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## Intragroup communication in intergroup conflict

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# **CHAPTER FOUR**

Improving Intergroup Perceptions: Abstract and Concrete Intragroup  
Communication About Stereotypes.



## Chapter Four

### Improving Intergroup Perceptions: Abstract and Concrete Intragroup Communication About Stereotypes.

Many situations of pervasive intergroup conflict are characterized by lack of substantial positive intergroup contact. Group members tend to self-segregate even in everyday situations in which outgroup members are present (Dixon & Durrheim, 2003; Dixon, Tredoux, Durrheim, Finchilescu, & Clack, 2008). One reason may be that anticipating problematic contact can motivate avoidance of intergroup interactions (Binder et al., 2009; Plant & Devine, 2003). Consequentially, interactions with members of conflicting outgroups tend to be ephemeral and superficial (Moody, 2001; Sigelman, Bledsoe, Welch, & Combs, 1996). Any intervention aiming to de-escalate intergroup conflict by intervening in intergroup communications would involve the struggle of organizing intergroup encounters. Staging intergroup encounters could be perilous because negative intergroup contact may more strongly influence intergroup perceptions than positive contact (Barlow et al., 2012; but see Stark, Flache, & Veenstra, 2013). Moreover, most part of our social life occurs with ingroup members (McPherson, Smith-Lovin, & Cook, 2001) and these interactions are more significant than intergroup conversations for crafting shared perceptions (e.g., Echterhoff, 2014; Postmes et al., 2014).

Therefore, the current research investigated the potential effectiveness of targeting *intragroup* communication in intergroup conflict interventions. We are interested in two aspects of such communication: The content (i.e., stereotype-confirming or -disconfirming) and the abstraction level. Previous research suggests that concrete information about negatively stereotyped outgroups is more likely to influence individuals' cognitions because its vividness makes it seem more real (e.g., Hansen & Wänke, 2010). However, other research suggests that because negative stereotypes are abstract cognitions, they can only be effectively disconfirmed at an abstract level (e.g., Paik, MacDougall, Fabrigar, Peach, & Jellous, 2009). Although stereotype change research seems to rely by default on providing stereotype-inconsistent exemplars (Paik et al., 2009), one can deduce several competing hypotheses from the literature regarding what sort of intragroup communication most strongly affects intergroup perceptions. The aim of the present research was to

test these alternative hypotheses and thereby shed light on the most effective intragroup intervention to improve intergroup perceptions.

## The Impact of Intragroup Communication on Cognition

Several strands of literature suggest that individuals' perceptions and cognitions emerge through communication and interaction (e.g. Echterhoff, Higgins, & Levine, 2009; Hardin & Higgins, 1996; Semin & Smith, 2013; Turner, Hogg, Oakes, Reicher, & Wetherell, 1987). Rather than concentrating on “vertical” information transmission between the more and the less knowledgeable (e.g., Vygotsky, 1978), these theorists state that people also rely on “horizontal” peer communication, especially with (ingroup) others similar to the self (e.g., Echterhoff, Kopietz, & Higgins, 2013; Haslam, McGarty, & Turner, 1996; Postmes, 2003).

Such horizontal, intragroup communication may be especially influencing perceptions of intergroup relations, because these social perceptions of “us” and “them” merely exist by virtue of their social sharedness. Indeed, intergroup perceptions and resultant behavior norms typically develop and transform when “we” communicate about “them” (e.g., Haslam, Turner, Oakes, McGarty, & Reynolds, 1997; Kashima, 2014; Lyons, Clark, Kashima, & Kurz, 2008; Postmes et al., 2014; Smith & Postmes, 2011). Thus, the content of communication within social groups seems to shape the intergroup perceptions people act upon. For instance, if small groups discuss their personal experiences with hostile intergroup encounters this leads to anxiety and fosters defensiveness (Chapter 2). Relatedly, we assume that discussing why a negative outgroup stereotype is true (false) yields more negative (positive) intergroup cognitions and attitudes.

However, less is known about possible influences of the *form* that this intragroup communication takes. Previous research suggested that group members are more open to intergroup cooperation provided that intragroup interactions are harmonious (Grijndanus, Postmes, Gordijn, & Van Zomeren, 2015). This implies that a successful intervention to improve intergroup perceptions should facilitate harmonious intragroup communication that aims at collective refutation of negative outgroup stereotypes. As a next step, the current research focused more in-depth on effects of the form of intragroup communication. Specifically, would an abstract or a concrete level of intragroup communication be more powerful in changing individuals' perceptions?

## The Power of Concrete versus Abstract Communication

An effective intragroup communication-based intervention to improve intergroup perceptions should comprise an optimal combination of stereotype disconfirmation and abstraction level. But the literature is inconclusive regarding what the optimal strategy would be. We deduced four competing hypotheses: The vividness hypothesis, generalization hypothesis, abstract-disconfirmation hypothesis, and the interaction hypothesis.

### Competing hypothesis 1: Vividness hypothesis.

Based on previous findings, it could be argued that an intervention aimed at de-escalating intergroup conflict through intragroup communication should make use of concrete communication. For instance, concrete, detailed information is more vivid than abstract, general information and therefore people perceive it as more real (Hansen & Wänke, 2010). Thus, “the traditional strategy in the stereotyping literature” to change stereotypic beliefs has been to present people with information about individual outgroups members who disconfirm the stereotype (Paik et al., 2009, p. 113). For instance, one might hear that “Mohammed Raji Elilah is a hard-working, competent, and warm driving instructor.” Relatedly, interventions that provide concrete details about positive intergroup contact have been shown to reduce prejudice. This happens when people observe a fellow ingroup member engaging in a close friendship with an outgroup member (i.e., vicarious or extended intergroup contact; Wright, Aron, McLaughlin-Volpe, & Ropp, 1997) or when they imagine having a pleasant intergroup interaction themselves (i.e., imagined intergroup contact; Crisp & Turner, 2009). However, a recent meta-analysis on imagined contact showed mixed evidence for the role of concreteness (Miles & Crisp, 2014). Studies that instruct people to mentally simulate details of the context in which they interact with a (fictitious) outgroup member more strongly affect intergroup perceptions than studies that lack such instructions. However, other factors related to concreteness (e.g., the amount of detail about the outgroup target, the time group members spend imagining contact) did not reveal significant effects. Thus, there is inconclusive empirical support for this *vividness hypothesis*, that concrete, vivid intragroup communication that confirms negative outgroup stereotypes should

worsen intergroup perceptions, whereas concrete communication that disconfirms stereotypes should improve perceptions.

### Competing hypothesis 2: Generalization hypothesis.

Interestingly (and perhaps not unrelated to the mixed evidence), there is also a literature that one could deduce the exact opposite prediction from: That abstract communication is more effective at changing intergroup perceptions. Linguistic abstraction facilitates generalized interpretation and inference of the conveyed information as context-independent (Assilaméhou, Lepastourel, & Testé, 2013; Semin & De Poot, 1997; Wigboldus, Semin, & Spears, 2000). Abstract, generalizing words (e.g., *always*, *everyone*) signal that a communicated piece of information cannot be dismissed as minor (Pomerantz, 1986). Of importance for the current research, communicators also strategically use abstraction to encourage generalization of information that deviates from prior knowledge, such as stereotypes (Douglas & Sutton, 2003; Fiedler, Bluemke, Friese, & Hofmann, 2003; Wenneker, Wigboldus, & Spears, 2005). Indeed, to modify abstract cognitions such as stereotypes one needs abstract disconfirmation (Paik et al., 2009). For example one might learn concrete evidence that “As a group, Moroccans tend to be competent.”

The fact that one can contest and debate the appropriateness of negative stereotypes by discussing them abstractly implies that abstraction per se does not automatically lead to the application of a negative stereotype (cf. Greijdanus, Postmes, Gordijn, & Van Zomeren, 2014; Chapter 3 of this dissertation), even though abstraction and negativity about the outgroup may often coincide (e.g., Fiske & Neuberg, 1990; Maass, Milesi, Zabbini, & Stahlberg, 1995). Research on abstraction level of cognition provides additional support for the supposed beneficial influences of abstraction on intergroup perceptions. That is, insofar abstract communication maps onto abstract cognition it may harmonize intergroup relations due to its focus on (intergroup) similarities (Förster, Liberman, & Kuschel, 2008). However, abstract cognition may also foster prejudice because it tends to increase perceived social (intergroup) distance (Liberman & Trope, 2008). Taken together, there is mixed evidence for this *generalization hypothesis*, that abstract, generalizing intragroup communication disproving negative outgroup stereotypes should improve intergroup perceptions, whereas abstract communication that confirms stereotypes should worsen perceptions.

### **Competing hypothesis 3: Abstract-disconfirmation hypothesis.**

To make things even more complex, the literature suggests two alternative hypotheses in addition to the vividness hypothesis and the generalization hypothesis, predicting interactions between abstraction level (i.e., communication form) and the stereotype-(dis)confirming nature of intragroup discussions (i.e., communication content). First, according to the *abstract-disconfirmation hypothesis*, abstraction level is likely to influence whether stereotype disconfirmation will have an effect on reducing stereotyping (Paik et al., 2009). That is, stereotype-disconfirming information may be discounted as an uninformative exception unless it is communicated abstractly (cf. Fiedler et al., 2003). Contrarily, stereotype-confirming information (whether communicated concretely or abstractly) should not influence individuals' perceptions because it does not add anything new to already existing knowledge, i.e., it confirms what we already know.

### **Competing hypothesis 4: Interaction hypothesis.**

Finally, according to the full *interaction hypothesis* one can similarly predict that abstract stereotype disconfirmation is more effective than concrete disconfirmation. However, stereotype-confirming information may also have an effect when it enlivens negative outgroup stereotypes with vivid, concrete communication (cf. Hansen & Wänke, 2010). That is, concrete stereotype confirmation is more influential than abstract confirmation.

In sum, in the current research we compared four competing hypotheses regarding influences of intragroup communication on intergroup perceptions: The *vividness hypothesis* states that concrete, vivid intragroup communication should be most influential in changing intergroup perceptions. Conversely, the *generalization hypothesis* predicts the most influence of abstract, generalizing communication. The *abstract-disconfirmation hypothesis* holds that for stereotype-confirming information there should be no difference between concrete and abstract communication, whereas stereotype-disconfirming information should be communicated in an abstract, generalizing manner to realize the largest effect. Finally, the *interaction hypothesis* implies that concrete stereotype-confirmation and abstract stereotype disconfirmation are most influential in individuals' intergroup perceptions. A comparative test of these four competing hypotheses could contribute to



development of an intragroup communication-based intervention in intergroup conflict.

## Overview of Present Research

The present research tests the applicability of an intervention targeting intragroup communication (i.e., circumventing practical issues of self-segregation in intergroup conflict; Dixon et al., 2008) to improve intergroup perceptions regarding a (mildly) conflicting outgroup. Three experiments examined the effects of intragroup communication about stereotypes on individuals' cognitions about the outgroup. Experiment 4.1 focuses on stereotypes about elderly people, Experiment 4.2 on Moroccan adolescents and Experiment 4.3 on non-native Dutch adolescents. We were interested in the content (i.e., stereotype-confirming, stereotype-disconfirming) and form (i.e., concrete level, abstract level) that intragroup communication should have. There are multiple ways in which communication could tend more towards either end of the concrete-abstract continuum. As we were interested in developing an intervention, we manipulated the abstraction level of communication rather than measuring it (e.g., Semin & Fiedler, 1988). Based on the premise that cognition and communication at a concrete level involve rich details of specific instances whereas an abstract level provides generalizations that transcend specific contexts, we decided to operationalize the level of intragroup communication in the following ways. Concrete communication focused on describing actual examples of behaviors of individual outgroup members that (dis)confirmed stereotypes. Abstract communication entailed collective stereotype-(dis)confirmation based on general group characteristics of the entire outgroup as a social category. Because intragroup harmony may be a precondition for intergroup harmony (Greijdanus et al., 2015), all intragroup communication in the current research (i.e., abstract, concrete, stereotype-confirming, and stereotype-disconfirming) was aimed at building intragroup consensus.

In Experiments 4.1 and .42, we tested the above-mentioned four competing hypotheses in natural intragroup discussions (Experiment 4.1) and with a confederate (Experiment 4.2) in three different intergroup contexts. Additionally, Experiment 4.3 tested the influences of concrete and abstract stereotype disconfirmation in natural intragroup discussions (i.e., potential, feasible

interventions to improve intergroup perceptions) against a neutral baseline condition.

## Experiment 4.1

The aim of Experiment 4.1 was to test four competing hypotheses regarding the influences of stereotype-(dis)confirming intragroup communication on intergroup perceptions (i.e., vividness, generalization, abstract-disconfirmation, and interaction hypotheses). As a first test of these hypotheses we focused on natural dialogue among adolescents about their outgroup elderly people, because the only slightly negative perceptions of this target group should be relatively malleable. The interventions consisted of instructions manipulating stereotype-(dis)confirmation and abstraction level of intragroup communication without further manipulations once discussions had started.

### Method

**Participants and design.** Eighty-two students from the University of Groningen (54 women;  $M_{\text{age}} = 21.95$ ,  $SD = 5.17$ ) were randomly assigned to four conditions of a two-by-two between-participants design crossing communication level (concrete, abstract) with communication aim (stereotype-confirmation, stereotype-disconfirmation).

**Materials.** All materials were provided on paper. *Stereotype-(dis)confirmation* and *communication abstraction* were manipulated via group instructions to spend five minutes discussing why the negative stereotype that elderly people (i.e., the target outgroup) are forgetful, boring, and old-fashioned is true [false], as proven by behaviors of individual elderly people (concrete communication) or by general characteristics of the elderly (abstract communication). To stimulate task engagement, the bottom of the instructions page provided room to list the top three most illustrative behaviors or characteristics as part of the manipulation.

*Stereotype application* was measured with 10 statements (“Elderly people are [trait]”, 1 *Absolutely not* - 7 *Absolutely*) referring to negative or positive traits. *Intergroup*

*attitudes* were measured as cognitive, affective, and social proximity components (cf. Rojas, Lozano, Navas, & Pérez, 2011). Cognitive judgments were measured with six items (e.g., “What do you think of the values of elderly people (how they educate their children, equality between men and women, role of religion in their lives, etcetera)?”, 1 *Very bad* - 7 *Very good*), affective reactions with eight items (e.g., “How strongly do you in general feel admiration for elderly people?”, 1 *Not at all* - 7 *Very strongly*), and attitudes regarding social proximity with three statements (e.g., “If I could choose, I would have elderly friends”, 1 *Strongly disagree* - 7 *Strongly agree*).<sup>26</sup>

The questionnaire ended with manipulation checks, a measure of conversation ease (“How did the conversation go?” 1 *Very difficult* - 7 *Very easy*), demographics (age, gender, nationality), and a debriefing.<sup>27</sup>

**Procedure.** After providing informed consent, participants received the group discussion instructions and engaged in a small-group interaction with two or three fellow students. In the next phase, the dependent measures and demographic questions were administered individually. Finally, participants were debriefed.

**Analytic strategy.** Because participants were nested within discussion groups, data of all experiments were analyzed with multilevel regressions in HLM (Raudenbush, Bryk, & Congdon, 2004). We used planned contrasts to test four competing hypotheses. All hypotheses translate to two simple effects comparing concrete with abstract confirmation and concrete with abstract disconfirmation, respectively. According to the *vividness hypothesis*, 1) concrete confirmation of (negative) stereotypes should yield more negative intergroup perceptions than abstract confirmation, and 2) concrete disconfirmation should yield more positive intergroup perceptions than abstract disconfirmation. Conversely, the *generalization hypothesis* states that 1) abstract stereotype confirmation should yield more negative intergroup perceptions than concrete confirmation, and 2) abstract stereotype

<sup>26</sup> We added *aversion* to the original affective subscale. Items that seemed inappropriate for the outgroup elderly people (e.g., referring to their work, intergroup romantic relations) were not included in intergroup attitude measures.

<sup>27</sup> We also measured participants’ emotional reactions and behavioral intentions. Because these variables are not of primary interest to the current focus on intergroup perceptions, we do not report these here. Overall there was strongest support for the abstract disconfirmation hypothesis. Further information is available upon request.

disconfirmation should yield more positive intergroup perceptions than concrete disconfirmation. The *abstract disconfirmation hypothesis* predicts 1) no difference between concrete and abstract stereotype confirmation, and 2) that concrete disconfirmation should yield more positive intergroup perceptions than abstract disconfirmation. Finally, based on the *interaction hypothesis* one would expect that 1) concrete stereotype confirmation should be more negative than abstract confirmation, whereas 2) abstract disconfirmation should be more positive than concrete disconfirmation. The umbrella term *intergroup perceptions* in these hypotheses denotes all dependent variables (e.g., stereotype application, intergroup attitudes). To test these hypotheses, concrete confirmation, abstract confirmation, concrete disconfirmation, and abstract disconfirmation were coded respectively as -1 1 0 0 (planned contrast 1) and 0 0 -1 1 (planned contrast 2). The estimated HLM models were:

$$\text{Level-1: } Y = \beta_0 + r$$

$$\text{Level-2: } \beta_0 = \gamma_{00} + \gamma_{01} * (\text{contrast1}) + \gamma_{02} * (\text{contrast2}) + u_0$$

$Y$  represents the dependent variable (higher means more positive intergroup perceptions),  $\beta$  is the individual-level regression coefficient,  $\gamma$ s are group-level regression coefficients, and  $r$  and  $u$  respectively are the individual-level and group-level errors. The hypothesized patterns were:

Vividness:  $Y_{\text{concrete confirmation}} < Y_{\text{abstract confirmation}}, Y_{\text{concrete disconfirmation}} > Y_{\text{abstract disconfirmation}}$

Generalization:  $Y_{\text{concrete confirmation}} > Y_{\text{abstract confirmation}}, Y_{\text{concrete disconfirmation}} < Y_{\text{abstract disconfirmation}}$

Abstract disconfirmation:  $Y_{\text{concrete confirmation}} = Y_{\text{abstract confirmation}}, Y_{\text{concrete disconfirmation}} < Y_{\text{abstract disconfirmation}}$

Interaction:  $Y_{\text{concrete confirmation}} < Y_{\text{abstract confirmation}}, Y_{\text{concrete disconfirmation}} < Y_{\text{abstract disconfirmation}}$

## Results

Because the manipulations were successful (i.e., more correct than incorrect manipulation checks in both levels of both manipulations,  $ps < .001$ ), data of 11

participants who failed at least one manipulation check were removed. Although this variable was not of primary interest for the current research focus, data of one participant with extremely high intergroup anxiety (univariate outlier with  $p < .001$ ) were deleted to prevent distorted results. Mahalanobis distance revealed no multivariate outliers with  $p < .001$ , leaving 70 cases for analysis (49 women;  $M_{\text{age}} = 22.00$ ,  $SD = 5.49$ ). Planned contrasts indicated that participants in the concrete confirmation condition had somewhat less easy group conversations than participants in the abstract confirmation condition,  $t(21) = -1.82$ ,  $p = .08$ , while abstraction level did not influence conversation ease of collective stereotype disconfirmation,  $t(21) = 1.11$ ,  $p = .28$ . To control for possible influences of conversation ease, this variable was entered group-mean centered at the individual level and grand-mean centered at the group level (both Level-1 and Level-2 main effects and Level-1 by Level-2 interaction). The estimated HLM models were:

$$\text{Level-1: } Y = \beta_0 + \beta_1 * (\text{conversation ease}^a) + r$$

$$\text{Level-2: } \beta_0 = \gamma_{00} + \gamma_{01} * (\text{contrast1}) + \gamma_{02} * (\text{contrast2}) + \gamma_{03} * (\text{conversation ease}^b) + u_0$$

$$\beta_1 = \gamma_{10} + \gamma_{11} * (\text{conversation ease}^b) + u_1$$

All symbols are defined as previously described, <sup>a</sup> means group-mean centered, and <sup>b</sup> means grand-mean centered.

Multilevel analyses on stereotype application provided converging support for the abstract disconfirmation hypothesis (see Table 4.1). Abstract stereotype disconfirmation led to application of more positive,  $t(20) = 3.30$ ,  $p < .01$ , and marginally less negative traits,  $t(20) = -1.87$ ,  $p = .08$ , than concrete disconfirmation. In line with this hypothesis, there were no such effects for stereotype confirmation,  $ps > .59$ . Results on intergroup attitudes regarding social proximity further supported the abstract disconfirmation hypothesis. Abstract disconfirmation increased acceptance of social proximity between groups compared to concrete disconfirmation,  $t(20) = 2.87$ ,  $p = .01$ . In line with this hypothesis, there was no such effect for stereotype confirmation,  $t(20) = -0.78$ ,  $p = .45$ . There were no significant effects on cognitive or affective attitude components,  $ps > .15$ .

**Table 4.1**

*Experiment 4.1 Estimated Marginal Means and Standard Errors (in Brackets) of Intergroup Perceptions by Communication Aim and Communication Level Corrected for Conversation Ease.*

*Note.* < and > indicate (marginally) significant planned contrasts comparing the

	Stereotype confirmation		Stereotype disconfirmation	
	Concrete	Abstract	Concrete	Abstract
<b>Stereotyping</b>				
Positive	4.32 (0.21) =	4.33 (0.25)	4.21 (0.22) <	5.04 (0.16)
Negative	4.19 (0.33) =	4.27 (0.41)	4.20 (0.35) >	3.45 (0.27)
<b>Intergroup attitudes</b>				
Social proximity	4.42 (0.30) =	4.14 (0.36)	3.91 (0.31) <	4.79 (0.24)
Cognitive judgments	4.29 (0.27) =	4.15 (0.34)	4.42 (0.28) =	4.70 (0.22)
Affective reactions	5.47 (0.29) =	5.32 (0.35)	5.17 (0.30) =	5.58 (0.23)

effects of communication level (concrete, abstract) within both communication aims (stereotype confirmation, disconfirmation). Contrasts denoted = are non-significant.

## Discussion

The findings in Experiment 4.1 support the abstract disconfirmation hypothesis over the other three competing hypotheses (i.e., vividness, generalization, interaction). Talking among “us” about “their” stereotype-inconsistent group characteristics seems to have beneficial influences on 1) our attitudes regarding mingling our social lives with theirs, and 2) positive and negative traits we apply to them. However, the effects of abstraction level of communication seem to be less consistent in small group interactions among “us” that focus on confirming negative outgroup stereotypes. The positive effect of abstract disconfirmation only emerged on the social proximity subscale of intergroup attitudes and not on the cognitive and affective components. Likewise, abstract disconfirmation affected application of positive traits more strongly than negative trait application. There are several possible explanations for the emergence of abstract disconfirmation effects on this subset of measures. For instance, the content of the current intragroup discussions may have matched more with some aspects of intergroup attitudes than with others. This is in line with research indicating that attitudes are more easily changed after persuasive messages that match the cognitive or affective basis of attitudes (e.g., Fabrigar &

Petty, 1999; Sherman & Kim, 2002). The abstract disconfirmation manipulation may have primarily affected positive traits because the aim was to generate positive outgroup traits or because it concerned the relatively positive outgroup of elderly people. Although these underlying explanations cannot be tested with the current data, Experiment 4.1 overall provided initial evidence indicating that abstract disconfirmation could improve intergroup perceptions.

## Experiment 4.2

As a next step in testing the potential applicability of abstract stereotype disconfirmation within groups as an intervention to improve intergroup perceptions, Experiment 4.2 aimed to replicate the results from Experiment 4.1 in a more conflictual intergroup context. We chose Moroccan-Dutch adolescents because stereotypes about this group are strong and mainly negative in The Netherlands (Dotsch, Wigboldus, & Van Knippenberg, 2011; Gordijn, Koomen, & Stapel, 2001; Van Prooijen & Coffeng, 2013). However, this target outgroup may pose a potential problem in the concrete communication conditions, which require listing positive outgroup individuals. Native Dutch students in Groningen do not regularly encounter Moroccan-Dutch adolescents. A pilot test among an unrelated psychology students sample in Groningen ( $N = 101$ ; two declined to answer) indicated that no-one encountered Moroccan-Dutch people on a daily basis. Their average score ( $M = 3.30$ ,  $SD = 1.22$ ) was significantly lower than the midpoint on a scale from 1 (*never*) to 7 (*daily*),  $t(98) = -5.70$ ,  $p < .001$ . Experiment 4.2 participants may therefore have difficulty providing examples of Moroccan-Dutch individuals. To solve this, all small-group interactions were between actual students and one confederate who reinforced the manipulations. In the abstract conditions, the confederate made general (positive or negative) remarks about group characteristics of Moroccan-Dutch adolescents. In the concrete conditions, she pretended to have contact with Moroccan-Dutch adolescents, thereby acting as an ingroup member providing (positive or negative) *extended* intergroup contact (Wright, Aron, McLaughlin-Volpe, & Ropp, 1997). Additionally, the pilot test was used to choose the stereotypic traits that group members should disprove during their discussions (i.e., *aggressive, criminal, antisocial*).

Furthermore, in addition to stereotype application and intergroup attitudes, in Experiment 4.2 we added dependent variables to shed more light on the specifics

of individuals' intergroup perceptions. We were interested in two concepts. First, it may be worthwhile to investigate influences on essentialism because this may constitute a potential drawback of abstract intragroup communication about outgroups. That is, abstract communication may foster an abstract and generalizing perception of the outgroup as a relatively invariable, natural category (i.e., outgroup essentialism), which may in turn have detrimental consequences such as prejudice, inhumanization, and stereotype endorsement (Haslam, Bastian, Bain, & Kashima, 2006). Second, we intended to move beyond direct questions regarding intergroup perceptions (e.g., whether an outgroup is seen as competent), which may be subject to social desirability concerns, to more indirect measures. Indeed, we expected that abstract disconfirmation of negative outgroup stereotypes would motivate individuals to give a successful outgroup member more credit for his achievement (i.e., outgroup success attributions characterized by more personal control, more internal locus of causality, more stability, and less external control).

Finally, we included control measures to rule out the possibility that the findings of Experiment 4.1 may have resulted from systematic differences in perceived validation or consensualisation of outgroup views, participants' internal and external motivations to respond without prejudice, or their ingroup and outgroup identification. The latter may show more variance with the target outgroup chosen for Experiment 4.2 (Moroccan-Dutch adolescents) than in Experiment 4.1 (elderly people).

## Method

**Participants and design.** Seventy-eight students (51 women;  $M_{\text{age}} = 20.97$ ,  $SD = 1.85$ ; demographics of one participant missing) were randomly assigned to conditions.<sup>28</sup> The design was identical to Experiment 4.1: A 2 (communication level: Concrete, abstract) X 2 (communication aim: Stereotype-confirmation, stereotype-disconfirmation) factorial design.

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<sup>28</sup> Nine additional participants were deleted prior to analysis because they had non-western immigrant backgrounds (i.e., did not belong to the intended sample).



**Procedure.** The procedure and analytic strategy were identical to Experiment 4.1, except for the target of discussion (i.e., Moroccan-Dutch adolescents) and the presence of one confederate in each group of two to four members who reinforced the manipulations by bringing up condition-consistent information in the discussion. After the debriefing, participants were checked for awareness of the confederate's presence.

**Materials.** All materials were provided on paper. Stereotype-(dis)confirmation and communication abstraction were manipulated via group instructions to spend five minutes discussing why the negative stereotype that Moroccan-Dutch adolescents are aggressive, criminal, and antisocial is true [false], as proven by behaviors of individual Moroccan-Dutch adolescents (concrete communication) or by general characteristics of Moroccan-Dutch adolescents (abstract communication). The bottom of the instructions page provided room to list the top three most illustrative behaviors or characteristics, as part of the manipulation.

Stereotype application and intergroup attitudes were measured as in Experiment 4.1. Four items measured validation (e.g., “My ideas about Moroccan adolescents are right”, 1 *Strongly disagree* - 7 *Strongly agree*) and three others consensualisation (e.g., “During the group interaction, we agreed more and more on how Moroccan adolescents are”). We used Morton, Hornsey, and Postmes's (2009, Study 2) scales to gauge natural kind and reification essentialism. To measure attributions of outgroup success, a scenario adapted from Iatridis and Fousiani (2009) described how Youssef (i.e., a Moroccan-Dutch adolescent) succeeded to get into a very prestigious and demanding postgraduate course. The revised causal dimension scale (McAuley, Duncan, & Russell, 1992) measured causal dimensions underlying participants' attributions of outgroup success on seven-point scales: Personal control, locus of causality, stability, and external control.<sup>29</sup> Internal and external motivations to respond without prejudice were measured with five items each (Plant & Devine, 1998). Ingroup and outgroup identification were measured

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<sup>29</sup> Additional questions about perceived warmth of fellow participants, identification with them, and evaluations of their contributions to the group discussions were not of main interest for the current research focus and, hence, not analyzed. Further information is available upon request.

with single items (Postmes, Haslam, & Jans, 2013). Conversation ease was unintentionally omitted from the present questionnaire.

## Results

Prior to analyses, we removed participants who identified more than the scale midpoint with the outgroup ( $N = 9$ ) or did not believe the confederate was a fellow student ( $N = 2$ ). Because the manipulations were successful (i.e., more correct than incorrect manipulation checks in both levels of both manipulations,  $ps < .001$ ), data of six participants who failed at least one manipulation check were removed. Attribution of outgroup success to stable factors was non-normally distributed, absolute  $z > 3.64$ ,  $p < .001$ . Log-transformation normalized this variable. There were no univariate or multivariate outliers with  $p < .001$ . These procedures resulted in a final sample of 58 cases for analysis (37 women;  $M_{age} = 21.24$ ,  $SD = 1.94$ ).<sup>30</sup> The planned contrasts did not show any effects on the control variables perceived validation, consensualisation, internal and external motivation to respond without prejudice, and ingroup and outgroup identification,  $ps > .15$ .

Overall, the data were again consistent with the abstract disconfirmation hypothesis (see Table 4.2). Unlike Experiment 4.1, none of the planned contrasts affected stereotype application,  $ps > .13$ . However, we did replicate the Experiment 4.1 finding on intergroup attitudes regarding social proximity and found additional support for the abstract disconfirmation hypothesis. Abstract disconfirmation led to marginally more acceptance of social proximity between groups than concrete disconfirmation,  $t(29) = 1.94$ ,  $p = .06$ , marginally less reification than concrete disconfirmation,  $t(29) = -1.76$ ,  $p = .09$ , more attribution of outgroup success to stable factors,  $t(29) = 2.87$ ,  $p < .01$ , and marginally more internal locus of causality,  $t(29) = 1.93$ ,  $p = .06$ . In line with the hypothesis, there were no equivalent effects for stereotype confirmation,  $ps > .95$ . One effect was inconsistent with the abstract disconfirmation hypothesis: Concrete disconfirmation led to less attribution of outgroup success to personal control than abstract confirmation,  $t(29) = -2.10$ ,  $p = .04$ . However, this single effect was not consistent with any of the other hypotheses either. As in Experiment 4.1, there were no effects on the cognitive and affective

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<sup>30</sup> Data of two additional participants could not be analyzed in HLM because they were the only members of their group left (i.e., no group-level data).

attitude subscales,  $ps > .15$ , nor on natural kinds or attribution of outgroup success to external control,  $ps > .11$ .

**Table 4.2**

*Experiment 4.2 Estimated Marginal Means and Standard Errors (in Brackets) of Intergroup Perceptions by Communication Aim and Communication Level.*

	Stereotype confirmation		Stereotype disconfirmation	
	Concrete	Abstract	Concrete	Abstract
<b>Stereotyping</b>				
Positive	4.06 (0.28) =	4.32 (0.30)	4.67 (0.29) =	5.11 (0.20)
Negative	4.61 (0.27) =	4.56 (0.29)	3.69 (0.27) =	3.64 (0.19)
<b>Intergroup attitudes</b>				
Social proximity	3.85 (0.34) =	3.69 (0.38)	4.13 (0.35) <	4.69 (0.24)
Cognitive judgments	3.58 (0.22) =	3.80 (0.24)	4.26 (0.23) =	4.59 (0.16)
Affective reactions	3.81 (0.25) =	3.64 (0.28)	4.27 (0.26) =	4.38 (0.18)
<b>Essentialism</b>				
Reification	3.71 (0.28) =	3.73 (0.30)	3.17 (0.28) >	2.63 (0.20)
Natural kinds	3.08 (0.34) =	3.31 (0.37)	3.10 (0.35) =	3.61 (0.25)
<b>Attributions of outgroup success</b>				
Stability <sup>†</sup>	1.40 (0.05) =	1.43 (0.05)	1.37 (0.05) <	1.51 (0.04)
Internal causality	4.59 (0.13) =	4.50 (0.15)	4.58 (0.14) <	4.85 (0.10)
Personal control	6.12 (0.22) =	5.61 (0.24)	5.84 (0.22) <	6.06 (0.16)
External control	4.77 (0.37) =	4.64 (0.40)	5.00 (0.38) =	5.08 (0.26)

*Notes:* Stability, denoted †, was transformed to normalize its distribution and re-inverted so that higher transformed values correspond to higher original values. < and > indicate (marginally) significant planned contrasts comparing the effects of communication level (concrete, abstract) within both communication aims (stereotype confirmation, disconfirmation). Contrasts denoted = are non-significant.

## Discussion

Replicating and extending the findings of Experiment 4.1 in a more conflicted intergroup context, the pattern of results across variables in Experiment 4.2 supports the abstract disconfirmation hypothesis. Small-group interactions with

ingroup members that focus on positive outgroup characteristics evoke more positive intergroup perceptions than interactions that focus on individual outgroup members' positive behaviors, whereas there is no effect of abstract versus concrete intragroup stereotype confirmation. Despite the broad range of intergroup perceptions affected by abstract disconfirmation (e.g., outgroup essentialism, attributions of outgroup success), Experiment 4.2 failed to replicate the beneficial effects of abstract disconfirmation on application of outgroup stereotypes in Experiment 4.1. That is, in Experiment 4.1 abstract disconfirmation increased application of positive group characteristics to the outgroup and marginally decreased application of negative traits, whereas there was no significant effect in the current experiment. There are several possible explanations for this. For instance, participants may have been more passive because of the presence of an active confederate. Alternatively, the shared views of Moroccan-Dutch adolescents may simply be too robust.

Replicating the findings of Experiment 4.1, the positive effect of abstract disconfirmation again only emerged on the social proximity subscale of intergroup attitudes and not on the cognitive and affective components. Likewise, beneficial effects on other measures surfaced on some subscales (e.g., outgroup reification, attribution of outgroup success to stable factors and internal locus of causality) rather than others (e.g., outgroup natural kinds perception, attribution of outgroup success to external control). These differences between subscales may be mere artifacts of question or response framing. Hence, open-ended questions may provide more comprehensive insight into (changes in) individuals' intergroup perceptions.

Moreover, although the findings of Experiments 4.1 and 4.2 are suggestive of the applicability of intragroup communication as an intervention to improve intergroup perceptions, to conclude that abstract stereotype disconfirmation is a useful intervention would be premature. First, we cannot isolate the cause of differences between abstract and concrete stereotype disconfirmation because we did not include a baseline condition. Although the interpretation that abstract stereotype disconfirmation has a more positive effect than concrete disconfirmation seems reasonable, it could also be that concrete disconfirmation is more negative than abstract disconfirmation. Second, Experiment 4.2 focused on an intergroup context in which interventions to improve intergroup perceptions seem most urgent, yet we found weaker effects of abstract stereotype disconfirmation. Finally, an intervention requiring a confederate would not be feasible in real-life. Experiment 4.3 was conducted to address these issues.

### Experiment 4.3

The aims of Experiment 4.3 were twofold. First, we compared abstract and concrete disconfirmation with a baseline. Second, we modified the instructions to eliminate the necessity of a confederate. This enabled us to test the usefulness of an intervention relying on *natural* intragroup communication in which ingroup members are instructed to disprove negative stereotypes about a truly conflicting outgroup by listing positive outgroup characteristics. Based on our previous results, we expected that such abstract stereotype disconfirmation would yield more positive intergroup perceptions than baseline. We expected that this would not be the case for concrete disconfirmation.

Furthermore, we administered open-ended questions to obtain insight into underlying changes in group members' associations with the outgroup, the ingroup, and self after abstract and concrete stereotype disconfirmation. We were particularly interested in four aspects here. The first two aspects relate to characteristics of language use. Specifically, we were interested in whether concrete disconfirmation would increase vividness of outgroup imagery and decrease language abstraction, or abstract disconfirmation would decrease vividness and increase language abstraction, or both. Another aspect of interest concerns whether or not participants in the experimental conditions list more target stereotypes (aggressive, criminal, antisocial) than participants in the baseline condition. An intervention explicitly mentioning specific outgroup stereotypes may prime people to associate the outgroup *more* with these stereotypes. Such a priming effect would be a potential weakness of the currently proposed intervention.

The final aspect of outgroup associations we were interested in was the extent to which group members engage in description of the outgroup as strange others who are different from us (i.e., outgroup *othering* or “the discursive differentiation between us and them,” Van Houtum & Van Naerssen, 2002, p. 125; Pehrson, Stevenson, Muldoon, & Reicher, 2013; Coupland, 2010). Experiment 4.2 showed that abstract disconfirmation decreases essentialism, thus making intergroup boundaries more permeable (e.g., Martin & Parker, 1995; Rothbart & Taylor, 1992). Moreover, Experiments 4.1 and 4.2 revealed that the abstract disconfirmation condition was associated with the acceptance of social proximity with the outgroup. Based on these combined findings, we expected abstract disconfirmation to reduce outgroup othering. In order to reduce drop-outs or lack of concentration due to an

overly lengthy questionnaire, we decided to save space for the open-ended questions by omitting the intergroup attitudes measure in the current experiment.

To summarize, we hypothesized 1) more positive intergroup perceptions after abstract disconfirmation of negative outgroup stereotypes compared to baseline, and 2) non-significant or only marginal differences between concrete disconfirmation and the baseline.

## Methods

**Participants and design.** One-hundred students (75 women;  $M_{\text{age}} = 23.00$ ,  $SD = 1.32$ ) were randomly assigned to one of three conditions: Abstract or concrete disconfirmation of negative outgroup stereotypes (experimental conditions) or a neutral control condition.

**Materials.** Stereotype disconfirmation and communication abstraction were manipulated via written group instructions to discuss why the negative view of non-native Dutch adolescents as aggressive, criminal, and antisocial is false, as proven by individual non-native Dutch adolescents (concrete communication) or by group characteristics of non-native Dutch adolescents (abstract communication). To account for potential lack of personal intergroup connections, the concrete instructions mentioned names of three famous non-native Dutch people who set a positive example. The control condition comprised written instructions to discuss why the negative view of illegal downloading of music and films is false.

Dependent variables were obtained using Qualtrics. The introduction explained that groups of participants talked about various societally relevant topics, such as immigrant adolescents or illegal downloading of music, and that all participants answered questions about these topics. To support this explanation, the next page displayed questions about illegal downloading of music and films.<sup>31</sup> Outgroup, ingroup, and self associations were measured with three subsequent open-ended question about all words that came to mind when participants thought about

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<sup>31</sup> The questions about music and films were not of theoretical interest and, hence, not analyzed.

(non-)native Dutch adolescents or themselves. We manually coded target stereotypes: Whether or not participants listed outgroup stereotypes explicitly targeted in the intervention instructions. Vividness was coded as visible outgroup characteristics (e.g., *fur collars, dark skin*), and othering as words signaling intergroup boundaries (e.g., *other, different, their own*). These three variables were coded exclusively in outgroup associations. Automated LIWC2007 output (Pennebaker et al., 2007) was used to calculate absolute language abstraction as less articles, numbers (including numerals), and human-related words (e.g., *man, women*; cf. Beukeboom, Tanis, & Vermeulen, 2013). Because language abstraction is linked with personality variables (Beukeboom et al., 2013), we additionally calculated relative, within-person difference scores. Outgroup-ingroup differences were calculated as each participant's language abstraction in outgroup associations minus that of their ingroup associations. Outgroup-self differences were calculated by subtracting self associations from outgroup associations.<sup>32</sup> Stereotyping was measured as in Experiments 4.1 and 4.2. The final part of the questionnaire contained questions on participants' ingroup and outgroup identification (10-point scales), acquaintance with immigrant adolescents, conversation evaluations (ease, interestingness, pleasantness on 7-point scales), and an open-ended question about their comments or questions regarding the experiment.

**Procedure.** The procedure was identical to Experiment 4.1 (small groups'  $n = 2 - 4$ ). After providing informed consent, participants received the group discussion instructions and engaged in a small-group interaction with one to three fellow students (no confederate). In the next phase, the dependent measures and demographic questions were administered individually. Finally, participants were debriefed.

**Analytic Strategy.** Because participants were nested within discussion groups, data were again analyzed in HLM (Raudenbush et al., 2004). Experiment 4.3

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<sup>32</sup> All participants answered items measuring validation of and consensualisation on views regarding immigrants. However, because this topic was never discussed in the control condition (i.e., not validated, nor consensualized upon), planned contrasts would likely be significant. Hence, we did not include these variables in the analyses. An additional sociogram task (Livingstone, Shepherd, Spears, & Manstead, unpublished manuscript) was included but not analyzed for this dissertation due to time constraints.

covered three conditions: Abstract and concrete disconfirmation of negative outgroup stereotypes (experimental conditions) and a neutral control condition. According to the *vividness hypothesis*, concrete disconfirmation of negative outgroup stereotypes should improve intergroup perceptions compared to the baseline whereas abstract disconfirmation should not affect individuals' perceptions. However, Experiments 4.1 and 4.2 suggested more evidence for the *abstract disconfirmation* hypothesis, holding that abstract stereotype disconfirmation should improve intergroup perceptions compared to the baseline, whereas concrete disconfirmation should not.<sup>33</sup> To test these two hypotheses, we translated the three conditions into two dummy-coded variables representing the concrete and abstract disconfirmation conditions respectively (the control condition was always coded 0). The estimated HLM models were:

$$\text{Level-1: } Y = \beta_0 + r$$

$$\text{Level-2: } \beta_0 = \gamma_{00} + \gamma_{01} * (\text{dummyC}) + \gamma_{02} * (\text{dummyA}) + u_0$$

$Y$  represents the dependent variable,  $\beta$  is the individual-level regression coefficient,  $\gamma$ s are group-level regression coefficients, and  $r$  and  $u$  respectively are the individual-level and group-level errors.

## Results

Prior to analyses, we removed one group ( $n = 4$ ) that interacted twice because participants could not answer the questionnaires after their first interaction due to mechanical failure, and four participants who identified more than the scale midpoint with Dutch immigrants. Univariate outliers were not deleted because this would not normalize all distributions. Square-root, log- or 1/x-transformed variables that were (closest to) normally distributed were used in subsequent analyses.

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<sup>33</sup> This abstract disconfirmation pattern would also be consistent with the generalization and interaction hypotheses. However, contrary to the abstract disconfirmation hypothesis, those two hypotheses also predict significant effects of abstraction when groups *confirm* stereotypes. Because Experiment 4.3 only includes stereotype-disconfirming instructions, we merely mention the abstract disconfirmation hypothesis here. Moreover, this is in line with the evidence from Experiments 4.1 and 4.2, providing most support for the abstract disconfirmation hypothesis.



Mahalanobis distance revealed no multivariate outliers with  $p < .001$ . These procedures resulted in a final sample of 94 (70 women;  $M_{\text{age}} = 19.26$ ,  $SD = 1.34$ ).

All analyses were controlled for influences of conversation ease (cf. Experiment 4.1) and conversation pleasantness. This latter variable was added in Experiment 4.3 because a pilot study showed that people experience abstract disconfirmation as less pleasant than concrete disconfirmation (i.e., more uncomfortable and uneasy). In this pilot study, paper-and-pencil scenarios among an unrelated sample of 193 western-European psychology students in Groningen (128 women;  $M_{\text{age}} = 21.74$ ;  $SD = 2.30$ ) indicated that abstract disconfirmation caused participants to feel more relieved,  $t(186) = 4.94$ ,  $p < .001$ , than concrete disconfirmation.<sup>34</sup> This is interesting because it implies that abstract disconfirmation has positive emotional consequences for participants *themselves*, besides improved intergroup perceptions. Nevertheless, abstract disconfirmation also increased negative feelings of uncomfortableness  $t(187) = 2.96$ ,  $p < .01$ , and unease,  $t(187) = 3.67$ ,  $p < .001$ . To control for these negative influences, conversation ease and pleasantness were entered grand-mean centered at the group level and group-mean centered at the individual level in all main analyses (models are presented below).<sup>35</sup> The scenarios revealed no significant effects on pride or refusal to participate in the abstract or concrete disconfirmation tasks,  $ps > .23$ .<sup>36</sup>

Because the dummies did not show any effects on the control variables number of immigrant acquaintances, conversation interestingness, ingroup and outgroup identification,  $ps > .12$ , the subsequently estimated HLM models were:

Level-1:  $Y = \beta_0 + \beta_1 * (\text{conversation ease}^a) + \beta_2 * (\text{conversation pleasantness}^a) + r$

Level-2:  $\beta_0 = \gamma_{00} + \gamma_{01} * (\text{dummyC}) + \gamma_{02} * (\text{dummyA}) + \gamma_{03} * (\text{conversation ease}^b) + \gamma_{04} * (\text{conversation pleasantness}^b) + u_0$

$\beta_1 = \gamma_{10} + \gamma_{11} * (\text{conversation ease}^b) + \gamma_{12} * (\text{conversation pleasantness}^b) + u_1$

$\beta_2 = \gamma_{20} + \gamma_{21} * (\text{conversation ease}^b) + \gamma_{22} * (\text{conversation pleasantness}^b) + u_2$

<sup>34</sup> Thirty-eight additional participants were deleted prior to analysis because they either had non-western immigrant backgrounds or did not answer seriously (i.e., zero variance on dependent variables).

<sup>35</sup> Because one participant did not answer the conversation ease measure, HLM did not include his data in analyses that statistically controlled for this measure.

<sup>36</sup> To reduce positive skew, all variables were  $1/x$ -transformed.

As in the previous experiments,  $Y$  represents the dependent variable (higher means more positive intergroup perceptions),  $\beta$  is the individual-level regression coefficient,  $\gamma$ s are group-level regression coefficients,  $r$  and  $u$  respectively are the individual-level and group-level errors,  $^a$  means group-mean centered, and  $^b$  means grand-mean centered.

**Content coding of outgroup, ingroup, self associations.** Multilevel content analyses on the process variables revealed no effects of dummies on outgroup target stereotypes,  $p$ s  $> .30$ . However, we found that vividness of outgroup associations after concrete disconfirmation of negative outgroup stereotypes did not differ from control,  $t(32) = -0.87, p = .39$ , whereas abstract disconfirmation resulted in less vivid imagery than the control condition,  $t(32) = -3.32, p < .01$ . Again supporting the abstract disconfirmation hypothesis, abstract disconfirmation reduced othering,  $t(32) = -2.49, p = .02$ . This effect was only marginally significant for concrete stereotype confirmation,  $t(32) = -1.73, p = .09$ . The dummies showed no significant effects on outgroup language abstraction absolute or relative to ingroup or self language abstraction,  $p$ s  $> .16$ .

**Stereotyping questionnaire.** Multilevel analyses on the closed stereotyping measures provided additional evidence for the hypothesis. Abstract disconfirmation reduced application of negative stereotypes,  $t(32) = -1.99, p = .05$ , whereas concrete disconfirmation did not,  $t(32) = -1.31, p = .20$ . There were no significant effects on application of positive traits to the outgroup,  $p$ s  $> .58$ . Overall, these results again corroborate the abstract disconfirmation hypothesis (see Table 4.3).

**Table 4.3**

*Experiment 4.3 Estimated Marginal Means and Standard Errors (in Brackets) of Content-Coded*

	Stereotype disconfirmation		
	Control	Concrete	Abstract
<b>Content coding</b>			
Vividness <sup>t</sup>	3.36 (0.41)	2.87 (0.56)	1.44 (0.58)**
Abstraction <sup>t</sup>	0.79 (0.06)	0.90 (0.08)	0.76 (0.08)
Outgroup othering <sup>t</sup>	-0.53 (0.08)	-0.71 (0.11) <sup>o</sup>	-0.80 (0.11)*
<b>Stereotyping</b>			
Negative	4.15 (0.17)	3.85 (0.23)	3.68 (0.24)*
Positive	4.84 (0.14)	4.83 (0.19)	4.95 (0.20)

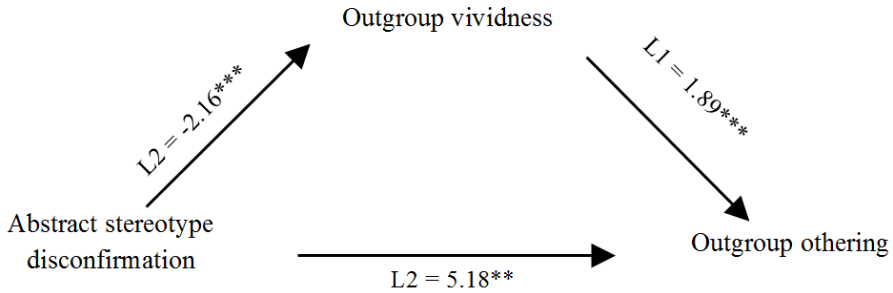
*Variables and Stereotyping by Condition Corrected for Conversation Ease and Conversation Pleasantness.*

*Notes:* All variables denoted † were transformed to (closest to) normal distributions and (if necessary) re-inverted so that higher transformed values correspond to higher original values. Significance of planned contrasts comparing each stereotype disconfirmation condition (concrete, abstract) with the control condition are denoted as: °  $p \leq .10$ , \*  $p \leq .05$ , \*\*  $p \leq .01$ .

**Mediation analyses.** Together, these results suggested a 2-1-1 multilevel mediation, in which the individual-level process variable outgroup vividness mediates the effects of group-level abstract disconfirmation on individual-level outcomes of outgroup othering and negative stereotyping. That is, did participants in the abstract disconfirmation condition engage in less outgroup othering and apply less negative stereotypes because abstract disconfirmation reduces the vividness of participants' mental images of the outgroup? Because Mplus is better equipped to test such upper-level mediations, we used this software to test the mediation patterns suggested by the HLM estimations in multilevel structural equation modelling (Preacher, Zyphur, & Zhang, 2010).<sup>37</sup> Multilevel 2-1-1 mediation analyses estimated a significant indirect effect of abstract disconfirmation on outgroup othering via outgroup vividness,  $b = -6.88$  ( $SE = 2.90$ ),  $p = .02$ , but a non-significant indirect effect on negative stereotype application,  $b = 0.05$  ( $SE = 3.07$ ),  $p = .99$ . These findings are consistent with a partial mediation in which abstract disconfirmation of negative outgroup stereotypes instills less vivid mental images of the outgroup in individuals' minds, and thereby contributes to less focus on how “they” differ from “us” (see Figure 4.1).

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<sup>37</sup> Covariates were not included in the current mediation tests.



**Figure 4.1.** Mediation analysis. A 2-1-1 mediation analysis (Preacher et al., 2010) estimated a significant indirect effect of abstract stereotype disconfirmation on outgroup othering via outgroup vividness,  $b = -6.88$  ( $SE = 2.90$ ),  $p = .02$ . Depicted estimated coefficients are consistent with a partial mediation model in which intragroup communication in which group members collectively disprove negative outgroup stereotypes by listing stereotype-inconsistent group characteristics (versus intragroup communication about an unrelated topic) inhibits the vividness of group members’ mental images of the outgroup, which ultimately reduces their view of the outgroup as “others” who fundamentally differ from “us.” L1 = individual-level coefficients; L2 = group-level coefficients.

\*  $p \leq .05$ . \*\*  $p \leq .01$ . \*\*\*  $p \leq .001$ .

## Discussion

Experiment 4.3 supported the hypothesized effects of 1) more positive intergroup perceptions after abstract disconfirmation of negative outgroup stereotypes compared to baseline, and 2) non-significant or only marginal differences between concrete disconfirmation and the baseline. Compared with neutral intragroup discussions as a baseline, intragroup communication aimed at generating positive outgroup characteristics that disprove negative stereotypes instills positive intergroup perceptions. Analogous effects of collective stereotype disproof based on positive outgroup exemplars were marginal or non-significant. The use of a baseline condition ruled out the alternative explanation that abstract stereotype

disconfirmation is not beneficial but concrete disconfirmation is detrimental for intergroup perceptions. Abstract disconfirmation reduced outgroup othering and application of negative outgroup stereotypes. Although there were no effects on language abstraction, estimations from multilevel structural equation modelling were consistent with a partial mediation model in which collective disconfirmation of negative outgroup stereotypes based on stereotype-inconsistent group characteristics reduces the vividness of mental images of the outgroup, which ultimately reduces their view of the outgroup as “others” who fundamentally differ from “us.” Thus, abstract disconfirmation reduced vividness rather than concrete disconfirmation boosting it compared to a baseline, and outgroup vividness mediated the effect of abstract stereotype disconfirmation on outgroup othering.

In line with Experiment 4.1 (and unlike Experiment 4.2) the current results showed changes in stereotype application. Interestingly, in Experiment 4.3 abstract disconfirmation resulted in less application of negative stereotypes whereas there was no effect on application of positive traits to the outgroup. This is of theoretical interest for two reasons. First, unlike Experiment 4.2 it demonstrates that intragroup communication can be directed to influence rather robust outgroup stereotypes. And second, it disproves the tentative conclusion from Experiment 4.1 that abstract disconfirmation selectively affects positive rather than negative traits because this intervention stimulates generation of positive outgroup traits. In sum, we can conclude that abstract intragroup disconfirmation of negative outgroup stereotypes can indeed improve individuals’ perceptions of a despised outgroup.

Importantly, the lack of effects on frequency of target stereotypes in outgroup associations in Experiment 4.3 implies either that the current instructions, which explicitly mentioned outgroup stereotypes, did not strengthen participants’ outgroup-stereotypic associations or that participants successfully suppressed these associations. Further research could explore this more in-depth. For instance, are these stereotypes so engrained that explicit referencing does not add to the salience of these outgroup associations? Or would differences emerge under cognitive load? In addition to providing food for thought, Experiment 4.3 empirically supported the abstract disconfirmation hypothesis

## General Discussion

The present research tested the effectiveness of an intervention targeting intragroup communication to improve intergroup perceptions. Three experiments provided converging evidence from different intergroup contexts for the abstract disconfirmation hypothesis, advocating the impact of intragroup refutation of negative outgroup stereotypes based on abstract positive outgroup characteristics rather than concrete examples of individual outgroup members' positive behaviors. This hypothesis was supported with interventions based on natural intragroup discussions and with a confederate, with a relatively positive target outgroup (elderly people) and more negative outgroups (Moroccan-Dutch and non-native Dutch adolescents). Intergroup perceptions were improved on various indicators, such as stereotype application, attitudes regarding social proximity to outgroup members, outgroup essentialism, attributions of outgroup success, and outgroup othering. Findings of Experiment 4.3 furthermore suggest that the beneficial influences of abstract stereotype disconfirmation on intergroup perceptions may be partially explained by less vivid mental images of the outgroup. In sum, abstract intragroup disconfirmation of negative outgroup stereotypes improves intergroup perceptions.

These findings contribute to the existing literature in several ways. First, the current results testify to the effectiveness of targeting intragroup communication in interventions to improve intergroup perceptions. Previous research has often focused on ideal forms of intergroup contact that could de-escalate intergroup conflict. Regarding this ideal contact, Pettigrew (1998, p. 69) observed that "Allport's hypothesis risks being an open-ended laundry list of conditions." Moreover, Dixon, Durrheim, and Tredoux (2005) noted that the ideal conditions for intergroup contact are rarely met in practice, and that "in focusing on rarefied forms of interaction, social psychologists have inadvertently widened the gap between theory and practice in contact research" (p. 700). This critical concern may be partially solved by distinguishing between conditions that are facilitating or essential for intergroup contact to improve intergroup relations (Pettigrew & Tropp, 2006). The current research adds to this effort by suggesting another way to address the rarity of ideal intergroup contact in real-life intergroup conflicts. We propose to target intragroup interactions, which are omnipresent (cf. Postmes et al., 2014). By targeting intragroup processes, the current findings extend the existing intergroup conflict literature, which primarily focuses on intra-individual and intergroup processes.

Thus, the current research reveals the promising potential of interventions targeting intragroup communication content and form to improve intergroup perceptions.

The second contribution to the literature concerns the relative power of concrete and abstract communication. The current research tested four competing hypotheses, revealing empirical support for the abstract disconfirmation hypothesis. This implies that intragroup discussion of general outgroup characteristics that are stereotype-inconsistent is more beneficial for intergroup perceptions than intragroup discussion of individual outgroup members who behave stereotype-inconsistently. This is not to say, however, that concrete communication could never impact intergroup perceptions. The current research contrasted concrete and abstract communication instructions to enable testing of their relative effects. It is likely that participants in the abstract conditions also mentioned concrete examples, and vice versa (cf. Nairn & McCreanor, 1991). Nonetheless, the abstract disconfirmation instructions yielded diverse positive outcomes.

### **Limitations and Directions for Future Research**

One important limitation is that, to narrow the focus of the current paper and increase readability, we assessed the usefulness of intragroup interventions in intergroup conflict with measures of intergroup perceptions. There are several reasons for why an interpretation of harmonized intergroup perceptions as indicative of harmonized intergroup relations is too simplistic. For one, in subsequent intergroup contact actual outgroup members may not be as nice or as similar to oneself as envisioned (cf. drawbacks of imagined contact). Moreover, even if improved intergroup perceptions succeed to de-escalate intergroup conflict this newly found harmony may inadvertently contribute to persistence of intergroup inequality by reducing the likelihood that disadvantaged group members engage in collective action (e.g., Dixon, Durrheim, Tredoux, Tropp, Clack, & Eaton, 2010; Saguy, Tausch, Dovidio, & Pratto, 2009; Wright & Lubensky, 2009). However, if advantaged group members stop othering the disadvantaged group as fundamentally different from “us” (as we found in Experiment 4.3), at least one ground for legitimizing intergroup inequality breaks down. This may be essential because positive intergroup perceptions may not hinder collective action if advantaged group members perceive and describe intergroup inequality as illegitimate (Becker, Wright, Lubensky, & Zhou, 2013). Moreover, reduced outgroup othering may lead to less

threat because it decreases the salience of intergroup boundaries (e.g., Gaertner & Dovidio, 2000) and, hence, result in more comfortable intergroup interactions (Trawalter, Richeson, & Shelton, 2009). Although reduced outgroup othering as merely signaling a social connection may not be appreciated by members of stigmatized groups (Rattan & Ambady, 2014), together these findings suggest that abstract negative stereotype disconfirmation within advantaged groups may improve intergroup interactions beyond merely improving intergroup perceptions.

One smaller issue concerns minor inconsistencies between findings across the three experiments. Experiment 4.1 revealed increased application of positive outgroup characteristics, Experiment 4.2 showed no significant effects, and Experiment 4.3 showed less application of negative outgroup traits. This inconsistency may be explained in several ways. One possible explanation involves valence. The target outgroups were relatively positive (elderly, Experiment 4.1) and negative (non-native Dutch adolescents, Experiment 4.3). Discussion of positive group characteristics in the abstract disconfirmation conditions may have affected the most salient valence dimension of the stereotype application measure. Although testing this speculation requires more research with other positive and negative outgroups, both significant effects (Experiments 4.1 and 4.3) testify to the beneficial influences of abstract intragroup disconfirmation of negative outgroup stereotypes. Moreover, the current findings show that this intervention has broader consequences than a mere increase in application of positive outgroup traits after collective generation of positive outgroup traits.

And as a final limitation, the current findings were controlled for conversation ease (Experiments 4.1 and 4.3) and conversation pleasantness (Experiment 4.3). However, we acknowledge that a real-life intervention should result in improved intergroup perceptions without controlling for such factors. One potential solution would be to investigate more in-depth why abstract disconfirmation of negative stereotypes makes a conversation feel uneasy and unpleasant. Anecdotal evidence suggests that this happens because people feel bad about making gross generalizations about outgroups. If this is indeed the case, this problem may be solved by including more explicit instructions explaining that such positive generalizations may seem wrong at first sight but can in fact counter existing negative generalizations such as stereotypes.



## Conclusion

Together, the current experiments provide new theoretical as well as practical insights by suggesting a viable way to intervene in intergroup conflict, accounting for the scarcity of high-quality intergroup contact. The proposed approach to target intragroup communication extends the existing intergroup contact and stereotyping literatures, which mainly revolve around intrapersonal and intergroup processes. Three experiments revealed converging support for the abstract disconfirmation hypothesis. Intragroup communication (natural or with a confederate) about group characteristics that disprove negative outgroup stereotypes (about elderly people, Moroccan-Dutch, or non-native Dutch adolescents) has beneficial influences on intergroup perceptions. Thus, intragroup interventions may contribute to bridging the gap between social psychological theories or ideologies and the tough and persistent practice of social relations in intergroup conflicts.