$  respectively indicate assignment of a pair to  $E_N$  and  $E_S$ , and assignment of an individual to  $U_L$  and  $U_F$ .

The effect of revealing real-world SES within a pair is estimated by comparing the coefficients  $\beta_{E_N}$  vs.  $\beta_{E_S}$ . The effect of being a Leader vs. a Follower (within a pair assigned to  $U$ ) is obtained by comparing the coefficients  $\beta_{U_L}$  vs.  $\beta_{U_F}$ .

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**Data materials and availability:** All data needed to evaluate the conclusions in the paper and the Supplementary Materials will be deposited in a public repository upon publication.

**References in Supplementary Materials:** Moore (2012); Santoro and Broockman (2022); Mohammad and Turney (2010, 2013); Lee (2009)