

**RESEARCH ARTICLE**

# Mitigating the Default? The Influence of Ingroup Diversity on Outgroup Trust

Kevin Winter\* and Kai Sassenberg\*<sup>†</sup>

Maintaining social cohesion in times of increasing diversity is a major challenge of modern societies. Mitigating defaults in group-based trust could be a solution because they are often driven by stereotypes and ingroup favoritism. Ingroup diversity could be a means to achieve such a mitigation given that it increases cognitive flexibility and cognitive flexibility changes defaults in trust – in the sense of increasing low and reducing high trust. We tested whether representing one's ingroup as high (compared to low) in diversity mitigates defaults in group-based trust using a variety of well-established manipulations of ingroup diversity and measures of trust. None of the four well-powered studies (total  $N = 885$ ), we conducted provided support for our hypothesis. However, an internal meta-analysis revealed a significant but very small effect in support of our prediction ( $r = 0.07$ , 95% CI [0.01, 0.14]). Thus, a diverse representation of the ingroup asserts a mitigating impact on group-based trust, but the size of the effect is very small. Thus, real world interventions should not rely on the current effect.

**Keywords:** outgroup trust; ingroup diversity; cognitive flexibility; stereotypes; ingroup favoritism

In modern societies, people are confronted with members of numerous other social groups – being it based on ethnicity, religion, education level, political ideology, or other social categories. In a diverse environment like this, trust in outgroups and their members is one of the central components of a peaceful and cooperative coexistence, because distrust can fuel intergroup polarization and conflicts (Esteban & Schneider, 2008). However, in general people trust ingroup members more than members of outgroups; and trust in many outgroups is a priori low (Foddy et al., 2009). Thus, it is important to understand how the low trust in outgroups – which seems to be the default in many cases – can be mitigated.

The key to reduce the influence of default trust towards different social groups might lie in a diverse representation of one's ingroup. There is good evidence that in particular a diverse mental representation of one's (superordinate) ingroup facilitates tolerant intergroup relations and, thus, potentially also intergroup trust (Wenzel et al., 2007). Furthermore, diversifying experiences in general are linked to less reliance on dominant responses when it comes to the evaluation of outgroups (e.g., less stereotype activation; Crisp & Turner, 2011). Therefore, we assume that construing one's ingroup as high in diversity could facilitate trust in outgroups that are by default seen as low in trustworthiness. We tested this relation between

ingroup diversity and outgroup trust, which has to the best of our knowledge not been investigated so far, in four experiments. In doing so, the theoretical basis for interventions against (inadequately) low outgroup trust is tested.

## Defaults in Group-Based Trust

Trust is the acceptance of vulnerability in a social interaction and is primarily based on positive expectations towards the respective interaction partner (Rousseau et al., 1998). In such an interaction, people often have to decide based on incomplete information whether to accept vulnerability (i.e., to trust) or not. One of the cues that guides these decisions is the group membership of the interaction partner. People trust ingroup as compared to outgroup members more because they expect their fellow group members to behave reciprocally (Foddy et al., 2009). Beyond this, different outgroups are perceived and treated differently. Based on stereotypes, people attribute certain characteristics and behaviors to a specific social group membership (Dovidio et al., 2010). Thus, if the characteristics people attribute to a certain outgroup signal low trustworthiness, they are less likely to trust and to behave cooperatively towards this outgroup (Mayer et al., 1995).

In line with this, Foddy and colleagues (2009) found that interaction partners from a positively stereotyped outgroup (i.e., nursing students) were preferred (i.e., trusted more) over members of a negatively stereotyped outgroup (i.e., economics students) in a monetary allocation game. Irrespective of the (positive or negative) outgroup

\* Leibniz-Institut für Wissensmedien, DE

<sup>†</sup> University of Tübingen, DE

Corresponding author: Kevin Winter ([k.winter@iwm-tuebingen.de](mailto:k.winter@iwm-tuebingen.de))

stereotype, however, they also observed participants to trust ingroup members more. Taken together, there seem to be two major sources of defaults in group-based trust. First, expectations of reciprocity cause higher default levels of trust in ingroups as compared to outgroups. Second, specific stereotypes determine whether an outgroup is by default seen as high or low in trustworthiness. The question that remains is how these defaults can be mitigated. According to earlier research, ingroup diversity might be the answer to this question.

### Ingroup Diversity and Intergroup Relations

Diversity is a feature of a social group that reflects the amount of objective or subjective differences between members of that group (van Knippenberg & Schippers, 2007). The impact of (especially ethnic) diversity on intergroup relations has been extensively studied across different disciplines. This resulted in an ongoing discussion in which some see the predominantly positive effects of diversity, whereas others perceive negative effects more likely (for a summary see Hewstone, 2015). This debate, however, mostly focusses on outgroup members' contribution to diversity at the societal level and its consequences. Therefore, no clear conclusions can be derived from this research regarding the effects of ingroup diversity.

One theoretical approach that explicitly predicts the consequences of *ingroup* diversity for outgroup judgments is the ingroup projection model. In line with the assumptions of the model, it has been shown that outgroup tolerance can be promoted by mentally representing a superordinate ingroup as highly diverse (Wenzel et al., 2007). For instance, when a diverse (vs. simple) representation of the superordinate ingroup (i.e., Europeans) is activated, participants' attitudes toward a subordinate outgroup (i.e., Polish) improved, because they perceived their subordinate ingroup (i.e., Germans) as less prototypical for the superordinate ingroup (Waldzus et al., 2003). Thus, the ingroup projection model is informative regarding the improvement of outgroup attitudes and potentially also outgroup trust through ingroup diversity. However, it remains silent when it comes to a general mitigation of default levels of trust – which would also include a reduction of trust toward outgroups high in trustworthiness as well as toward ingroups.

In addition, the focus on diversity at the level of a superordinate category incorporates changes in the mental representation of both subordinate ingroup and subordinate outgroup. This makes it hard to disentangle the effects of (subordinate) ingroup diversity from changes in (subordinate) outgroup perceptions within this model. A more suitable theoretical framework for the current purpose – that is, reducing the influence of default levels of group-based trust through ingroup diversity – can be found in the literature on diversity and cognitive flexibility.

### Diversifying Experiences Foster Cognitive Flexibility

In their Categorization-Processing-Adaptation-Generalization (CPAG) model, Crisp and Turner (2011) promote the idea that various forms of diversifying experiences

(among other things living in a diverse context) can lead to a change in information processing, because they are inconsistent with established mental routines (e.g., stereotypes). To resolve these inconsistencies, one needs to broaden one's mental scope and consider alternatives beyond the dominant one. This in turn, leads to a state of cognitive flexibility which is characterized by an inhibition of default responses such as stereotypes (Crisp & Turner, 2011). Thus, experiencing diversity can reduce the influence of default outgroup judgements via increased cognitive flexibility.

On a broader level, cognitive flexibility is enhanced through the mental simulation of conflicting alternatives (Kleiman & Enisman, 2018; Winter et al., 2021) – which should also be involved in inconsistency resolution after diversifying experiences. With regard to the current research and in line with the CPAG model, increased cognitive flexibility has an impact on outgroup related judgements. It reduces perceived differences between ingroup and outgroup (Stern & Kleiman, 2015) as well as the reliance on stereotypes (Kleiman et al., 2014; Sassenberg & Moskowitz, 2005), and, most relevant in the current context, it enhances trust in previously marginalized outgroups (Vasiljevic & Crisp, 2013). This means that by increasing cognitive flexibility, default outgroup judgements become less influential.

Because cognitive flexibility reduces the reliance on *default* responses (such as outgroup stereotypes), it is necessary to consider how these responses initially look like. Taking this into account, Winter and colleagues (2021) showed that the impact of a cognitive flexibility manipulation on outgroup trust depended on participants' initial level of outgroup trust. When initial outgroup trust was low, high cognitive flexibility increased outgroup trust, whereas it reduced outgroup trust when initial outgroup trust was high. Thus, cognitive flexibility led to a mitigation of participants' default outgroup trust.

In the current research we test the role of high (vs. low) ingroup diversity as another elicitor of this process and its outcomes. More precisely, we hypothesize that representing one's ingroup in terms of low diversity facilitates default tendencies in group-based trust (i.e., higher ingroup than outgroup trust), whereas these tendencies should be less pronounced when representing the ingroup as highly diverse.

Initial support for this idea comes from Xin and colleagues (2016). They demonstrated that participants who construed their social identity as more complex (either measured or manipulated) reported higher levels of outgroup trust. Although the focus was here on the complexity of the social identity rather than the complexity or diversity of the ingroup, high social identity complexity<sup>1</sup> can be seen as a form of diversifying experience (Crisp & Turner, 2011; Roccas & Brewer, 2002).

### Overview of the Current Studies

All studies followed the same principal set-up. We manipulated ingroup diversity using a variety of well-established procedures. In Study 1, participants had to think about differences (vs. similarities) of their ingroup (Waldzus et

al., 2003). In Studies 2 and 4, we let participants judge their ingroup in terms of a diverse (vs. non-diverse) set of characteristics (Hutchison et al., 2006). Finally, in Study 3, we asked participants to draw a diverse (vs. simple) mind-map representing characteristics of their ingroup (Xin et al., 2016).

Afterwards, we assessed group-based trust in each study with one behavioral and one attitudinal measure. In Study 1, we measured behavioral trust with an information pooling game (adapted from Steinel et al., 2010). In Studies 2 and 3, we used a well-established version of the trust game to assess group-based trust (Xin et al., 2016). In Study 4, we asked participants about their intentions to behave trustfully in different scenarios. Across all studies, we used the same attitudinal measure of outgroup trust (Noor et al., 2008).

Default tendencies in group-based trust were operationalized differently across studies. In Studies 1 and 3, the trust measures were assessed either in relation to the ingroup or in relation to an outgroup to implement high as compared to low default trust. In contrast, in Study 2, the trustee group was an outgroup either low (i.e., insurance agents) or high (i.e., aviators) in stereotypic trustworthiness. Finally, in Study 4, we assessed initial outgroup trust (toward asylum seekers) as an interindividual difference variable at the outset of the study.

In sum, we applied a wide variety of different operationalizations of all three key constructs (ingroup diversity, initial trust, and resulting trust) and thus operationalized the constructs broadly. Across all studies we used the same preregistered criteria to exclude participants from analyses. Because central parts of the materials were text-based and required good command of German language, we only included participants who spoke German fluently. Furthermore, participants older than 35 years were excluded, because they represent a rather peripheral subgroup of the student population and might thus not self-identify with students (i.e., the ingroup used in all diversity manipulations). Psychology students were excluded, because they might have come across materials and procedures in their undergrad studies and, therefore, be suspicious about our hypothesis. After applying these criteria, we checked for statistical outliers in the main analysis. Participants with absolute studentized deleted residuals  $|SDR| > 2.69$  were excluded as statistical outliers (Neter et al., 1996). We report whether including these participants affected results. In addition, we report all measures and manipulations used. In all studies, sample size was determined before any data analysis.

## Study 1

### Method

#### Design and participants

We conducted an online experiment with a 2 (ingroup diversity: low vs. high)  $\times$  2 (trustee group: outgroup vs. ingroup) between-subjects design. Unfortunately, we did not conduct an a priori power analysis for this study, but rather aimed at recruiting 70 participants per cell (i.e.,  $N = 280$ ; without documenting a particular reason for this decision). Overall, 290 participants took part in our exper-

iment; we had to exclude two of them based on our preregistered criteria (see <https://aspredicted.org/qy4hy.pdf>). Including them did not affect results. The final sample, thus, consisted of  $N = 288$  participants (205 female, age:  $M = 21.89$ ,  $SD = 2.92$ , range = 18–35). With this sample size we would be able to detect a minimum effect of  $f = 0.166$  (equivalent to  $d = 0.33$ ) with  $\alpha = 0.05$  and a statistical power of  $(1-\beta) = 0.80$  in an analysis of variance (ANOVA; fixed effects, main effects and interaction; numerator  $df = 1$ , number of groups = 4). Such an effect would be considered small-to-medium in size (Cohen, 1988). Participants could take part in a lottery of 30 vouchers each worth 10 Euros.

#### Procedure

First, participants were asked some questions about their studies to make their ingroup (i.e., students of the same subject) salient. For the same purpose, a scale assessing social identification with said ingroup was presented.

Second, we manipulated ingroup diversity with a procedure adapted from previous research (Waldzus et al., 2003; Experiment 2). We asked participants to think about how they perceive students of their own subject. In the *low diversity* condition, participants were supposed to write about *similarities* of these ingroup members, whereas participants in the *high diversity* condition had to name *differences*. They had three minutes to list as many similarities/differences as came to their mind.

Next, we measured behavioral trust. We asked participants to imagine a scenario in which they were part of a team of students that participated in a scientific competition (for the complete instructions see Supplement). Their team consisted of other students of their own subject. We informed participants that after a while of working together, their team recognized that they would need the help of another team in order to win the competition. In the *outgroup* condition, this team consisted of students of *another subject* than their own. In the *ingroup* condition, it was another team of students of participants' *own subject*.<sup>2</sup> Participants were made aware that giving too much information to the other group might result in being exploited (i.e., the other team could use the information to win the competition alone). However, a certain amount of information should be transferred in order to facilitate successful cooperation. In the actual information pooling game that followed (adapted from Steinel et al., 2010), participants received a list of 16 pieces of information related to their project (e.g., own survey results, timeline of the project, potential target group, legal regulations). Eight pieces of information were marked as public (i.e., everybody had access to this information), eight as private (i.e., only members of the own team knew about this). Participants were asked whether they wanted to share each piece of information with the other group or not. Sharing (especially private) information with the other group was used as an indicator of behavioral trust. After the information pooling game, participants responded to a measure of attitudinal trust, manipulation checks, and further exploratory questions.

## Measures

### Manipulation check

Two items on a 7-point scale ('How versatile/multifaceted do you perceive the students of your own subject?'; 1 = *not at all* to 7 = *very much*) assessed perceived ingroup diversity. The items were subsumed to one scale,  $r(288) = 0.76, p < 0.001$ .

### Behavioral trust

As a measure of behavioral trust, we summed up the number of private information sent to the other group. Thus, values ranged between 0 and 8, respectively. Sharing private and public information were weakly correlated,  $r(288) = 0.13, p = 0.023$ .

### Attitudinal trust

We assessed attitudinal trust in the other group (consisting of either ingroup or outgroup members) with six items on a 7-point scale (1 = *does not apply at all* to 7 = *does fully apply*; e.g., 'The other group will try to be fair';  $\alpha = 0.83$ ; adapted from Noor, Brown, & Prentice, 2008). Attitudinal trust was correlated with sharing private information,  $r(288) = 0.24, p < 0.001$ , but not with sharing public information,  $r(288) = 0.08, p = 0.197$ . This validates our assumption that sharing private (but not public) information can be seen as an indicator of trust.

Due to a technical error, no verbal scale anchors were presented to participants who completed the study via mobile devices. This did not affect completion of the behavioral trust measure. However, the results for the manipulation check and the attitudinal trust measure should be handled with care.

## Results

### Manipulation check

To test whether our manipulation enhanced perceived ingroup diversity, we conducted a two-way ANOVA containing the experimental factors trustee group (ingroup vs. outgroup) and ingroup diversity (low vs. high). Counter to our expectations, there was no main effect of our manipulation of ingroup diversity,  $F(1, 284) = 0.21, p = 0.644, \eta^2_{\text{partial}} = 0.001, 90\% \text{ CI } [0.00, 0.01]$ . The other effects were not significant,  $F_s < 2.18, p_s > 0.140$ .

### Behavioral trust

To test our hypothesis that the difference between ingroup and outgroup trust would be smaller when ingroup diversity is high (vs. low), we calculated a two-way ANOVA. Participants tended to share more private information with the ingroup ( $M = 4.77, SD = 1.75$ ) as compared to the outgroup ( $M = 4.44, SD = 1.56$ ),  $F(1, 284) = 2.82, p = 0.094, \eta^2_{\text{partial}} = 0.01, 90\% \text{ CI } [0.00, 0.04]$ . Although the means pointed into the expected direction (for  $M$ s and  $SD$ s see Supplement), this was not moderated by ingroup diversity,  $F(1, 284) = 0.36, p = 0.548, \eta^2_{\text{partial}} = 0.001, 90\% \text{ CI } [0.00, 0.02]$ . Thus, this result was not in line with our hypothesis. The main effect of ingroup diversity was not significant,  $F < 1$ .

### Attitudinal trust

We conducted a two-way ANOVA with attitudinal trust as dependent variable. Contradicting our hypothesis, there was no interaction effect of ingroup diversity and trustee group,  $F(1, 284) = 0.04, p = 0.836, \eta^2_{\text{partial}} = 0.0001, 90\% \text{ CI } [0.00, 0.01]$ . The other effects were not significant either,  $F_s < 1$ .

## Discussion

The results of Study 1 do not support our hypothesis. Even the 'default' difference in trust towards ingroup and outgroup was only of descriptive nature. It is possible that the expected default pattern was absent, because participants perceived students of another subject as ingroup to some degree (students in general). Therefore, we chose a higher-level ingroup (i.e., students of the same university) and clearly separable outgroups (aviators and insurance agents) in Study 2. More important, however, the hypothesized interaction with ingroup diversity did not occur. The manipulation check remained unaffected by the manipulation. Therefore, we altered the manipulation of ingroup diversity in Study 2. In addition, we chose another better-established measure of behavioral trust. Finally, we did not investigate differences between ingroup and outgroup trust, but rather between two outgroups of different stereotypic trustworthiness (i.e., aviators and insurance agents) inspired by the findings of Foddy and colleagues (2009).

## Study 2

### Method

#### Design and participants

Study 2 was an online experiment with a 2 (ingroup diversity: low vs. high)  $\times$  2 (default outgroup trust: low vs. high) between-subjects design. Lacking a reliable effect size estimate, we assumed a small to medium effect size ( $f = 0.175$ , equivalent to  $d = 0.35$ ) in an ANOVA (fixed effects, main effects and interaction; numerator  $df = 1$ , number of groups = 4). An a priori power analysis using G\*Power (Faul et al., 2007) with  $\alpha = 0.05$  and  $(1-\beta) = 0.80$  indicated a desired sample size of  $N = 259$ . We recruited 259 participants via the university's mailing list. Based on our preregistered criteria (see <https://aspredicted.org/4db3d.pdf>), we had to exclude one participant, which did not affect results. Thus, our final sample consisted of  $N = 258$  participants (190 female, age:  $M = 22.60$  years,  $SD = 3.13$ , range = 18–31). Participants had the chance to win one out of 30 vouchers each worth 10 Euros.

#### Procedure

At the outset of the study, we manipulated ingroup diversity (low vs. high). Following the procedure of Hutchison and colleagues (2006), participants were asked to judge the percentage (from 0 to 100) of students of their university to whom a given characteristic applies. The questions were grouped into five personally relevant topics (politics, religion, social engagement, sports, place of birth). In the *high diversity* condition, there was one question for each topic (e.g., 'What percentage of students of your university

would define their political orientation as follows?') which was followed by five more detailed options, respectively (e.g., 'liberal', 'green', 'conservative', 'social democrat', 'socialist'). In the *low diversity* condition, only one general question was asked per topic (e.g., 'What percentage of students of your university is interested in politics or political topics?'). According to Hutchison and colleagues (2006), this manipulation should make either intragroup similarities (i.e., low diversity) or differences (i.e., high diversity) salient. To strengthen the manipulation, participants then had to write some keywords regarding either the similarities (low diversity) or differences (high diversity) of the students at their university. Afterwards, the manipulation check was completed.

In the second part of the study, participants were introduced to a trust game that we used as a behavioral measure of outgroup trust (Xin et al., 2016). Participants were told to imagine playing a game with another person, that the objective of the game was to send money to the other person, and that they themselves were supposed to take the role of the *sender* in this game. Then, participants learned that both parties had 10 Euros in the beginning of the game and that the other person would receive three times the amount of money that the participant decided to send (i.e., 3\*X Euros). Importantly, the other person (i.e., the receiver) then had the chance to transfer any amount of money between 0 and 3\*X Euros back to the participant (i.e., the sender). This was explained with an example. Before being able to proceed to the actual game, participants were asked three control questions to ensure they had correctly understood the rules. Participants only arrived at the actual game phase after correctly answering the control questions, which was checked by the experimental software. In the actual game, participants were then asked to transfer any amount of money between 0 and 10 Euros to the other person.

Here, we manipulated the second experimental factor. In one condition, participants played the game with a person who belonged to a rather untrustworthy outgroup (i.e., insurance agents). In the other condition, the receiver belonged to a rather trustworthy outgroup (i.e., aviators). We chose insurance agents and aviators as outgroups, because several pilot studies showed differences in perceived trustworthiness between these two groups (see Supplement).

Finally, we measured attitudinal trust towards the outgroup which the other person in the trust game belonged to (i.e., either aviators or insurance agents).

## Measures

### Manipulation check

We used two items on a 7-point scale (1 = *do not agree at all* to 7 = *fully agree*) to assess how 'different' and how 'similar' participants perceived students at their university. These items were taken from previous research using the same manipulation (Hutchison et al., 2006). After recoding the item 'similar' the two items were subsumed to one scale with higher scores indicating higher ingroup diversity,  $r(258) = 0.78, p < 0.001$ .

### Behavioral outgroup trust

As a measure of behavioral outgroup trust we used the amount of money sent to the receiver in the trust game. Thus, the possible range of values lies between 0 and 10 with only integers being allowed.

### Attitudinal outgroup trust

In addition, we measured attitudinal trust in either aviators or insurance agents with the same scale as in Study 1 (adapted from Noor et al., 2008;  $\alpha = 0.86$ ). The behavioral and the attitudinal measures of outgroup trust were correlated,  $r(258) = 0.41, p < 0.001$ .

## Results

### Manipulation check

In order to test whether our manipulation of ingroup diversity was successful, we conducted a two-way ANOVA with trustee group (insurance agents vs. aviators) and ingroup diversity (low vs. high) as between-subject factors. Perceived ingroup diversity was the dependent variable. There was a marginal effect of trustee group on perceived ingroup diversity,  $F(1, 254) = 3.14, p = 0.078, \eta^2_{\text{partial}} = 0.01, 90\% \text{ CI } [0.00, 0.04]$ .<sup>3</sup> When participants later interacted with the group high in trustworthiness (i.e., aviators;  $M = 4.75, SD = 1.20$ ) they tendentially perceived their ingroup as more diverse than when they later interacted with the group low in trustworthiness (i.e., insurance agents;  $M = 4.48, SD = 1.29$ ). However, given the small effect size we do not consider this a meaningful difference that deserves further attention. More interestingly, the manipulation of ingroup diversity had no effect on perceived ingroup diversity,  $F(1, 254) = 2.65, p = 0.105, \eta^2_{\text{partial}} = 0.01, 90\% \text{ CI } [0.00, 0.04]$ . The interaction effect of both experimental factors was not significant,  $F < 1$ .

### Behavioral outgroup trust

In a two-way ANOVA we tested our hypothesis that the difference in trust in aviators and insurance agents would be smaller in the high diversity condition than in the low diversity condition. A significant main effect of trustee group occurred,  $F(1, 254) = 18.62, p < 0.001, \eta^2_{\text{partial}} = 0.07, 90\% \text{ CI } [0.03, 0.12]$ . Participants sent more money to aviators ( $M = 5.86 \text{ Euros}, SD = 3.07$ ) as compared to insurance agents ( $M = 4.23, SD = 2.88$ ). Although the pattern pointed into the expected direction, the predicted interaction effect of trustee group and ingroup diversity was not significant,  $F(1, 254) = 1.84, p = 0.176, \eta^2_{\text{partial}} = 0.01, 90\% \text{ CI } [0.00, 0.03]$ .<sup>4</sup> The main effect of ingroup diversity was not significant,  $F < 1$ .

Simple comparisons between conditions revealed that there was a significant difference between aviators ( $M = 6.08, SD = 3.03$ ) and insurance agents ( $M = 3.97, SD = 2.72$ ) in the low diversity condition,  $F(1, 254) = 16.88, p < 0.001, \eta^2_{\text{partial}} = 0.06, 90\% \text{ CI } [0.02, 0.12]$ . However, the difference between aviators ( $M = 5.61, SD = 3.13$ ) and insurance agents ( $M = 4.52, SD = 3.05$ ) was less pronounced in the high diversity condition,  $F(1, 254) = 4.18, p = 0.042, \eta^2_{\text{partial}} = 0.02, 90\% \text{ CI } [0.00, 0.05]$ . This can be seen as partial support for our hypothesis that high ingroup diversity

would reduce the difference between outgroups low and high in trustworthiness.

#### Attitudinal outgroup trust

The same pattern of results as for behavioral outgroup trust also emerged for the attitudinal measure. There was a significant main effect of outgroup trustworthiness on attitudinal outgroup trust,  $F(1, 254) = 121.23, p < 0.001, \eta^2_{\text{partial}} = 0.32, 90\% \text{ CI } [0.25, 0.39]$ . Participants reported more trust in aviators ( $M = 4.64, SD = 0.89$ ) than in insurance agents ( $M = 3.40, SD = 0.91$ ). This validates our choice of outgroups as being either low or high in trustworthiness. Against our expectations, this main effect was not qualified by an interaction with ingroup diversity, although it pointed in the expected direction,  $F(1, 254) = 1.79, p = 0.183, \eta^2_{\text{partial}} = 0.01, 90\% \text{ CI } [0.00, 0.03]$ . The main effect of ingroup diversity was not significant,  $F < 1$ .

#### Discussion

Study 2 did once again not provide solid evidence for our hypothesis, even though we successfully implemented the intended difference between the trustee groups. Again, the direction of the interaction was as expected, but not statistically significant. We did find the hypothesized default difference in trust towards insurance agents and aviators in the low ingroup diversity condition. However, high ingroup diversity only descriptively reduced this default difference in outgroup trust. As in Study 1, the manipulation check suggests that the ingroup diversity manipulation did not work out properly, although we used a well-established manipulation of ingroup diversity including the manipulation check items reported in the same research (Hutchison et al., 2006). Therefore, we decided to test a different manipulation of ingroup diversity in Study 3. In addition, we changed the groups that were part of the trust game comparing trust towards an ingroup (i.e., students) with trust towards an outgroup (i.e., asylum seekers).

### Study 3

#### Method

##### Design and participants

This study had a 2 (ingroup diversity: low vs. high)  $\times$  2 (trustee group: outgroup vs. ingroup) between-subjects design and was conducted in a paper-and-pencil format. Since the methods used in this study were comparable to those of Xin and colleagues (2016, Study 2), we took their reported effect size as an estimate for our expected effect size ( $f = 0.295$ ). Taking into account that the true effect might be smaller, we aimed at a higher statistical power of 95%. An a priori power analysis with G\*Power (Faul et al., 2007) indicated a desired sample size of  $N = 152$  for a two-way ANOVA with  $(1-\beta) = 0.95$  and  $\alpha = 0.05$ . We recruited 156 participants in the university library, where they also filled in the questionnaire. In addition to our preregistered criteria (see <https://aspredicted.org/ep243.pdf>), we also excluded participants who did not complete the ingroup diversity manipulation or reported to not have followed the prescribed order of the questionnaire ( $n = 14$  excluded), which did not change results. While we could

control for this by technical means (i.e., forced responses) in the preceding (online) studies, we were not able to do so in Study 3, because of the paper-and-pencil format. A sample of  $N = 142$  participants (94 female, age:  $M = 22.85$  years,  $SD = 2.57$ , range = 19–34) remained for the analyses. Participants received a small bar of chocolate as compensation.

#### Procedure

In the beginning of the experiment, participants were asked some questions about their studies in order to make their ingroup (i.e., students of the same university) salient. For the same purpose, we measured social identification with their ingroup.

In the second part, we manipulated ingroup diversity. The procedure was closely adapted to the one used by Xin and colleagues (2016) to manipulate social identity complexity. Participants were told that they would have to draw a so-called 'Group Identity Map'. In the *low diversity* condition, participants were instructed that group identity maps are most suitable to depict the 'most essential and basic characteristics of a group'. In the *high diversity* condition, the benefits of group identity maps for gathering the 'most comprehensive characteristics of a group' were highlighted. On the next page, participants were given an example of a group identity map (the depictions are provided in the Supplement). In the middle of the map, there was a big circle containing the name of an unrelated social group (i.e., celebrities). In the *low diversity* condition, four branches were drawn from the central node that pointed to one characteristic of the group each (i.e., successful, buy expensive things, are good looking, being in the spotlight). In the *high diversity* condition, eight branches were depicted with four additional characteristics (i.e., talented, role model, aloof, fastidious). Then, in both conditions, participants were asked to draw their own group identity map with characteristics that best described their ingroup. As a starting point, only the central node was present containing the description 'Students of the university of Tübingen'.

After drawing the group identity map, participants were asked to play a trust game. The instructions were the same as used in Study 2. Again, we manipulated the group membership of the person with who participants played the game (i.e., the trustee). In the *outgroup* condition, we asked participants to imagine playing the game with an asylum seeker. At the time of the study, the pros and cons of receiving asylum seekers were heavily debated and we were sure that none of the participants belonged to this group. In the *ingroup* condition, the imagined other person was a student of participants' own university. After that, attitudinal trust in the respective group was assessed and participants indicated whether they had followed the prescribed order of the questionnaire.

#### Measures

##### Manipulation check

As an objective manipulation check we counted the number of nodes participants drew in their group identity maps (as in Xin et al., 2016).

### Behavioral trust

As in Study 2, behavioral trust was measured with the amount of money (i.e., 0 to 10 Euros) sent in a trust game.

### Attitudinal trust

With the same scale as before we measured attitudinal trust in either the ingroup or the outgroup ( $\alpha = 0.75$ ). The measures of trust were correlated,  $r(142) = 0.24, p = 0.004$ .

## Results

### Manipulation check

A two-way ANOVA was conducted to see whether our manipulation of ingroup diversity affected the number of nodes drawn. Trustee group was used as additional between-subjects factor. The ANOVA included both experimental factors and their interaction. Indeed, there was a significant main effect of ingroup diversity on the number of nodes,  $F(1, 138) = 19.77, p < 0.001, \eta^2_{\text{partial}} = 0.13, 90\% \text{ CI } [0.05, 0.21]$ . Participants in the high diversity condition ( $M = 6.96, SD = 2.84$ ) drew more nodes than participants in the low diversity condition ( $M = 5.07, SD = 2.28$ ). None of the other effects was significant,  $F_s < 2.20, p_s > 0.140$ . Thus, the manipulation of ingroup diversity was successful in this study.

### Behavioral trust

We performed a two-way ANOVA to test our hypothesis that the difference between ingroup and outgroup trust would be smaller in the high (vs. low) diversity condition. There was a main effect of trustee group,  $F(1, 138) = 10.65, p = 0.001, \eta^2_{\text{partial}} = 0.07, 90\% \text{ CI } [0.02, 0.15]$ . Counter to our expectations, participants transferred more money to the outgroup (i.e., asylum seekers;  $M = 7.15, SD = 3.17$ ) as compared to their ingroup (i.e., students;  $M = 5.52, SD = 2.76$ ). The predicted interaction effect of ingroup diversity and trustee group was in the expected direction, but not significant,  $F(1, 138) = 2.30, p = 0.131, \eta^2_{\text{partial}} = 0.02, 90\% \text{ CI } [0.00, 0.07]$ .<sup>5</sup> Simple comparisons were in line with our reasoning. The difference between money sent to ingroup ( $M = 5.09, SD = 2.51$ ) and outgroup ( $M = 7.49, SD = 3.16$ ) was significant in the low diversity condition,  $F(1, 138) = 11.27, p = 0.001, \eta^2_{\text{partial}} = 0.08, 90\% \text{ CI } [0.02, 0.15]$ . However, there was no difference between ingroup ( $M = 5.94, SD = 2.96$ ) and outgroup ( $M = 6.82, SD = 3.19$ ) in the high diversity condition,  $F(1, 138) = 1.55, p = 0.216, \eta^2_{\text{partial}} = 0.01, 90\% \text{ CI } [0.00, 0.05]$ . The main effect of ingroup diversity was not significant,  $F < 1$ .

### Attitudinal trust

The same ANOVA was conducted with attitudinal trust as dependent variable. The pattern of results was similar to the behavioral trust measure. However, we did not find a significant interaction of ingroup diversity and trustee group,  $F(1, 138) = 1.09, p = 0.298, \eta^2_{\text{partial}} = 0.01, 90\% \text{ CI } [0.00, 0.05]$ . All other effects were not significant,  $F_s < 1.26, p_s > 0.263$ .

## Discussion

As in the previous studies, our hypothesis was not compellingly supported by the data. As before, we observed

a tendency into the expected direction, but no statistically significant interaction effect of ingroup diversity and default trust. In the low diversity condition, there was a difference in trust between the two trustee groups. This difference was absent in the high diversity condition. This is in line with our reasoning, because a default difference in group-based trust could be diminished through our manipulation. Interestingly and counter to our expectations, participants did not favor their ingroup by default, but rather the outgroup. This finding is in contrast to previous research (Foddy et al., 2009) and might be caused by the specific outgroup chosen in the current study. Given that participants were taken from a (usually rather left-winged) student sample, it seems plausible that they would transfer rather high amounts of money to asylum seekers – whether that represents higher trust or not. Additionally, this tendency could have been reinforced by social desirability effects or perceptions of the outgroup's neediness. Notwithstanding this unexpected default pattern, high ingroup diversity mitigated the difference in trust between ingroup and outgroup that was present under conditions of low ingroup diversity and, thus, in principle supports our reasoning.

To gain further insights in how to shift the actual group-based trust away from the default tendencies via ingroup diversity, we carried out some changes in Study 4. First, we assessed default group-based trust (in asylum seekers) at the outset of the study. Second, we returned to the manipulation of ingroup diversity from Study 2, because it is better established than the one used in Study 3 and both manipulations resulted in similar effects. Finally, we measured intentions to engage in trustful behavior towards the outgroup.

## Study 4

### Method

#### Design and participants

We conducted an online experiment with two conditions (ingroup diversity: high vs. low). Initial outgroup trust was assessed as continuous predictor. We aimed at collecting 199 participants, as we assumed a small effect size ( $f^2 = 0.04$ ) for the test of  $R^2$  increase in a regression with one tested predictor out of a total number of three predictors,  $\alpha = 0.05$ , and  $(1-\beta) = 0.80$ . We recruited 225 participants via the university's mailing list. Based on our preregistered (see <https://aspredicted.org/a4dp2.pdf>) criteria we excluded 28 participants, which did not influence the reported results. The final sample consisted of  $N = 197$  participants (117 female, age:  $M = 23.45$  years,  $SD = 3.43$ , range = 18–35). Participants could take part in a lottery of 10 vouchers each worth 10 Euros.

#### Procedure

At the outset of the study, participants had to rate 20 social groups regarding their trustworthiness. This procedure was used to cover the assessment of initial outgroup trust (in asylum seekers). Then, to make their ingroup salient, participants had to indicate some demographic data related to their studies (university, semester, subject). This

time, we chose students at German universities (instead of only participants' own university) to provide more room for a diverse ingroup representation. This was followed by the manipulation of ingroup diversity which was used in Study 2 with slight adaptations. We changed the topics of the questions as to be more relevant to students in particular (i.e., side job, residence, hobbies, social media use, politics, staying abroad) and provided participants in the *high diversity* condition with six (instead of five) options to rate their ingroup to allow for a more diverse ingroup representation (for the complete materials see Supplement).

In the following part of the study, participants were instructed to put themselves into six hypothetical situations. All situations were described in a few sentences and involved an interaction with at least one member of the outgroup (i.e., asylum seekers). Finally, participants were asked to judge how likely they would show a specific behavior in each situation. The situations covered all three dimensions of trustworthiness equally (i.e., for ability, benevolence, and integrity, respectively; Mayer et al., 1995). Furthermore, each dimension was represented with one situation that involved a negative response (implying low trustworthiness) and one that involved a positive response (implying high trustworthiness). For instance, the dimension of *benevolence* (*high trustworthiness*) was addressed with the following situation:

You live in an apartment building with your three-year-old child. In the afternoon you have a very important appointment and your babysitter has called in sick at short notice. In the apartment next door, a family has recently moved in, waiting for their decision on asylum. How likely is it that you will show the following behavior? 'I'll ask the family if they can take care of my kid.'

The second situation that involved *benevolence* (*but low trustworthiness*) was:

You've been shopping and are now driving home from the supermarket with a full bike basket and two more bags. Directly in front of an asylum seekers' home is a sharp bend, where the bike basket falls from the luggage carrier and the shopping is spread out on the street. Two asylum seekers, who were just standing in front of the home, run to you and start picking up the groceries from the ground. How likely is it that you will behave like this? 'I'll let them know clearly that I can do this on my own.'

The situations were followed by the attitudinal measure of outgroup trust and a measure of cognitive flexibility that was included for exploratory purposes.

#### Measures

*Initial outgroup trust* towards asylum seekers was assessed with one item (1 = *not at all trustworthy* to 7 = *very trustworthy*). The same two items as in Study 2 served as *manipulation check*,  $r(197) = 0.70$ ,  $p < 0.001$ .

#### *Behavioral outgroup trust*

Participants rated the likelihood of performing specific (dis)trusting behaviors towards asylum seekers in six hypothetical situations. Answers were given on a 7-point scale (1 = *very unlikely* to 7 = *very likely*). Due to low internal consistency of all six situations ( $\alpha = 0.46$ ), we decided to remove one item (i.e., the one measuring *ability* with a *negative* response) and continue with a compound measure of the remaining five situations which had considerably higher and acceptable internal consistency ( $\alpha = 0.60$ ).

#### *Attitudinal outgroup trust*

We used the same measure of attitudinal outgroup trust as in the preceding studies ( $\alpha = 0.87$ ). The attitudinal and the behavioral measure of outgroup trust were correlated,  $r(197) = 0.61$ ,  $p < 0.001$ . This validates the hypothetical situations we created in the sense that they tap into the same construct as the established self-report measure of trust.

## Results

### Checks

Initial outgroup trust did not differ between the two experimental conditions,  $t(195) = 0.16$ ,  $p = 0.871$ ,  $d = 0.02$ , 95% CI [-0.26, 0.30]. Thus, there was no correlation between the predictors in the regression analyses reported below. In addition, we tested the influence of ingroup diversity, initial outgroup trust, and their interaction on the perceived diversity of the ingroup with a multiple regression analysis. Following recommendations by Aiken and West (1991), the continuous predictor initial outgroup trust was mean-centered. Experimental condition was effect-coded (-1 high diversity, +1 low diversity). We applied this procedure in any of the subsequently reported multiple regression analyses.

We found a main effect of initial outgroup trust on perceived ingroup diversity,  $B = -0.18$ ,  $SE = 0.08$ , 95% CI [-0.35, -0.02],  $t(193) = -2.24$ ,  $p = 0.026$ . The more participants initially trusted asylum seekers the less they perceived their ingroup as diverse. Most relevant to the success of our manipulation, we also found a significant main effect of experimental condition,  $B = 0.39$ ,  $SE = 0.10$ , 95% CI [0.20, 0.58],  $t(193) = 4.05$ ,  $p < 0.001$ . However, counter to our expectations, participants in the high diversity condition ( $M = 4.47$ ,  $SD = 1.52$ ) perceived their ingroup as *less* diverse than participants in the low diversity condition ( $M = 5.24$ ,  $SD = 1.23$ ). These two main effects were qualified by a significant interaction of initial outgroup trust and experimental condition,  $B = 0.22$ ,  $SE = 0.08$ , 95% CI [0.06, 0.38],  $t(193) = 2.67$ ,  $p = 0.008$ . The higher initial outgroup trust was, the more did the high (vs. low) diversity manipulation reduce perceived ingroup diversity. These results call into question whether the manipulation of ingroup diversity worked as intended.

### *Behavioral outgroup trust*

We tested our prediction that high (vs. low) ingroup diversity would reduce (enhance) outgroup trust more the higher (lower) initial outgroup trust is. In a multiple regression analysis, we found a significant main effect of

initial outgroup trust on outgroup trust,  $B = 0.47$ ,  $SE = 0.06$ , 95% CI [0.37, 0.58],  $t(193) = 8.62$ ,  $p < 0.001$ . Unsurprisingly, higher initial outgroup trust predicted higher outgroup trust after the diversity manipulation. There was no main effect of experimental condition,  $B = -0.03$ ,  $SE = 0.06$ , 95% CI [-0.16, 0.10],  $t(193) = -0.46$ ,  $p = 0.650$ . Crucially with regard to our hypothesis, the interaction of initial outgroup trust and experimental condition was not significant,  $B = 0.06$ ,  $SE = 0.06$ , 95% CI [-0.05, 0.16],  $t(193) = 1.02$ ,  $p = 0.309$  (see **Figure 1**).

#### Attitudinal outgroup trust

Based on the same predictions as for behavioral outgroup trust, we ran another multiple regression analysis. Here, we also found a significant effect of initial outgroup trust on attitudinal outgroup trust,  $B = 0.59$ ,  $SE = 0.06$ , 95% CI [0.48, 0.71],  $t(193) = 10.00$ ,  $p < 0.001$ . Again, higher ratings of initial outgroup trust were associated with higher ratings of outgroup trust after the diversity manipulation. There was no significant main effect of experimental condition,  $B = 0.13$ ,  $SE = 0.07$ , 95% CI [-0.01, 0.27],  $t(193) = 1.87$ ,  $p = 0.062$ . Participants in the low diversity condition ( $M = 5.45$ ,  $SD = 1.11$ ) tended to report higher outgroup trust than participants in the high diversity condition ( $M = 5.15$ ,  $SD = 1.14$ ). Importantly, against our hypothesis, there was no interaction effect of initial outgroup trust

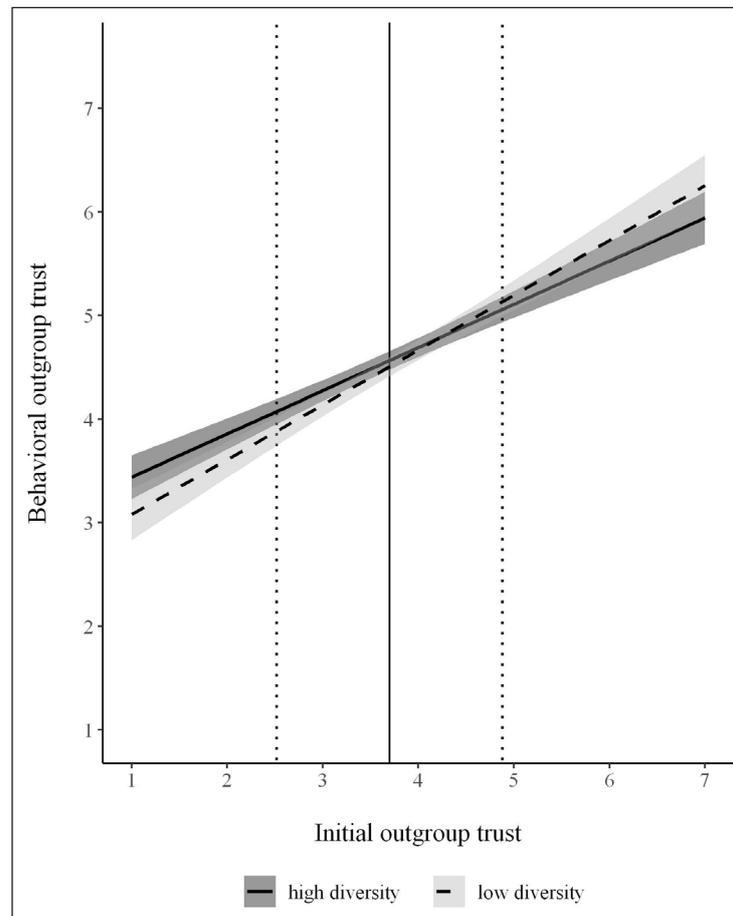
and experimental condition,  $B = 0.04$ ,  $SE = 0.06$ , 95% CI [-0.08, 0.16],  $t(193) = 0.71$ ,  $p = 0.482$ .

#### Discussion

Study 4 did not provide evidence in support of our hypothesis. Although descriptively the relationship between initial outgroup trust and behavioral outgroup trust was weaker in the high (vs. low) ingroup diversity condition, the interaction was not significant. As in Study 2, the manipulation check did not validate the manipulation and calls the effectiveness of the manipulation into question. In addition, an exploratory multiple regression analysis revealed that the manipulation did not affect cognitive flexibility (for details see Supplement). Despite the theoretical relevance of cognitive flexibility for our reasoning, we only included it as an exploratory variable here, because we aimed at establishing a base effect first. Given the limited evidence for such an effect in the first place, it is hard to draw any conclusions about potential underlying processes.

#### Meta-Analysis across Studies

Across all studies, the pattern of results for our main analyses pointed into the hypothesized direction but did not reach statistical significance. To provide a clearer picture of our data, we conducted an internal meta-analysis



**Figure 1:** Behavioral outgroup trust as a function of manipulated ingroup diversity (high vs. low) and measured initial outgroup trust. Shaded areas represent the  $\pm 1$  standard error margin. The solid vertical line represents the sample mean of initial outgroup trust; the dotted vertical lines mark one standard deviation below and above the mean, respectively (Study 4;  $N = 197$ ).

to test our assumption that high (vs. low) ingroup diversity would mitigate the default tendencies in behavioral trust. Importantly, although previous research argues that people usually trust their ingroup more than an outgroup (Foddy et al., 2009), we found the reverse default pattern in Study 3. Therefore, we reversed the coding of trustee group (i.e., -1 ingroup, +1 outgroup) in the meta-analysis for this particular study. We converted effect sizes of every single study into correlation coefficients  $r$  and used the R-package 'metafor' (Viechtbauer, 2010) to calculate a mean effect size  $r$  (random-effects model) weighted for sample size. The interaction effect (i.e., ingroup diversity  $\times$  default trust) as well as planned contrasts for low and high ingroup diversity were analyzed.

The meta-analysis revealed a small, but significant interaction of ingroup diversity (low vs. high) and default tendencies to trust (low vs. high) on behavioral trust,  $r = 0.07$ ,  $SE = 0.03$ ,  $p = 0.028$ , 95% CI [0.01, 0.14] (see **Figure 2a**). This is supported by the two planned contrasts. In the low diversity condition, there was a medium effect of default trust,  $r = 0.26$ ,  $SE = 0.07$ ,  $p < 0.001$ , 95% CI [0.13, 0.39] (see **Figure 2b**). Thus, behavioral trust was affected by participants' default level of trust towards the respective group. This effect was still present, but considerably smaller in the high diversity condition,  $r = 0.17$ ,  $SE = 0.08$ ,  $p = 0.030$ , 95% CI [0.02, 0.32] (see **Figure 2c**). To test whether the aggregated effect sizes obtained from the planned contrast analyses differed from each other, we compared the correlation coefficients from the planned contrasts using Fisher  $r$ -to- $z$  transformation. This analysis revealed that the aggregated effect sizes differed at  $p = 0.048$ . Thus, our manipulation of high diversity weakened the relationship between default trust towards the respective group and behavioral trust.

## General Discussion

In the current work, we investigated whether representing one's ingroup as high (vs. low) in diversity mitigates default tendencies in group-based trust. We predicted that under conditions of low ingroup diversity, default tendencies to trust others based on their social group membership would be present, whereas they should be less pronounced under conditions of high ingroup diversity. In four studies, applying a variety of well-established manipulations and measures, support for our hypothesis was limited. None of the four single studies revealed a significant interaction effect between ingroup diversity and default trust. However, inspecting these findings with a meta-analytic approach contributed to clarify the overall picture. Here, the hypothesized interaction was statistically significant, but its tiny effect size ( $r = 0.07$ ) indicates that it is not meaningful in size. This also reflected in a significant difference of the simple comparisons meaning that the manipulation of high diversity mitigated default tendencies to trust, but to a very small extent.

The conclusion of these findings is twofold. On the one hand, the small interaction effect found in the internal meta-analysis is in line with the theoretical reasoning outlined in the introduction. Thus, the results in

principle support the idea that an increase in cognitive flexibility (here due to high ingroup diversity) would lead to a reconsideration of previously held beliefs, that is, mitigate defaults in group-based trust. This ties in with a growing body of research that shows debiasing effects of increased cognitive flexibility in intergroup relations (e.g., Sassenberg & Moskowitz, 2005; Vasiljevic & Crisp, 2013; Winter et al., 2021). On the other hand, the observed effect is too small in size to be considered meaningful for practical purposes. Therefore, the current set of studies provides insights relevant for both theoretical advancement and practical application – although the latter might not be very promising.

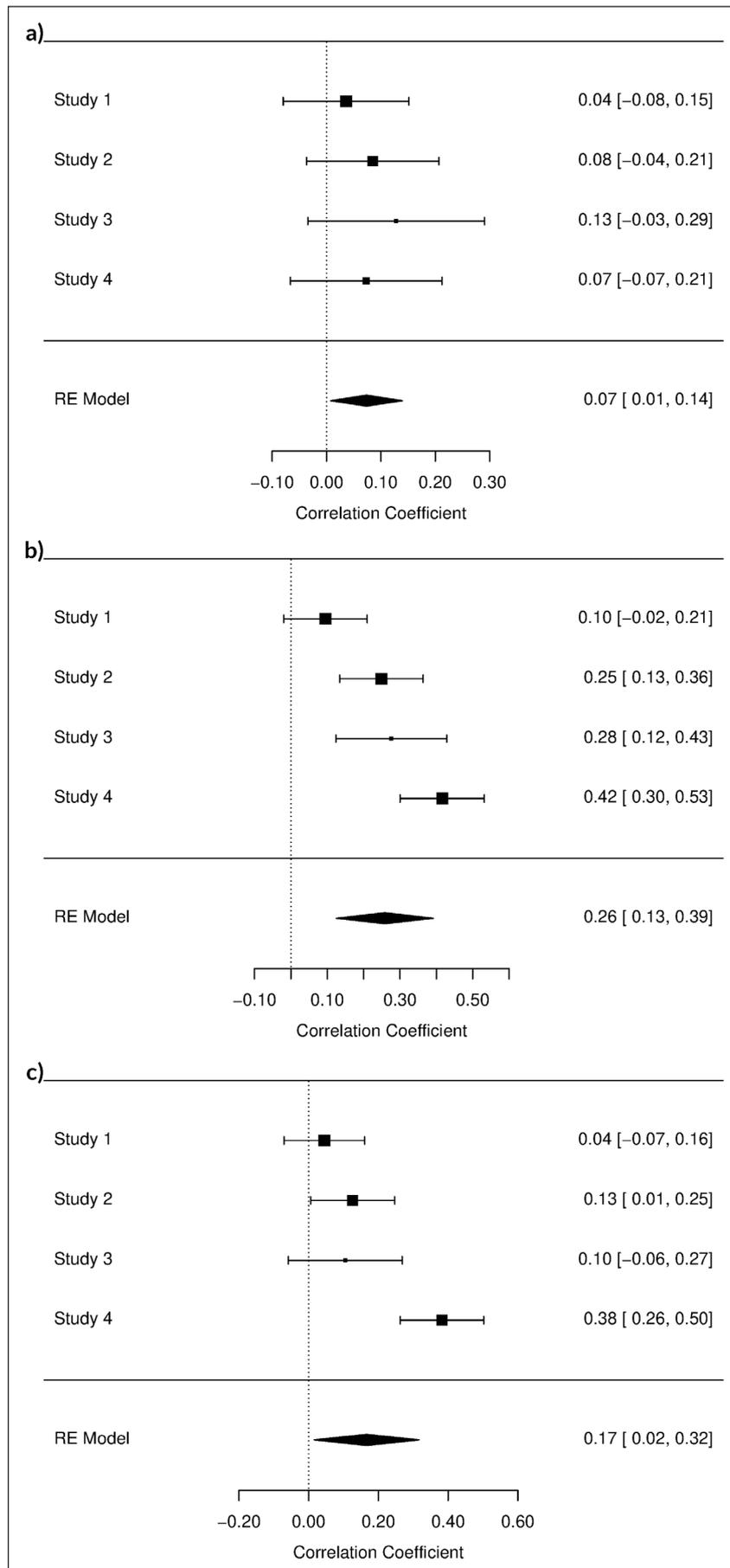
## Methodological considerations

It is worth noting that meta-analyses within papers have been criticized (Vosgerau et al., 2019). However, the main basis for the criticism is selective reporting within papers. Given that we report all studies that we have conducted on this research question, this criticism is less relevant in the current case. To draw more definite conclusions, one would need to conduct a study that is appropriately powered to detect an effect of the size the meta-analysis suggests. However, given that the small aggregated effect this would require a giant sample size (around  $N = 1600$ ). We did not conduct such a final study, because it would have required to throw tons of good money after bad (i.e., tiny) effects.

The small overall effect might also result from a failure to successfully manipulate ingroup diversity as indicated by the lack of an effect on the manipulation checks. On the contrary, the original research using these manipulations consistently reports effects on the same or similar manipulation check items (Hutchison et al., 2006; Waldzus et al., 2003). The fact that we hardly found any effects on the manipulation checks while reporting all the studies we conducted questions the validity of the used manipulations. Furthermore, these (null-)findings also demonstrate the need for a critical evaluation of research that was conducted prior to the replication crisis in psychology and that does not meet current methodological and reporting standards (e.g., small sample sizes, selective reporting).

The usefulness of subjective manipulation checks has long been questioned. Some researchers argued that participants are often unable to verbally report a mental state that has been induced via a manipulation (Nisbett & Wilson, 1977). Thus, it remains unclear from a 'failed' manipulation check whether the manipulation indeed failed or whether participants could just not adequately describe their internal state. In any case, the fact that the manipulations' effects on trust were still in the expected direction brings up the question whether consciously perceiving increased ingroup diversity is a necessary step in the assumed process. It might be that high diversity affected the underlying way of thinking – for instance, in terms of enhanced cognitive flexibility – without reaching participants' conscious awareness.

Furthermore, Fayant and colleagues (2017; relying on Sigall & Mills, 1998) argue that results in line with a



**Figure 2:** Correlation coefficients  $r$  and 95% CIs from Meta-Analyses across Studies 1 to 4. Results refer to **(a)** the interaction of ingroup diversity and default trust, **(b)** simple comparisons in the *low* diversity condition, and **(c)** simple comparisons in the *high* diversity condition.

prediction are not invalidated by a lack of the expected effect on a manipulation check due to numerous alternative interpretations (e.g., low construct validity of the manipulation check). Following this reasoning, the absence of effects on our manipulation checks hardly provides evidence for or against the success of our manipulations. In any case, one can conclude from the current data that perceived ingroup diversity is difficult to manipulate – even if one relies on methods that are well-established in the literature. This raises the question how valid these manipulations are in the first place and calls for a closer examination in the future.

Another issue raised by our studies is the use of different measures of trust. On the one hand, the behavioral measures we used were consistently correlated with the attitudinal measures of trust. This indicates that these different forms of measurement, indeed, captured the same construct. On the other hand, it remains an open question, why (if at all) the behavioral measures were affected more strongly by our manipulations of ingroup diversity than the attitudinal ones. One potential explanation for this could again be that the manipulations have an impact that does not require conscious awareness of increased diversity. Thus, a very explicit measurement of trust – as is the case for attitudinal measures – could be less likely influenced than a behavioral measure that might be affected by more implicit factors. This dovetails with our explanation for the failed manipulation checks.

#### **Limitations and future research**

One reason for not finding any substantial effects might lie in the preconditions specified in the CPAG model (Crisp & Turner, 2011). It is possible that the manipulations we used did not contain enough inconsistencies with previously held beliefs or that participants were not sufficiently motivated or able to resolve them. If either of these is the case, the diversifying experience we induced might not have enhanced cognitive flexibility (as the exploratory results from Study 4 suggest), which could explain why we did not find any meaningful changes in default trust. Other types of diversity or related concepts might be more useful for the same purpose. For instance, generating counter-stereotypic combinations of social categories, which directly asks for subjectively perceived inconsistencies, leads to both higher cognitive flexibility and a mitigation of default outgroup trust (Vasiljevic & Crisp, 2013; Winter et al., 2021).

Similar results were obtained by letting participants describe their social identity in a more complex way (Xin et al., 2016). The effects of social identity complexity are usually explained with an increase in perceived proximity or overlap between ingroup and outgroup (Roccas & Brewer, 2002). However, it would be interesting to follow up on recent studies and see whether cognitive flexibility plays a role in these processes as well (Steffens et al., 2016) and which boundary conditions one needs to consider (Damer et al., 2018). Overall, providing a clearer picture with regard to various ways of operationalizing diversity could, contribute to explaining why some researchers

report negative effects and others highlight the benefits of diversity for intergroup trust (Hewstone, 2015). The current research undertook a first step towards disentangling different operationalizations of diversity. In doing so, we came to the conclusion that high ingroup diversity does not contribute much to the development of intergroup relations.

#### **Conclusion**

Across four studies, we found weak (meta-analytical) evidence for the hypothesis that high ingroup diversity leads to a reduction of default tendencies in group-based trust. However, this must be judged as barely relevant given the small aggregated effect size and calls into question the validity of some well-established manipulations. From a theoretical perspective, the current work contributes to the research on diversity, cognitive flexibility, and intergroup relations. From a practical view, our results indicate that enhancing (the perception of) ingroup diversity is surely not the most effective means when it comes to mitigating defaults in group-based trust.

#### **Data Accessibility Statement**

The data (<http://dx.doi.org/10.23668/psycharchives.4969>) and analysis code (<http://dx.doi.org/10.23668/psycharchives.4968>) of all studies are openly accessible.

#### **Notes**

- <sup>1</sup> Based on this theoretical background, in some of the preregistrations we labelled the construct under investigation ‘ingroup complexity’, which we deem interchangeable with the term ‘ingroup diversity’ used in the manuscript according to the CPAG model (Crisp & Turner, 2011).
- <sup>2</sup> Note that the distinction between ingroup and outgroup was not based on the demographic information collected in the study. Rather the instructions literally referred to students of ‘another’ or ‘participants’ ‘own’ subject. This had the advantage that participants could choose for themselves which subject they feel part of (or not).
- <sup>3</sup> For  $\eta^2$  we report 90% CIs, because 95% CIs in this case may contain zero, although the difference is significant. This is because  $\eta^2$  can only take on positive values. Thus, 90% CIs around  $\eta^2$  equals 95% CIs around  $d$  for the exactly same statistical test (for more detail, see Lakens, 2014; Steiger, 2004).
- <sup>4</sup> In an additional analysis, we accounted for the bimodal distribution of the raw scores using transformed values. This analysis led to the same results.
- <sup>5</sup> As in Study 2, raw scores of the trust game followed a bimodal distribution. Analyses with transformed scores yielded the same results.

#### **Additional File**

The additional file for this article can be found as follows:

- **Supplement.** Instructions, measures, and exploratory analyses. DOI: <https://doi.org/10.5334/irsp.520.s1>

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## Competing Interests

The authors have no competing interests to declare.

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