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Watching the Mimickers:

Mimicry and Identity in Observed Interactions

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Abstract:

Mimicry enhances one's judgements of the mimicker when it is directed towards the self. However, often interactions do not involve only the participants; observers also judge people, and such judgements are influenced by social identities. So, does mimicry also have positive effects even on observers' evaluations of the mimicker? Furthermore, does that hold even if the mimicker is an out-group member? To answer these questions, we used two video-experiments ($N_1 = 377$; $N_2 = 670$) to compare mimicry and neutral (no mimicry) interactions between two individuals who were primed to be in either the participant's in-group or out-group. In both studies, we found the expected negative out-group bias when participants observed the neutral interaction but only for competence-related variables. However, such biases were diminished in the mimicry condition, indicating that mimicry, even when it is merely observed and directed at someone else, may alter mimicker-related attitudes stemming from social identities. Our findings therefore contribute to the literature on reducing intergroup prejudice by demonstrating the behavior-based malleability of a negative out-group bias.

Public Significance Statement: While societal polarisation is increasingly inducing biases against people holding opposing views, we found that observing one individual mimicking the body language of another, might help to reduce such biases in mimicker evaluations.

Keywords: mimicry; social identity; judgement; observation; intergroup relations

Humans naturally "do what others do" (Stel, van Baaren, & Vonk, 2008). This mimicry occurs outside of individual awareness and can manifest itself in multiple ways. Its scope reaches from copying accents to adapting body and facial movements in response to another individual (Chartrand & van Baaren, 2009). Not only has mimicry been understood to serve fundamental human needs such as belonging or affiliation (Lakin & Chartrand, 2003) but it also gives rise to various social consequences. Mimicked people tend to act more pro-socially (van Baaren, Holland, Kawakami, & van Knippenberg, 2004), tend to like the mimicker more (Chartrand & Bargh, 1999), and show a more interdependent self-construal (Ashton-James, van Baaren, Chartrand, Decety, & Karremans, 2007).

To date, studies on mimicry have mostly relied on dyadic interactions in which one person mimics another (e.g., Stel, van Dijk, & Olivier, 2009). In practice, however, such interactions rarely occur in isolation. Instead, individuals who observe the conversation draw conclusions about individual traits based on witnessed behavior even though they are not directly involved (Cohen & Ebbesen, 1979). This perspective provides a novel, previously unexplored lens through which to research mimicry (see a call made by Chartrand & Lakin, 2013). If Sue mimics Beth and this interaction is observed by Julia, does Julia find Sue more competent or more trustworthy? Besides consequences for the individuals directly involved in interactions, the effects of mimicry may therefore extend to individuals observing such interactions. This suggests that mimicry, as a concept, may serve a greater social function than previously anticipated. Accordingly, the first aim of this paper is to explore whether mimicry deployed in an interaction makes the mimicker appear more competent or trustworthy to a passive observer.

Though socially intelligent individuals are inclined to mimic others (Chartrand & Bargh, 1999; Genschow, Klomfar, de Haene & Brass, 2018), this tendency has been shown to be moderated by the social context (Yabar, Johnston, Miles, & Peace, 2006). Specifically,

out-group members are mimicked less than in-group members (Bourgeois & Hess, 2008);

and although recent literature on the related construct of automatic imitation has questioned

such results (see Genschow, Westfal, Cracco, & Crusius, 2021; De Souter, Braem,

Geschnow, Brass, & Cracco, 2021) the degree to which automatic imitation and mimicry are

similar is also questionable (Genschow, van Den Bossche, Cracco, Bardi, Rigone, & Brass,

2017). Thus, whether mimicry elicits effects in intergroup contexts and how such effects

differ from intragroup contexts, especially in observed interactions, is still an open question.

Yet, the prevalence of social identities when observing others appears to be an important

factor to consider, particularly when judgements of individual attributes are made based on

such observations. Considering the example above, how do judgements by Julia change

depending on whether she perceives Sue and Beth as in-group members or out-group

members? As mimicry serves an essential function for humans to navigate social interactions

(Lakin, Jefferis, Cheng, & Chartrand, 2003), it is important to understand how mimicry alters

judgements in various social contexts. Consequently, the second aim of this paper is to

illuminate how the effects of mimicry on an observer are moderated by social identities,

thereby contributing to a deeper understanding of the functioning of mimicry itself.

To our knowledge, the social identity and the mimicry literatures have remained

separate. Although they have similar outcomes regarding interpersonal evaluations, the

theorised mechanisms are different. Indeed, the literature on observed mimicry more

generally is scarce. One study shows that observers of an interaction in which mimicry was

displayed, compared to no mimicry, rated the overall interaction as smoother (Sanchez-

Burks, Bartel, & Blount, 2009). Note, though, that this was examining evaluations of the

interaction and not of the mimicker. In a similarly tangential way, Kavanagh et al. (2011)

demonstrated that the mimickee's behavior can negatively affect observers' judgements of

the mimicker. However, investigating the relationship between mimickee's behavior and

mimicker's behavior on judgements by observers leaves the effect of mimicry itself on such judgements unanswered. To our knowledge, the only studies directly examining the effects of mimicry in observed interactions demonstrate that a mimicker is perceived as more submissive (Geschnow & Alves, 2020) and as more affiliative (Powell & Spelke, 2018) than a non-mimicker. These two studies are in line with the mainstream dyadic mimicry literature which has revealed a plethora of positive, rather than negative, effects of mimicry on interpersonal evaluations (Chartrand & Lakin, 2013). For example, mimickers are perceived as more likable (Chartrand & Bargh, 1999) and as acting more pro-socially (van Baaren et al., 2004). Thus, although very little direct evidence can be found, the related literature all suggests that mimicry, compared to no-mimicry, will positively affect the observer's judgement of the mimicker.

Though behavioral scholars appear to agree on the positive effects of mimicry, adding literature on group biases complicates the hypothesizing. Generally, social identity theory states that individuals categorize themselves and others into social groups, resulting in biases in favour of the in-group and biases against the out-group (Tajfel & Turner, 1979). The strength of these biases, however, depends on the strength of social identities in the specific context (Jackson, 1999). Hypothesizing effects of mimicry within inter- and intragroup contexts, therefore, requires knowledge of the strength of social identity related biases relative to the strength of the effects of observed mimicry. However, as the literature currently offers no indications, we offer competing hypotheses to unpack the relationships.

If the effects of mimicry are stronger than social identity related biases, the positive effects of the former may outweigh the negative biases against out-group members of the latter. Specifically, we expect mimicry to have positive effects on mimicker-related judgements independently from whether the observed individuals are perceived to be in-group members or out-group members. This view aligns with the literature demonstrating

how sufficiently strong or positive contact with members of the out-group reduces negative

out-group biases (see Dovidio, Love, Schellhaas, & Hewstone, 2017; Hewstone, Rubin, &

Willis, 2002).

Moreover, if mimicry mechanisms are stronger than intergroup mechanisms, then we

predict different responses to the outgroup conversation depending on whether mimicry

occurred or not. A conversation observed without mimicry would represent the "usual"

intergroup situation and, given the research showing that individuals are negatively biased

against out-group members compared to in-group members (see Abrams & Hogg, 2010), we

expect such bias to occur in the no mimicry condition.

However, in the mimicry condition, we expect mimicry to improve mimicker-related

judgements due to its known positive effects (see Chartrand & van Baaren, 2009).

Specifically, research on social identities has not only shown that perceived threats are

important predictors of prejudice (Stephan, Ybarra, & Morrison, 2009; Al Raminah &

Hewstone, 2013) but also that intergroup contact can reduce perceived threat (Stephan &

Stephan, 2000; Tausch, Tam, Hewstone, Kenworthy, & Cairns, 2007). As mimicry is

understood as a mechanism to foster pro-social behaviour (van Baaren et al., 2004) and to

signal the need for social inclusion (Lakin, Chartrand, & Arkin, 2008; Lakin & Chartrand,

2013), it may also constitute a mechanism to alleviate perceptions of threat. A positive

experience of intergroup contact due to mimicry may therefore reduce intergroup biases

through reducing perceived threat.

Hypothesis 1a: Mimicry will enhance evaluations of the observed mimicker for all

interactions, but a negative out-group bias will occur in the neutral condition

On the other hand, if social identity related biases are stronger than the positive

effects of mimicry, we expect a different pattern. Equivalent to Hypothesis 1a, we expect to

find negative out-group biases in mimicker-related judgements in the neutral (no mimicry)

condition. However, if group biases are stronger than the effects of mimicry, then mimicry will not overcome the negative out-group biases (Stephan & Stephan, 1993; Ybarra & Stephan, 1994; see also Bail et al., 2018). Even if mimicry was able to produce positive evaluations (see Chartrand & van Baaren, 2009), intergroup contact with strong, salient group identities could suppress the rise of these positive feelings (Hewstone et al., 2002). Accordingly, we would expect a negative out-group bias to persist in the mimicry condition, and propose the following hypothesis competing with 1a.

Hypothesis 1b: In-group members will be evaluated more positively than out-group members regardless of mimicry.

Overall, we argue that it is difficult to hypothesize how mimicker-related judgements are affected by observed mimicry and social identities because the effects of mimicry have not been studied in contexts of social groups. Although there has been some notable neuroscience research on imitation that found individuals imitate less when engaging with a socially undesirable person (e.g., an out-group member), it is important to understand that this work focuses on imitation (i.e., not on mimicry; see, Heyes, 2011) and it examines the reverse causality, that is, imitation as the dependent variable (e.g., Aragon, Sharer, Bargh, & Pineda, 2013). To test our hypotheses, we conduct two separate experiments with different social identities to explore the effects of observed mimicry through the lens of social identities and test competing hypotheses.

As a final contribution, we recognise the potential for practical implications of our research. If mimicry does improve evaluations, then training people to mimic may be considered as a valuable step; but research in dyadic mimicry suggests that the effects of mimicry might only prevail if individuals are not aware of mimicry as a concept (Kulesza, Dolinski, & Wicher, 2016). We therefore also examine whether the effects of observed

mimicry are moderated by the awareness of participants of such non-verbal cues and tested

this in Study 2, hypothesizing that:

Hypothesis 2: The effects of mimicry only occur when observers are not aware of

mimicry.

Across our two studies, then, we examine the effects of observed mimicry, compared

to neutral behaviour, for in-groups and out-groups. Based on the literature, we use a range of

dependent variables ranging from affect-related variables such as likability or trustworthiness

of the mimicker to skill-related variables such as competence or performance appraisal.

Study 1a

Transparency and Openness

The data used for this study are not available. The analytic code used for the main analyses

and examples of stimulus materials are provided in the Supplemental Online Materials.

Participants

We calculated the required sample size a priori via G*Power 3.1 (Faul, Erdfelder,

Lang, & Buchner, 2007). Based on previous research, we estimated that mimicry has medium

to large effect sizes (η^2 = .1; see Hale & Hamilton, 2016b; Stel, Rispens, Leliveld, &

Lokhorst, 2011; d = 0.7; see Sanchez-Burks et al., 2009). As we were examining passive

observers rather than active recipients, we took a conservative approach and used a medium

effect size of f = .25 to calculate the required sample size. Further, we used α = .05,

numerator df = 1, number of groups = 4 (see below), and a power of .95 as input variables as

suggested by Faul et al. (2007). This indicated a minimum sample size of N = 210 and we

over-recruited to ensure adequate power following potential removal of non-respondents or

outliers.

We recruited participants via the online panel Prolific Academic. We preferred

Prolific to other panels due to their ethical standards and because participants tend to be less

experienced with experimental research designs and therefore better represent the average population (Peer, Brandimarte, Samat, & Acquisti, 2017). In line with our social identity manipulation (see below), we set filters via Prolific to narrow the eligibility of participants to those of working age and political affiliation (see below). A total of $N = 247$ individuals participated in the study. In addition to this sample, another 130 participants were recruited to a third behavioral condition that was part of a separate investigation (https://osf.io/2m634). This third condition examined non-responsive (static) behaviour rather than mimicry or no-mimicry and thus was not designed to be included in the research discussed in this paper; we report this data collection for transparency and completeness only. Participants were paid an equivalent of 7.96 US dollars per hour.

Procedure

All studies presented in this research were approved by the Faculty Research Ethics Committee. In line with previous studies, we employed pre-recorded videos to test the effects of mimicry (e.g., Sanchez-Burks et al., 2009; Stel, van den Bos, Sim, & Rispens, 2013) in order to standardize the content across conditions. Before showing the video, we primed the level of categorization between participants so that they perceived the dyad in the video as either in-group members or as out-group members. Participants watched one of two pre-recorded videos in which two male individuals were having a discussion. As detailed later, the content of the discussion, the setting and the behavior of the mimickee were constant across the two videos and the only difference was whether the second individual mimicked or behaved neutrally (that is, no mimicry while not sitting still). Participants were then asked to evaluate the mimicker, providing both the dependent variables and manipulation-checks, as well as providing their own demographic data to control for gender, ethnicity, and age. After the study, participants were fully debriefed and verified their consent.

Materials

Videos.

The videos were recorded in an office setting. The two individuals were dressed in smart casual attire, discussed simple business matters, and were introduced as directors of a tech-company. The content of the conversation was scripted and did not vary across the videos. Therefore, the videos only differed in the movements of one person, the mimicker, who either mimicked the other individual or behaved neutrally but without displaying mimicry. The behavior of the second person, the mimickee, did not differ across conditions. To make sure that the individuals were as comfortable as possible, they decided for themselves who was going to be the mimicker (or non-mimicker, depending on the condition) and who was the counterpart, however both had acting training and were comfortable learning the script and performing in front of the camera. The mimicker was instructed to copy body movements and posture of the mimickee with a slight delay of a few seconds. Regardless of the condition, the background in the videos, the position of the individuals within the videos, and the individuals themselves were the same. By standardizing the content and controlling extraneous variables in this way, we isolated the effect of mimicry. Each of the videos was 3.5 minutes in duration. The link to the videos can be found in the Supplementary Online Materials. Figure 1 illustrates the mimicry and no mimicry condition from the participant's perspective.

Fig. 1 Screenshot of the videos used for the mimicry manipulation (left) and for the no-mimicry manipulation (right). Depending on the group condition, both individuals were either perceived by the participant to be in-

group or out-group members. Participants were then asked to evaluate the person on the left on several dimensions after watching the videos.

To ensure that the mimicry video contained a high degree of mimicry, compared to the no mimicry video, we conducted a short experiment via Prolific with $N = 213$ participants (age: $M = 28.92$ years, $SD = 9.42$ years; gender: *female* = 99, *other* = 3) and randomly allocated participants to either the mimicry or the no mimicry condition. We then asked them to rate the degree of mimicry they observed on a scale from $0 =$ "*No Mimicry*" to $100 =$ "*Full mimicry*". To check that the videos did actually manipulate the level of observed mimicry, we conducted a one-way ANOVA with the behavioral condition as the independent variable and the mimicry measure as the dependent variable. The results indicated a main effect ($F(1, 211) = 198.97$; $p < .001$; $\eta^2 = .49$). As expected, when participants watched the video in which mimicry was displayed, they perceived a higher degree of mimicry ($M = 77.82$; $SD = 17.90$; 95% CI = [74.37, 81.27]), compared to when they watched the video with no mimicry ($M = 35.64$; $SD = 25.10$; 95% CI = [30.83, 40.46]). Accordingly, our behavioural manipulation of observed mimicry was successful.

Priming.

Our research design depended on participants having a particular social identity so that we could manipulate the ingroup/outgroup identity of those in the observed interaction. We chose labor union membership as the primed social identity due to the relevance of labor unions to the workplace and their presence in society more generally (Hofmann, Altreiter, Flecker, Schindler, & Simsa, 2019). We conducted the priming manipulation using two distinct steps. First, we set filters via Prolific to narrow the eligibility of participants to only those who identified themselves with the left side of the political spectrum as they are more likely to support labor unions (we also included a question in the survey to check this). Second, before watching either of the two videos, all participants were informed about the purpose of labor unions through a short introductory paragraph (see Supplementary Online

Materials). In the in-group condition they were told that the individuals in the videos are known in the company to be supporters of labor unions. In the out-group condition, in contrast, they were told that the individuals are known in the company to disapprove of labor unions. Accordingly, the study was designed such that all participants would have their "natural" labor union identity primed while also creating in-group (videoed individuals are also labor union supporters) and outgroup (videoed individuals are not labor union supporters) conditions.

Measures.

Following the mimicry literature we included both affective-related and competency-based measures. Unless otherwise stated, we measured all items on a 7-point Likert scale from *1 =* "*Strongly disagree*" to *7 =* "*Strongly agree*".

Likability and Trustworthiness. To measure perceived likability and trustworthiness, we employed the respective 5-item and 4-item measures developed by Ahearne, Gruen, and Jarvis (1999). Example items included "The person appears to be nice" and "The person is easy to like" for the likability measure (Cronbach's $\alpha = .90$) and "The person is someone I feel I can trust" or "The person tried to mislead the other person" (reverse coded) for the trustworthiness measure (Cronbach's $\alpha = .76$).

Competence. Competence was measured using a 4-item measure (Cronbach's $\alpha = .87$) from Fiske and Cuddy (2006). Example items included "The person appears to be competent" and "The person appears to be capable".

Interactional Justice. We measured the degree of perceived interactional justice with a 4-item measure (Cronbach's $\alpha = .81$) developed by Colquitt (2001). Examples of such items included "The person treated the other person in a polite manner" or "The person treated the other with respect".

Cooperation. The perceived degree of cooperation was measured using a slider-scale (e.g., Effron & Raj, 2019) ranging from 0 = "*No degree*" to 100 = "*Full degree*".

Performance Appraisal. We measured performance appraisal with a 6-item scale adapted from Greenhaus, Parasuraman, and Wormley (1990). Example items include "I would promote this person" and "The person is driven to succeed" (Cronbach's α = .88).

Transformational Leadership. To assess perceived transformational leadership qualities of the mimicker, we employed a 7-item measure developed by Carless, Wearing, and Mann (2000). Example items include "The person communicates a clear and positive vision of the future" and "He treats others as individuals and encourages their development" (Cronbach's α = .88).

Manipulation checks. To check whether our identity manipulation worked, we adapted a version of the IOS-scale originally developed by Aron, Aron, and Smollan (1992). Specifically, the scale comprised seven pictures, each containing three circles representing the participant, the individuals observed, and labor union supporters, respectively. Depending on the option, the distance between the circles, and therefore their overlap, changed gradually. Option 1 showed the greatest overlap between the circles representing the participants and the dyad observed, whereas option 7 showed the greatest distance between these circles. The overlap between the circles representing the participants and union supporters remained unchanged. An in-group situation is one where the circles overlap while an out-group situation is represented by greater distance between the participant circle and the dyad circle. The scale can be viewed in the Supplementary Online Materials.

We also included an attention-check within the questionnaire, a common practice when utilizing online platforms for data collection (Peer et al., 2017). Participants were asked to click "*Strongly agree*" to pass this attention-check. Participants who did not pass the test were excluded from the analysis (see Results). We also asked participants to indicate whether

they noticed anything suspicious and excluded them from the analysis if they reported

noticing mimicry (Hale & Hamilton, 2016a; Stel & Vonk, 2010). Finally, as labor unions

played a fundamental role for our social identity manipulation, we needed to be able to

control for varying labor union support. Therefore, we included a measure asking participants

the extent of their agreement with the statement "*I support labor unions*" on a 7-point Likert

scale from *1* = "*Strongly disagree*" to *7* = "*Strongly agree*".

Results

For the analysis, we excluded participants who failed the attention-check ($n = 7$)

and/or who detected the mimicry manipulation ($n = 5$). Following best practice (Leys,

Delacre, Mora, Lakens, & Ley, 2019), we screened for outliers using absolute deviation

around the median (Leys, Ley, Klein, Bernard, & Licata, 2013) and these were removed ($n =$

22). Data of N = 213 participants were therefore analyzed (gender: *female* = 134, *prefer not

to say* = 3; age: *M* = 32.89 years*, SD* = 5.89 years).

Manipulation check. To check whether our identity manipulation was successful, we

conducted a one-way analysis of variance (ANOVA) with the manipulation check measure as

the dependent variable and the group condition as the independent variable. The analysis

revealed a main effect of group membership ($F(1, 211) = 24.33$; $p < .001$; $d = 0.68$).

Specifically, participants perceived greater overlap (indicated by lower ratings) with the

individuals in the video in the in-group condition (*M* = 2.70; *SD* = 1.23; 95% CI = [2.47,

2.94]) than in the out-group condition (*M* = 3.53; *SD* = 1.20; 95% CI = [3.30, 3.76]).

Accordingly, we had successfully manipulated perceived social group membership.

Analysis of dependent variables. For the remaining part of the analysis, we conducted

a 2 (mimicry: mimicry, no mimicry) × 2 (group membership: in-group, out-group) between-

participants MANOVA and included all dependent variables. We chose a MANOVA instead

of separate ANOVAs because it accounts for correlations among dependent variables (Grice

& Iwasaki, 2007) and can therefore rule out explanations based on a halo effect. Nonetheless, our dependent variables are, at least in part, "*conceptually independent*" (Biskin, 1980, p. 70), thus it is not surprising that the overall multivariate test of the MANOVA is non-significant for the mimicry × group membership interaction ($F(7, 203) = 1.47$; $p = .180$; $\eta^2 = .05$). In such cases, non-significant multivariate tests may be forgiven and between-subject effects examined for each dependent variable (see Huberty & Morris, 1989). The means, standard deviations, and confidence intervals for each dependent variable can be viewed in Table 1. The correlation matrix can be found in the supplementary materials.

Before outlining the specific results, it is worth noting that we also conducted a number of additional analyses to rule out alternative explanations and to check for the robustness of our findings (see Supplementary Online Materials for details on all the following analyses). First, we ran a MANCOVA to control for age, gender, and ethnicity – the results did not differ substantially from the findings reported below, thus ruling out an explanation based on demographic differences. Second, to ensure that the results were applicable to those who were most likely to have a labor union identity, we ran the MANCOVA with the subset of participants who reported high scores on support for labor unions (that is, those who selected response option 5 or higher on the 7-point scale). As expected, given our filtering and sample selection, the majority of the participants supported labor unions (n = 206) and, consequently, the results of the MAN(C)OVA remained unchanged within this subset. Third, we checked that the removal of the suspicious participants had not affected the results; running the MANCOVA with these participants included ($n = 218$) made no difference to our results. Then, to rule out the alternative explanation that the manipulation might have directly affected liking, we conducted a fourth set of analyses where we regressed the dependent variables onto the reported measure of identification, the mimicry condition and their interaction. By substituting the reported

measure of identification for the group condition we are directly testing the proposed

psychological mechanism and thus ruling out other psychological effects that might have

emerged from the videos. The results for these regressions remained largely the same, thus

again demonstrating the robustness of our findings. We have provided a table giving all p-

values and F-values for all main effects and interactions for each of the analyses in the

Supplementary Online Materials.

Table 1

Mean ratings of participants across conditions for each of the dependent variables in Study 1.

		Likability			Trustworthiness		
Behavior	Group	*M*	*SD*	95% CI	*M*	*SD*	95% CI
Mimicry	In	5.19	0.75	[4.99, 5.41]	5.41	0.75	[5.19, 5.63]
	Out	4.67	0.76	[4.45, 4.89]	4.99	0.80	[4.77, 5.22]
No Mimicry	In	5.19	0.75	[4.98, 5.42]	5.39	0.89	[5.17, 5.62]
	Out	4.55	0.89	[4.34, 4.75]	4.83	0.84	[4.62, 5.04]

		Competence			Interpersonal Justice		
		M	*SD*	95% CI	*M*	*SD*	95% CI
Mimicry	In	5.74	0.69	[5.55, 5.93]	6.24	0.53	[6.07, 6.41]
	Out	5.55	0.69	[5.35, 5.75]	5.99	0.67	[5.81, 6.17]
No Mimicry	In	5.99	0.63	[5.78, 6.19]	6.43	0.61	[6.25, 6.61]
	Out	5.38	0.82	[5.19, 5.56]	6.09	0.73	[5.93, 6.27]

		Performance Appraisal			Transformational Leadership		
		M	*SD*	95% CI	*M*	*SD*	95% CI
Mimicry	In	5.25	0.67	[5.07, 5.44]	5.24	0.72	[5.05, 5.44]
	Out	5.00	0.66	[4.81, 5.19]	4.94	0.73	[4.73, 5.15]
No Mimicry	In	5.53	0.69	[5.34, 5.73]	5.59	0.79	[5.38, 5.80]
	Out	4.91	0.76	[4.73, 5.09]	4.89	0.76	[4.70, 5.09]

		Cooperation		
		M	*SD*	95% CI
Mimicry	In	82.95	12.21	[79.69, 86.20]
	Out	79.78	13.24	[76.37, 83.19]
No Mimicry	In	87.28	10.71	[83.87, 90.69]
	Out	77.12	12.61	[73.95, 80.29]

Note. *M* = mean; *SD* = standard deviation; CI = confidence interval.

Competence. The results revealed no main effect of mimicry ($F < 1$; $p = .727$; $d = 0.01$; $\eta^2 < .01$), but there was a significant main effect of group membership ($F(1, 209) = 16.41$; $p < .001$; $d = -0.55$; $\eta^2 = .07$) and a significant mimicry × group membership interaction ($F(1, 209) = 4.49$; $p = .035$; $\eta^2 = .02$; see Figure 2). Participants rated a neutrally behaving out-group member as less competent ($M = 5.38$; $SD = 0.82$; 95% CI = [5.19, 5.56]) compared to a neutrally behaving in-group member ($M = 5.99$; $SD = 0.63$; 95% CI = [5.78, 6.19]), indicating a negative out-group bias ($t(106) = -4.35$; $p < .001$; $d = -0.84$). This bias, however, diminished in the mimicry condition ($t(103) = -1.40$; $p = .164$; $d = -0.28$). Surprisingly, mimicking in-group members received *lower* competence ratings ($M = 5.74$; $SD = 0.69$; 95% CI = [5.55, 5.93]) than neutrally behaving in-group members (see above), but this difference was of marginal significance ($t(103) = 1.88$; $p = .063$; $d = 0.37$). The interaction remained ($F(1, 206) = 3.44$; $p = .065$; $\eta^2 = .02$) when we controlled for gender ($F < 1$; $p = .365$; $\eta^2 < .01$), age ($F < 1$; $p = .400$; $\eta^2 < .01$), and ethnicity ($F(1, 206) = 7.06$; $p = .009$; $\eta^2 = .03$). Thus, we find partial support for Hypothesis 1a for competence evaluations.
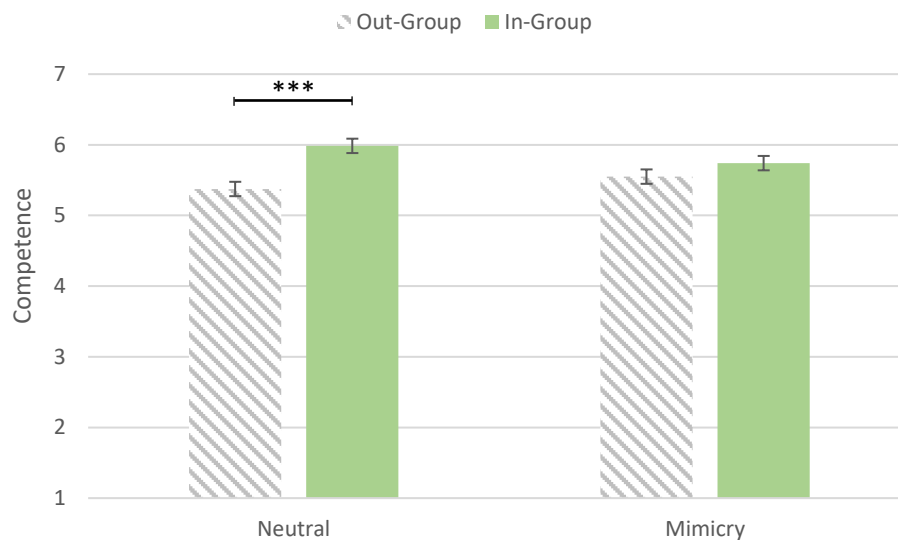

Fig. 2 *Mean competence ratings as a function of mimicry condition and group membership. Asterisks indicate a significant statistical difference between conditions rounded to two decimal places (***p < .001). Error bars represent ±1 SE.*

Performance Appraisal. We found a main effect of group membership ($F(1, 209) =$ 21.06; $p < .001$; $d = -0.63$; $\eta^2 = .09$), as well as a mimicry × group membership interaction ($F(1, 209) = 3.83$; $p = .052$; $\eta^2 = .02$), but no main effect of mimicry ($F < 1$; $p = .322$; $d = 0.08$; $\eta^2 < .01$) for our performance appraisal measure. Separate group comparisons showed a similar pattern to our competence measure as illustrated in Figure 3 and in support of Hypothesis 1a. Participants showed signs of a negative out-group bias on performance appraisal ratings of the mimicker in the no mimicry condition ($t(106) = -4.46$; $p < .001$; $d = -0.85$). Specifically, participants gave lower appraisals in the out-group condition ($M =$ 4.91; $SD = 0.76$; 95% CI = [4.73, 5.09]) compared to the in-group condition ($M = 5.53$; $SD =$ 0.69; 95% CI = [5.34, 5.73]). However, such biases were diminished in the mimicry condition ($t(103) = -1.942$; $p = .055$; $d = -0.38$). There was a significant effect when we compared performance appraisal ratings for mimicking in-group members to those of not mimicking in-group members ($t(103) = 2.14$; $p = .035$; $d = 0.42$); indicating that participants viewed the performance of mimicking in-group members ($M = 5.25$; $SD = 0.67$; 95% CI = [5.07, 5.44]) less favourably than non-mimicking in-group members (see above). When we controlled for gender ($F < 1$; $p = .361$; $\eta^2 < .01$), ethnicity ($F < 1$; $p = .348$; $\eta^2 < .01$), and age ($F < 1$; $p = .682$; $\eta^2 < .01$), the interaction remained ($F(1, 206) = 4.26$; $p = .040$; $\eta^2 = .02$), again providing partial support for Hypothesis 1a.
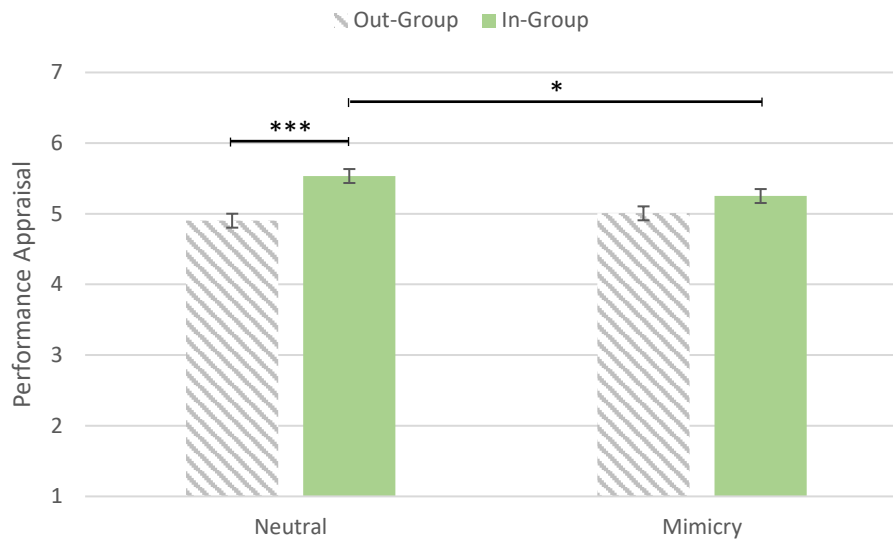
Fig. 3 *Mean performance appraisal ratings as a function of mimicry condition and group membership. Asterisks indicate a significant statistical difference between conditions rounded to two decimal places (***p < .001). Error bars represent ±1 SE.*

Cooperation. Though the analysis did not reveal a main effect of mimicry ($F < 1$; $p = .619$; $d = 0.03$; $\eta^2 < .01$), we found a main effect of group membership ($F(1, 209) = 15.71$; $p < .001$; $d = -0.54$; $\eta^2 = .07$), as well as a mimicry × group membership interaction ($F(1, 209) = 4.33$; $p = .039$; $\eta^2 = .02$). Separate group comparisons demonstrated a negative out-group bias in the no mimicry condition ($t(106) = -4.53$; $p < .001$; $d = -0.86$). In particular, participants perceived the observed interaction as less cooperative when the individuals observed were perceived as out-group members ($M = 77.12$; $SD = 12.61$; 95% CI = [73.95, 80.29]) compared to when they were perceived as in-group members ($M = 87.28$; $SD = 10.71$; 95% CI = [83.87, 90.69]). In the mimicry condition, however, this negative bias did not exist ($t(103) = -1.27$; $p = .205$; $d = -0.25$), as illustrated in Figure 4. Similar to competence, the lower cooperation ratings of mimicking in-group members ($M = 82.95$; $SD = 12.21$; 95% CI = [79.69, 86.20]) compared to neutrally behaving in-group members (see above) were of marginal significance ($t(103) = 1.93$; $p = .057$; $d = 0.38$). The interaction remained ($F(1, 206) = 5.24$; $p = .023$; $\eta^2 = .03$) when we controlled for gender ($F(1, 206) = 4.73$; $p = .031$; $\eta^2 = $

.02), age ($F(1, 206) = 2.99$; $p = .085$; $\eta^2 = .01$), and ethnicity ($F < 1$; $p = .535$; $\eta^2 < .01$). We therefore found partial support for Hypothesis 1a for cooperation evaluations.
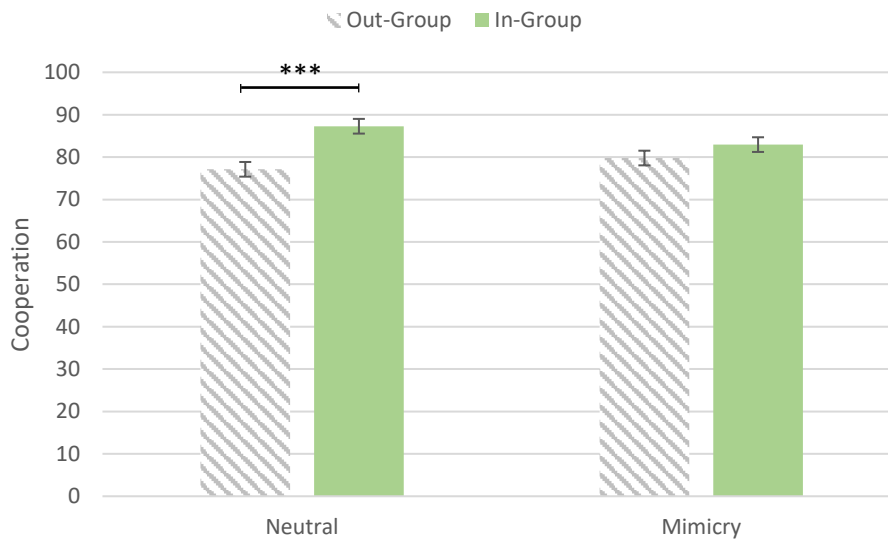

Fig. 4 *Mean cooperation ratings as a function of mimicry condition and group membership. Asterisks indicate a significant statistical difference between conditions rounded to two decimal places (***p < .001). Error bars represent ±1 SE.*

Transformational Leadership. The analysis revealed a main effect of group membership ($F(1, 209) = 23.46$; $p < .001$; $d = -0.65$; $\eta^2 = .10$), a marginal mimicry × group membership interaction ($F(1, 209) = 3.59$; $p = .060$; $\eta^2 = .02$), but no effect of mimicry ($F(1, 209) = 2.17$; $p = .142$; $d = 0.16$; $\eta^2 = .01$). However, once we controlled for gender ($F(1, 206) = 5.04$; $p = .026$; $\eta^2 = .02$), ethnicity ($F(1, 206) = 1.82$; $p = .179$; $\eta^2 = .01$), and age ($F < 1$; $p = .551$; $\eta^2 < .01$), the mimicry × group membership interaction was significant ($F(1, 206) = 4.38$; $p = .038$; $\eta^2 = .02$). Separately for each mimicry condition, we then conducted a one-way analysis of covariance with social group membership as the dependent variable. The results showed a negative out-group bias in the neutral condition ($F(1, 103) = 22.07$; $p < .001$; $\eta^2 = .18$), but not in the mimicry condition ($F(1, 100) = 3.71$; $p = .057$; $\eta^2 = .04$), providing partial support for Hypothesis 1a.

We did not observe any other interactions or main effects of mimicry for likability, trustworthiness, and perceived interactional justice (see p-values and F-values in the Supplementary Online Materials). Given that we conducted multiple tests, we needed to control for multiple comparisons. To do so, we used the False Discovery Rate (FDR) proposed by Benjamini and Hochberg (1995). As the FDR procedure controls the error rate for all the tests performed it is preferred to other methods that merely account for the probability of making one type I error (see Noble, 2009). Following the FDR procedure, we ordered the p-values for the mimicry × group membership interaction for the seven dependent variables from lowest to highest and assigned them their rank accordingly. The formula suggested by Benjamini and Hochberg (1995) was used to calculate the coefficient q for each dependent variable ($q = (i/m)Q$, where i = rank, m = total number of tests, in this case 7, and Q = false discovery rate set to .05). Whenever q was greater or equal to the corresponding p-value, then the result of q became the new, multiple comparison adjusted p-value. Comparing our p-values with q, the smallest p-value is smaller than the critical value q, indicating that all higher-ranked p-values (that is, all our tests) are significant. Accordingly, α remains at 0.05 and we have further support for the validity of our findings.

Discussion

Study 1 explored the effects of observed mimicry on mimicker-related judgements and the role of social identities in moderating such effects. Interestingly, univariate tests revealed significant interactions for those variables that seem more work and competence-related (competence, performance appraisal, transformational leadership; see also correlation matrix) but the univariate tests were not significant for the more general, affective-related variables (e.g., liking and trustworthiness). In these former interactions, we found the expected negative out-group bias in the no-mimicry condition but importantly, such out-group bias was diminished in the mimicry condition.

These findings indicate that mimicry, even when observed, may help to overcome intergroup biases in context-related evaluations. According to our competing hypotheses, these results suggest that the effects of mimicry may be stronger than the effects of social group membership (Hypothesis 1a), which would have considerable implications for inter-group research. However, we found few main effects for mimicry behaviour suggesting perhaps that the mimicry effect does not overwhelm the out-group bias completely. Moreover, the split in the findings between general and context-related evaluations and the fact that moderating effects of social identities may vary depending on the type and the relevance of specific social categories (see Shah, Brazy, & Higgins, 2004; see also Jackson, 1999), indicate the need to replicate our results.

Before conducting a second study, however, we needed to rule out an alternative explanation for our results. Though intergroup biases were not observed in the mimicry condition, it seems that observed mimicry may potentially both reduce evaluations of an in-group mimicker, at least when related to performance. In other words, the benefit of reconciling intergroup relations through mimicry may come at the cost of appraising the performance mimicking in-group members more negatively, compared to non-mimicking in-group members. This finding is somewhat unexpected and although its examination is not the main aim of this paper, it highlights the need to rule out an alternative explanation for our results before conducting another study with the aim of replicating the results of Study 1a.

Despite the effort we have taken to standardize our mimicry manipulations as thoroughly as possible, one may argue that it is not mimicry per se but rather differences between the two videos in the mimicker's body movements and speech patterns (e.g., intonation) that are difficult to standardize and may thus explain the revealed effects. Such underlying patterns may have also caused the unexpected pattern of reduced in-group ratings in the mimicry condition. Accordingly, we tested this alternative explanation in Study 1b.

Study 1b

Transparency and Openness

The data used for this study are not available. The analytic code used for the main analyses

and examples of stimulus materials are provided in the Supplemental Online Materials.

Participants

We recruited N = 100 participants (gender: *male* = 45, *prefer not to say* = 1; age: *M* =

30.11 years, *SD* = 10.53 years) from Prolific and paid them an equivalent of 9.44 US Dollars

per hour to complete the survey.

Procedure

We followed the same procedure as in Study 1a. However, as we were interested in

ruling out an alternative explanation to the results of Study 1a rather than examining effects

of mimicry in intergroup contexts, we did not need to prime social group categories.

Accordingly, we allocated participants randomly to either the mimicry condition or the no

mimicry condition.

Materials

Videos.

We amended the videos in such a way as to isolate the mimicker's behaviour from the

mimickee. Such a design allowed us to examine the mimicker's behavior and speech patterns

and whether these differed across the two conditions. Accordingly, we cropped the videos

from Study 1a so that only the mimicker was visible, not allowing participants to see whether

the mimicker did or did not mimic his counterpart. The links to the cropped videos can be

found in the Supplementary Online Materials.

Measures.

We used the same dependent variables as measured in Study 1a. Additionally, we

used two new measures to assess the mimicker's body movements and speech patterns. To

assess the former, we selected three items used by Granziol, Spoto, and Videtto (2017) and measured them on a 7-point Likert scale from *1 = "Strongly disagree"* to *7 = "Strongly agree"*. "The person moves spontaneously, shifts position, and moves arms" is an example item. We assessed the mimicker's speech patterns using an adapted 4-item measure developed by Hecht and LaFrance (1995), on a 7-point Likert scale from *1 = "Strongly disagree"* to *7 = "Strongly agree"*. "The person has a clear pronunciation" and "The person's voice is monotone" (reverse coded) are example items. Finally, as in Study 1a, we included an attention-check.

Results

The correlation matrix can be found in the Supplementary Online Materials. For the analysis, we removed $n = 8$ participants who did not pass the attention-check. Accordingly, we included data of $N = 92$ participants in our analysis. As in Study 1a, we conducted a between-participants MANOVA with the behavioral condition as the independent variable and our measures as dependent variables. As expected, we did not find any effect of the behavioral condition on the measures employed in Study 1a ($F < 1$; $p > .342$; $\eta^2 < .01$). Importantly, we also did not find any difference in participant's ratings of the mimicker's body movements ($F < 1$; $p = .632$; $\eta^2 < .01$) and speech patterns ($F < 1$; $p = .369$; $\eta^2 = .01$) between the conditions. These results remained unchanged when we conducted a between-participants MANCOVA to control for gender, age, and ethnicity.

We conducted Study 1b to rule out the alternative explanation for our results found in Study 1a. Specifically, one could have argued that differences in the mimicker's behavior and speech patterns between the conditions (i.e., videos) may have caused the effects rather than mimicry, compared to no mimicry. However, by isolating the mimicker across the conditions and assessing participant's perceptions of his body movements and speech patterns in addition to our existing dependent variables, we found no evidence for this alternative

explanation. Instead, the results found in Study 1a appear to stem from the difference between neutral behavior and mimicry, as intended.

With this in mind, we conducted another study and used the same videos to check the generalisability and replicability of our findings from Study 1a with a different primed identity. Conducting a second study also allowed us to explore whether such effects are moderated by the awareness of participants.

Study 2

Transparency and Openness

The data used for this study are not available. The analytic code used for the main analyses and examples of stimulus materials are provided in the Supplemental Online Materials.

Participants

Using the same method as in Study 1a and six groups (instead of four), the required minimum sample size was 210 which was met through access to a wider sample. A total of N = 497 individuals participated in the study. As in Study 1a, an additional n = 173 participants were part of a non-responsive (static) condition for a separate investigation which we report here for reasons of transparency and completeness (https://osf.io/2m634). Participants were paid an equivalent of 5.23 US dollars per hour.

Procedure

The procedure was similar to the one described in Study 1a. Again, we recruited participants via the online panel Prolific and they were told that we wanted to see how communication is perceived by others but not that we were specifically examining mimicry. The only procedural difference to Study 1a was the inclusion of the awareness manipulation before watching the video. To standardize the content across conditions and studies, we used the same pre-recorded videos as described in Study 1a. After the study, participants were fully debriefed and verified their consent.

Materials

Priming.

In this study, we based our priming strategy on research suggesting that merely imagining a social context can lead to the same effects as experiencing it directly (Bargh, Chaiken, Raymond, & Hymes, 1996; Garcia, Weaver, Moskowitz, & Darley, 2002). Participants were asked to imagine that they work in the same company as the individuals in the video. For the outgroup condition, we asked participants to imagine themselves to be the Chief Executive Officer (CEO) of that company, meaning that they perceive the dyad in the video (i.e., directors) as out-group members (see Brewers' (1991) argument for optimum distinctiveness). For the ingroup condition, we asked participants to imagine themselves to be colleagues of the directors displayed in the video leading to a perception of shared group-membership (see supplementary materials).

Awareness.

To manipulate the awareness of participants, we employed a method similar to Kulesza et al. (2016). In the awareness condition, we told participants that the director on the left side of the video (i.e., the mimicker), had attended a non-verbal behavior course for how to come across as more competent. We refrained from making the manipulation too obvious to simulate a real-life scenario. In the no awareness condition, participants were only told to pay attention to the conversation they were about to watch.

Measures.

Dependent Variables. Similar to Study 1a, we used measures for general affective-related variables (e.g., liking, trustworthiness). Due to the high correlations among the work-specific, competence-related variables used in Study 1a and 1b (see correlation matrices), we only measured competence in Study 2.

Manipulation Checks. To check whether participants were aware of their relationship with the recorded individuals, they were asked to indicate the perspective from which they perceived the dyad in the video with the options a = "*Colleague*", b = "*Board member*", c = "*CEO*", d = "*Competitor*", or e = "*None of the above*". Only data provided by participants who answered the latter question correctly were used for the analysis.

Similar to Study 1a, we included an attention-check in our questionnaire and also ensured that participants were given the opportunity to indicate whether they noticed anything suspicious. If participants noticed mimicry in the no awareness condition, we excluded them from the analysis (see Results; Hale & Hamilton, 2016a; Stel & Vonk, 2010).

Results

For the analysis, we excluded participants who did not provide final consent (n = 1), who failed the attention-check (n = 37), and/or the relationship test that underpinned the group-condition (n = 71). Participants in the no awareness condition who detected the mimicry manipulation were also excluded (n = 13), as were outliers (n = 22) based on the absolute deviation around the median (Leys et al., 2013). Data of N = 353 participants were therefore analyzed (gender: *female* = 172, *prefer not to say* = 3; age: M = 30.99 years, SD = 8.71 years)[1].

For the remaining part of the analysis, we conducted a 2 (mimicry: mimicry, no mimicry) × 2 (group membership: in-group, out-group) × 2 (awareness: awareness, no awareness) between-participants MANOVA to account for correlations among our dependent variables (Grice & Iwasaki, 2007). As mentioned in Study 1a, however, we did not further consider the overall multivariate effects of the mimicry × group membership × awareness interaction (F < 1; p = .721; η^2 < .01) because of the conceptual independence of the variables (Huberty & Morris, 1989; Biskin, 1980). The means, standard deviations, and confidence intervals for each dependent variable can be viewed in Table 2. The correlation matrix can be

found in the supplementary materials. Similar to Study 1a, we also report the results of a

MANCOVA controlling for age, gender, and ethnicity. We have provided a table containing

all p-values and F-values for all main effects and interactions in the Supplementary Online

Materials.

Table 2

Mean ratings of participants across conditions for each of the dependent variables in Study 2.

Behavior	Group	Awareness	Likability			Trustworthiness		
			M	*SD*	95% CI	*M*	*SD*	95% CI
Mimicry	In	No	4.79	1.09	[4.49, 5.10]	4.97	0.85	[4.66, 5.28]
		Yes	4.77	1.01	[4.47, 5.06]	4.72	1.17	[4.43, 5.02]
	Out	No	5.15	0.96	[4.86, 5.44]	5.03	0.99	[4.74, 5.32]
		Yes	4.96	0.82	[4.67, 5.24]	4.94	0.84	[4.65, 5.22]
No Mimicry	In	No	4.69	0.94	[4.38, 5.00]	5.08	1.00	[4.77, 5.39]
		Yes	4.69	1.03	[4.39, 4.98]	4.72	0.97	[4.43, 5.01]
	Out	No	4.53	0.94	[4.25, 4.82]	4.65	0.88	[4.37, 4.94]
		Yes	4.69	1.09	[4.41, 4.96]	4.54	1.13	[4.26, 4.82]

Behavior	Group	Awareness	Competence			Interpersonal Justice		
			M	*SD*	95% CI	*M*	*SD*	95% CI
Mimicry	In	No	5.26	1.02	[4.93, 5.58]	5.71	0.95	[5.43, 5.99]
		Yes	5.34	1.14	[5.02, 5.65]	5.59	0.86	[5.33, 5.87]
	Out	No	5.30	1.10	[4.99, 5.61]	5.79	0.76	[5.53, 6.06]
		Yes	5.58	0.89	[5.28, 5.89]	5.61	0.83	[5.35, 5.87]
No Mimicry	In	No	5.23	0.94	[4.90, 5.56]	5.64	1.07	[5.35, 5.92]
		Yes	5.42	0.97	[5.11, 5.72]	5.56	0.87	[5.29, 5.82]
	Out	No	4.88	1.04	[4.58, 5.18]	5.32	0.89	[5.06, 5.58]
		Yes	5.03	1.21	[4.74, 5.32]	5.53	0.94	[5.28, 5.78]

Behavior	Group	Awareness	Cooperation		
			M	*SD*	95% CI
Mimicry	In	No	79.13	15.88	[74.09, 84.16]
		Yes	73.58	17.91	[68.73, 78.44]
	Out	No	76.93	15.79	[72.13, 81.73]
		Yes	74.17	16.54	[69.48, 78.87]
No Mimicry	In	No	78.79	15.99	[73.69, 83.89]
		Yes	72.71	15.91	[67.97, 77.46]
	Out	No	72.32	16.23	[67.68, 76.96]
		Yes	71.96	15.17	[67.41, 76.51]

Note. *M* = mean; *SD* = standard deviation; CI = confidence interval.

Likability. As shown in Figure 5, results indicated a main effect of the mimicry condition on the likability of the mimicker ($F(1, 345) = 6.41$; $p = .012$; $d = 0.27$). In particular, participants expressed greater liking for the mimicker when he mimicked ($M = 4.92$; $SD = 0.97$; 95% CI = [4.77, 5.07]) compared to when he did not mimic ($M = 4.65$; $SD = 1.00$; 95% CI = [4.50, 4.79]). We did not observe any main effect of group membership ($F(1, 345) < 1$; $p = .368$; $d = 0.08$) or awareness ($F(1, 345) < 1$; $p = .867$; $d = 0.02$). However, we found a marginal mimicry × group interaction on the likability of the mimicker ($F(1, 345) = 2.81$; $p = .095$; $\eta^2 = .01$). The main effect of mimicry ($F(1, 342) = 6.58$; $p = .011$; $d = 0.27$) and the marginal mimicry × group interaction ($F(1, 342) = 2.73$; $p = .099$; $\eta^2 = .01$) were still observed after controlling for age ($F < 1$; $p = .574$; $\eta^2 < .01$), ethnicity ($F(1,342) = 2.29$; $p = .131$; $\eta^2 = .01$), and gender ($F < 1$; $p = .437$; $\eta^2 < .01$).
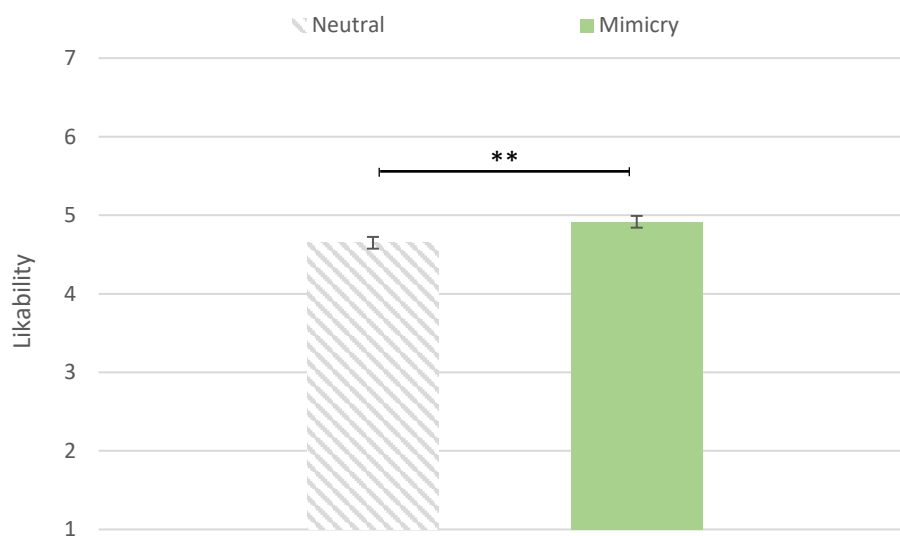

Fig. 5. *Mean likability ratings as a function of mimicry condition. Asterisks indicate a significant statistical difference between conditions rounded to two decimal places (**p ≤ .01). Errors bars represent ±1 SE.*

Competence. As illustrated in Figure 6, the analysis revealed a main effect of mimicry on perceived competence of the mimicker ($F(1, 345) = 4.19$; $p = .041$; $d = 0.23$) but no main effect of group membership ($F(1, 345) < 1$; $p = .321$; $d = 0.11$) or awareness ($F(1, 345) = 2.42$; $p = .121$; $d = 0.17$). We observed a mimicry × group membership interaction ($F(1, 345)$

= 5.24; p = .023; η^2 = .02). Similarly, when we controlled for age (F < 1; p = .805; η^2 < .01),

ethnicity ($F(1, 342)$ = 1.98; p = .160; η^2 = .01), and gender (F < 1; p = .907; η^2 < .01), we still

observed the main effect of mimicry ($F(1, 342)$ = 4.28; p = .039; d = 0.22) as well as the

interaction ($F(1, 342)$ = 5.36; p = .021; η^2 = .02). We then explored the groups separately.

When the participants perceived the dyad in the video as colleagues, or in-group members,

competency ratings of the mimicker, compared to a non-mimicker, did not differ ($t(165)$ =

0.20; p = .839; d = 0.04); but when participants perceived the dyad as subordinates, or out-

group members, competence ratings differed when we compared the two behavioral

conditions ($t(184)$ = −3.11; p = .002; d = 0.45). Specifically, participants rated a mimicking

out-group member as more competent (M = 5.44; SD = 1.00; 95% CI = [5.22, 5.66]) than a

non-mimicking out-group member (M = 4.96; SD = 1.13; 95% CI = [4.75, 5.17]).

Importantly, however, when we compared in-group competence ratings with out-group

competence ratings separately for each mimicry condition, we found an effect of group

membership when participants observed no mimicry ($t(178)$ = 2.39; p = .018; d = 0.35).

Specifically, participants perceived a non-mimicking in-group member as more competent

(M = 5.33; SD = 0.95; 95% CI = [5.09, 5.55]) than a non-mimicking out-group member. This

effect, however, disappeared in the mimicry condition ($t(171)$ = −0.92; p = .358; d = 0.14),

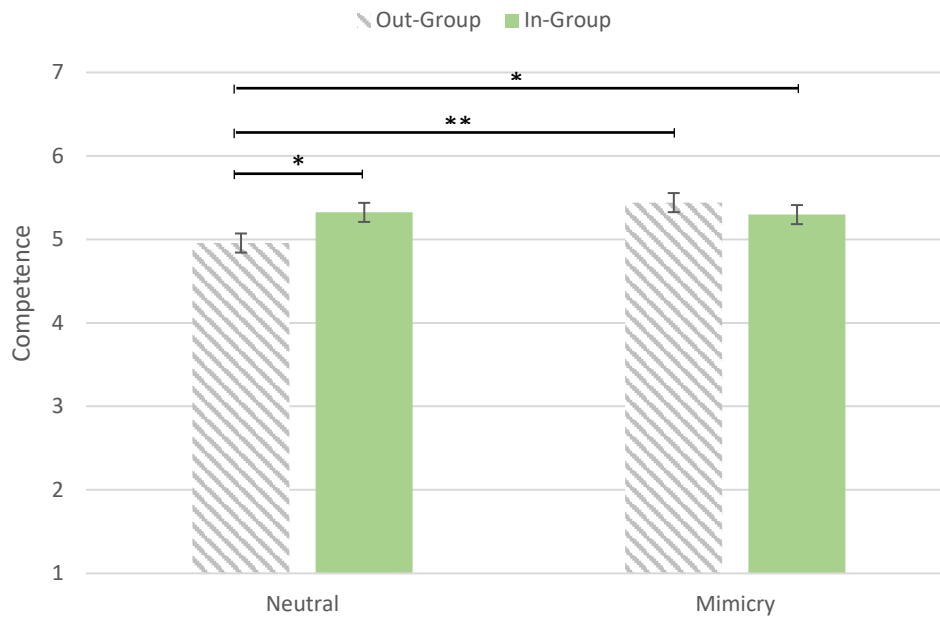suggesting that observing mimicking behavior reconciles group effects.

Fig. 6 *Mean competence ratings as a function of mimicry condition and group membership. Asterisks indicate a significant statistical difference between conditions rounded to two decimal places (*p < .05; **p ≤.01). Error bars represent ±1 SE.*

Trustworthiness. While the analysis revealed a main effect of the awareness condition ($F(1, 345) = 3.79$; $p = .052$; $d = 0.20$), we did not observe a main effect of the mimicry condition ($F(1, 345) = 2.43$; $p = .120$; $d = 0.18$) or the group condition ($F(1, 345) < 1$; $p = .433$; $d = 0.08$) on the trustworthiness of the mimicker. However, we found a mimicry × group interaction ($F(1, 345) = 4.34$; $p = .038$; $\eta^2 = .01$), illustrated in Figure 7. As we controlled for gender ($F(1, 342) = 1.04$; $p = .309$; $\eta^2 < .01$), age ($F < 1$; $p = .464$; $\eta^2 < .01$), and ethnicity ($F < 1$; $p = .882$; $\eta^2 < .01$), the main effect of awareness diminished ($F(1, 342) = 3.42$; $p = .065$; $d = 0.21$) but we still observed the interaction effect ($F(1, 342) = 4.59$; $p = .033$; $\eta^2 = .01$). A separate analysis of groups revealed that, as with competence, mimicry did not affect the degree of trust towards an in-group member ($t(165) = 0.29$; $p = .767$; $d = 0.04$). Trust towards an out-group member, however, was affected by mimicry ($t(184) = -2.72$; $p = .007$; $d = 0.40$). Specifically, participants trusted a mimicking out-group member more ($M = 4.98$; $SD = 0.92$; 95% CI = [4.78, 5.19]) than a non-mimicking out-group member ($M = 4.59$; $SD = 1.01$; 95% CI = [4.39, 4.79]). Further, we observed a difference between groups in the

no-mimicry condition ($t(178) = 1.94$; $p = .054$; $d = 0.29$). Participants trusted non-mimicking

in-group members more ($M = 4.89$; $SD = 0.99$; 95% CI = [4.69, 5.11]) than non-mimicking

out-group members. This effect disappeared in the mimicry condition ($t(171) = -0.94$; $p =$
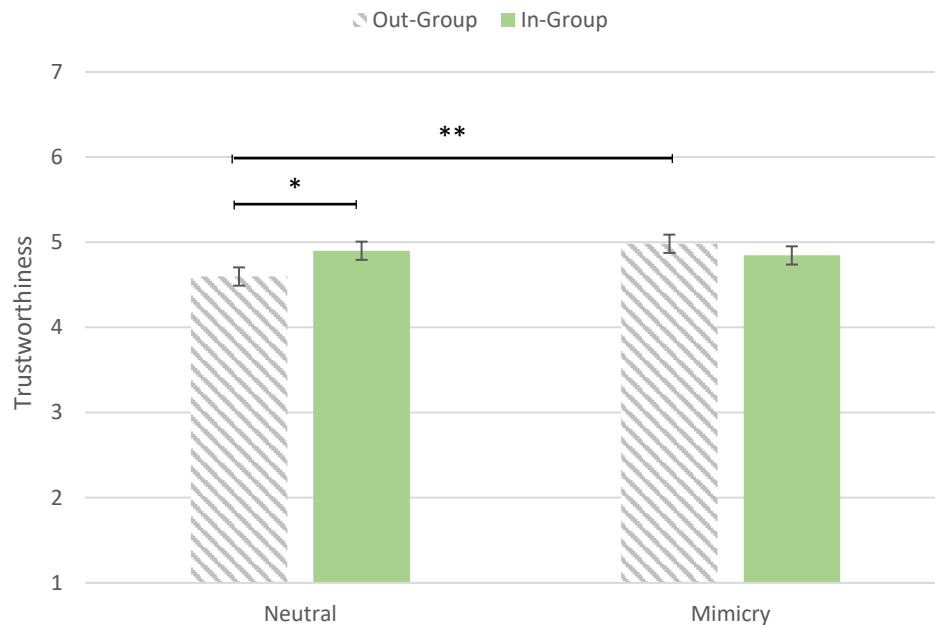
$.347$; $d = 0.14$).

Fig. 7 Mean trustworthiness ratings as a function of mimicry condition and group membership. Asterisks

indicate a significant statistical difference between conditions rounded to two decimal places (*p ≤ .05; **p <

.01). Error bars represent ±1 SE.

Interactional justice. Using perceived interactional justice as the dependent variable,

the results only revealed a marginal main effect of mimicry ($F(1, 345) = 3.07$; $p = .081$; $d =$

0.19) but no effect of group membership ($F(1, 345) < 1$; $p = .509$; $d = 0.07$), or awareness

($F(1, 345) < 1$; $p = .671$; $d = 0.04$), nor did they reveal interactions. The main effect of

mimicry remained marginal ($F(1, 342) = 2.98$; $p = .085$; $d = 0.19$) after we controlled for

gender ($F(1, 342) = 2.79$; $p = .096$; $\eta^2 = .01$), age ($F < 1$; $p = .442$; $\eta^2 < .01$), and ethnicity (F

< 1; $p = .453$; $\eta^2 < .01$).

Cooperation. With regards to perceived cooperation, the results did not reveal main

effects of mimicry ($F(1, 345) = 1.35$; $p = .246$; $d = 0.13$) or group membership ($F(1, 345) =$

1.63; $p = .202$; $d = 0.13$), nor did they reveal interactions. However, we observed a main effect of the awareness condition ($F(1, 345) = 4.55$; $p = .034$; $d = 0.22$). Specifically, when participants were not aware of mimicry, they perceived the interaction as more cooperative ($M = 76.79$; $SD = 16.08$; 95% CI = [74.35, 79.24]) compared to when they were aware of mimicry ($M = 73.11$; $SD = 16.26$; 95% CI = [70.75, 75.46]). The main effect of awareness ($F(1, 342) = 4.35$; $p = .038$; $d = 0.22$) remained when we controlled for gender ($F < 1$; $p = .732$; $\eta^2 < .01$), age ($F < 1$; $p = .577$; $\eta^2 < .01$), and ethnicity ($F < 1$; $p = .874$; $\eta^2 < .01$).

As in Study 1a, we used FDR to control for multiple comparisons (see Benjamini and Hochberg, 1995). Similar to Study 1a, none of the values satisfied the equation and thus α remains at 0.05 for our tests performed.

General Discussion

Humans are constantly surrounded by others whom they observe and judge, and such judgements are influenced by social identities. The main aim of this paper was to explore whether, and how, the effect of observed mimicry is affected by social group membership. Using two online video studies, participants were shown a dyadic interaction in which either mimicry or no mimicry was displayed by one individual towards the other. Before participants watched the video, we manipulated their social identities so that they perceived the dyad in the video as either fellow in-group members or as out-group members. We tested competing hypotheses as we integrated two previously detached bodies of literature. In general, we found support for the premise that the effects of mimicry outweigh the negative out-group bias and that this occurred regardless of whether the participant had a potential external attributional explanation (body language training) at hand. As expected, we found negative out-group biases in our neutral condition that represents the "normal" interaction (for further discussion, see below). More importantly, we found an interaction that supports the view that mimicry may help to overcome intergroup biases. Negative out-group biases

existed in the no mimicry condition in terms of lower ratings of competence (Study 1a, Study

2), leadership (Study 1a), performance (Study 1a), cooperation (Study 1a), and

trustworthiness (Study 2). The results across the two studies, however, show that such biases

were not apparent in the mimicry condition.

Our findings thus have important implications. First, observed mimicry, compared to

no mimicry, may contribute to a positive intergroup experience and, therefore, reduce the

threat perceived by the out-group (Abrams, Eller, Bryant, 2006; see also Tredoux &

Finchilescu, 2010). Accordingly, the results appear to align with the literature on positive

intergroup contact and how it can, under the right circumstances, diminish out-group

prejudice (for reviews, see Crisp & Turner, R., 2009; Dovidio et al., 2017). These are the first

studies that suggest that intergroup prejudice may decline when individuals watch others

engage in mimicry. Thus, the results offer insights into the possible behavior-related

malleability of biases stemming from social identities. Practically, our results imply that

when we observe an interaction of out-group members, mimicry displayed in that interaction

may be useful to reduce intergroup animosities that an observer may have. Considering that

observers were at no point in direct contact with out-group members and that mimicry was

directed at someone else, our findings are noteworthy. Though scholars have demonstrated a

positive impact of intergroup contact, or even imagined contact (Turner, R., Crisp, &

Lambert, 2007; Turner, R. & Crisp, 2010), on reducing out-group biases (Turner, R.,

Hewstone, Voci, & Vonofakou, 2008; Turner, R., Hewstone, & Voci, 2007), they have often

focused on attitudes towards the out-group as a whole. Our study, in contrast, contributes to

the literature on intergroup contact (e.g., Brown & Hewstone, 2005) by showing that an out-

group bias measured at the individual level—that is, for a specific representative of the out-

group—can be reduced through observed non-verbal mimicry. It is also important to note that

it was not intergroup contact per se, but rather intergroup contact in conjunction with observed mimicry that reduced an out-group bias in mimicker judgements.

Second, our findings also contribute to an enhanced understanding of how we evaluate others. While research had already demonstrated that mimicry may increase the likeability of the mimicker (Chartrand & Bargh, 1999), such findings were based on dyadic mimicry where the participant was in direct contact with the mimicker. Our research extends such findings by revealing that mimicry can (in intergroup scenarios) also affect mimicker evaluations when the mimicry occurred in an interaction that the participant merely observed. This finding implies that the effects of human mimicry extend beyond the dyad and points to mimicry as a broader affiliative concept and offers a range of practical applications (Genschow et al., 2020). Be it during a televised debate, an interview, or on a train, wherever we may observe out-group individuals, mimicry between these individuals may reduce intergroup bias perceived by an independent observer. Indeed, the potential for such observation to be scaled up using mass media to reach millions of viewers (e.g., internet, TV) would enable these bias-reduction effects to have substantially greater impact on bias-reduction throughout society than those occurring only within dyadic interactions.

Third, given that a lot of research on intergroup relations focuses more on polarized groups based on religion (Turner, R. & Crisp, 2010) and less on applied group categories such as labor union support, our results further demonstrate the practical value of mimicry. We showed that when participants evaluated an out-group member who was, for example, a junior colleague (Study 2), mimicry by that out-group member towards another out-group member made the participant favour the mimicker more, compared to when no mimicry occurred. Although more research is required, we believe that mimicry among intragroup individuals may be a useful mechanism to diminish such applied intergroup biases when the individuals are observed by others. Considering that individuals in intragroup interactions are

already more likely to engage in mimicry (e.g., Aragon et al., 2013), it may be further

evidence that demonstrates the evolutionary purpose of mimicry.

We also feel the need to briefly elaborate on our effect sizes. Across our studies, the

effect sizes for our mimicry × group membership interaction were small. This might be due

to the lower realism in online experiments compared to real-life settings, thus reducing the

impact of our manipulations (see e.g., Cummins, Roche, Tyndall, & Cartwright, 2018).

Nonetheless, given the context we believe that the effects we found are sufficiently large to

have a practical value, because they can affect multiple individuals simultaneously – such as

viewers of a televised interview, as discussed above – and they can affect one individual

multiple times (Greenwald, Banaji, & Nosek, 2015). Indeed, novel research findings such as

ours such should not be dismissed purely based on effect size (Funder & Ozer, 2019) as they

will stimulate further research able to detect larger effect sizes as the methods develop.

Instead, future research could explore whether there are greater cumulative bias reduction

effects from repeated observations of out-group mimicry as this would have important

practical implications.

We showed across both studies that observed mimicry may help to overcome

intergroup discrimination in terms of mimicker evaluations. However, given the lack of

research on mimicry in intergroup contexts, the psychological mechanisms at work are less

clear. While reduced in-group mimicker ratings in the mimicry, compared to the no mimicry

condition, appear to have been one driver of the results in Study 1a, the pattern was different

for Study 2. Here, higher out-group mimicker evaluations in the mimicry condition,

compared to the no mimicry condition, led to a reduction of intergroup biases. One possible

reason may be that neutral behavior (i.e., our no mimicry condition), may not be normal. As

individuals have been shown to display some natural degree of mimicry in their interactions,

displays of no mimicry may be subconsciously perceived as somewhat atypical. Another

reason may be related to the different social identities used in both studies and their

respective relevance for participants (see below). Given that the underlying reasons for why

intergroup biases do not exist when participants observe a person mimicking another person

were not the main focus of this paper, we leave it to future research to investigate this further.

Relatedly, we received support for our thinking that membership of different social

groups, possibly due to their context-specific relevance, may moderate the effects differently

(see also Jackson, 1999; Shah et al., 2004). Though our main finding—related to reduced

intergroup biases in the mimicry condition—was evident across the two studies, mimicry per

se increased out-group ratings in Study 2 only. Specifically, observed mimicry, compared to

no-mimicry, increased perceptions of competence and trustworthiness for out-group

members. One of the possible reasons for this difference between the studies may be the

varying relevance of the social categories. Research has demonstrated that the impact of

behaviour on evaluations of members of social groups depends on their context-related

relevance, or strength (Christensen, Rothgerber, Wood, & Matz, 2004). In particular, the

stronger social identities are, the more antipathy is expressed towards members of different

groups and the more rapport is expressed towards members of the same group (Gibson &

Gouws, 2000; for discussions on identity strength, see Howard, 2000). Taken to the extreme,

stronger biases can make individual behaviour of out-group members (and in-group

members) irrelevant for judgements so that biases persist (see also Reicher, Spears, &

Postmes, 1995). Though not such an extreme case, it may well be that the social categories

used in Study 2, namely imagined leader or colleague, were less strong compared to the

social categories in Study 1a, namely labor union supporter or non-supporter. Although we

conducted a robustness-check in Study 1a to ensure that the results did not differ when we

included labor union supporters only in our analysis, this difference in strength of the social

category may have allowed out-group behaviour per se (i.e., mimicry) to elicit effects in

Study 2 but not in Study 1a.

This difference may also underpin the disparity in findings across the univariate tests

of affective-related evaluations and more competency-based evaluations. Both identities that

were primed were workplace-related, as was the videoed interaction that was the basis of the

evaluations. As such, it could be that the workplace-related evaluations were more salient and

thus these were the only ones affected by mimicry and intergroup biases. This is an

interesting finding and one that needs more investigation. Nonetheless, we find support for

this argument in the literature showing that competence evaluations but not evaluations of

likability and trustworthiness are affected by mimicry in job interview settings (Kavanagh et

al., 2011). Similarly, when the context of the interaction was trust-related, mimicry

influenced trustworthiness of the mimicker but not competence-ratings (Kavanagh et al.,

2013, see also Verberne, Ham, Ponnada, & Midden, 2013).

On a broader scale, our findings also challenge the assumption that mimicry is an

affiliative signal limited to the dyad (Salazar-Kämpf et al., 2018). Advancing previous studies

which show that witnessing mimicry can influence overall judgements of conversations

(Sanchez-Burks et al., 2009), Study 2 demonstrates that interpersonal judgements can also

improve merely as a result of observing mimicry. Therefore, we see particular value in our

findings for impression management theory as they advance our understanding of ingratiation

(Bolino, Long, & Turnley, 2016). Although our study, like previous ones (Stel et al., 2013),

employed pre-recorded videos, future research could explore whether similar effects occur

when individuals observe the interactions in-person. We hope that our findings provide a

starting point for this new perspective on mimicry research and for its application to

situations in which individuals observe interactions of others (e.g., job interviews and

teaching).

However, the results need to be treated with caution. Despite the benefits of the method, the fact that we employed an online-study, in which participants imagined a scenario (Study 2), might have led to less relevant social group categories (Turner, J., Hogg, Oakes, Reicher, & Wetherell, 1987). Consequently, it was possible for participants to cognitively switch or mix social identities (Roccas & Brewer, 2002) which might have affected our results. Accordingly, further research could examine the effects of mimicry with more relevant social groups, such as gender in an in-person experiment, to provide a more comprehensive understanding of mimicry in different social contexts. Relatedly, we need to mention the trade-off between the internal and external validity of our experimental manipulations. We aimed to use practically relevant inter- and intragroup scenarios to enhance the applied value of our experiments. Therefore, our group manipulations were conducted in the context of labor union (non-) support and leader-follower relationships. While this increased external validity, it may have decreased internal validity by adding confounding factors. For example, our leader-follower manipulation (Study 2) may also have manipulated power or status differences which may have affected our results. Hence, while challenging, future research should replicate our experiments with contexts offering higher internal validity without sacrificing external validity unduly. Although we standardized our videos as much as possible, we also feel the need to highlight that we did not control for the spatial orientation of the individuals on the videos. Given that the mimicker (or non-mimicker, depending on the condition) was always on the left and the counterpart on the right, there may have been some confounding effects. Future research should thus counterbalance these spatial orientations, as Genschow and Alves (2020) have done.

Additionally, in line with the literature (see Christensen et al., 2004), we argued throughout this paper that the way social identities moderate the effects of mimicry depends on their context-related relevance. Consequently, we do not suggest that our findings can be

generalized to observed mimicry in all intergroup contexts. Instead, we show that mimicry may be understood as a powerful social mechanism that may reduce intergroup prejudice, even when it is observed. Accordingly, we encourage researchers to build on our findings and use different social groups to further explore the effects of mimicry.

On a more general note, we acknowledge that it may be somewhat challenging for participants to distinguish between some of our variables. We measured several different constructs, such as likeability, trust, and competence; and although these may be treated as conceptually independent, the psychological literature emphasises that individual judgements on one dimension may affect judgements on other dimensions (for research on the halo effect, see Nisbett & DeCamp Wilson, 1977). This may be one of the reasons why we found high correlations among some of our variables, particularly the competence-related measures, though such correlations are not uncommon in psychological research (Bakker, van Dijk, & Wicherts, 2012). However, it is equally important to recall that our study was exploratory. In particular, we aimed to test how mimicry, if at all, alters intergroup prejudice measured on several dimensions. The fact that participants may not have been able to distinguish between all of the constructs we measured, was thus a trade-off we accepted in exchange for a broader exploratory approach. Nonetheless, future research may benefit from further disentangling the dimensions for which observed mimicry may alter intergroup prejudice, perhaps by including further, more conceptually distinctive dependent variables.

We also would like to briefly elaborate on our exclusions. Given that we conducted online surveys to collect our data, we needed to exclude those participants who did not pay attention to the questions. Thus, we included an attention-check and excluded participants who did not pass the check, as suggested by Peer et al. (2017). Given that outliers distort the results and their interpretation, we also felt the need to exclude those. We thus used absolute deviation around the median as research has shown it is more robust, compared to more

traditional ways of outlier detection (see Leys et al., 2013). The effects of non-verbal mimicry, as a process of subconscious influence, have been shown to diminish when participants become aware of it (see Kulesza et al., 2019; see also below). We thus needed to exclude participants who became aware of our mimicry manipulation, as commonly done in mimicry research (Hale & Hamilton, 2016a; Stel & Vonk, 2010). It is thus important to notice that although we did not pre-register such exclusions and despite their limitations in terms of potential biases, we decided on those in advance and followed best-practice procedures to do so.

Finally, we explored whether individual awareness of mimicry influences the effects of observed mimicry on mimicker-related judgements. Though an effect has been demonstrated in the literature on dyadic mimicry (Kulesza et al., 2016), we did not observe an interaction and only found one main effect. One possible reason for this might be that our awareness manipulation was less obvious compared to other studies. However, we aimed to create a subtle, implicit manipulation, because a more explicit method might have shifted the focus of participants to the mimicker as opposed to the overall interaction.

Overall, when combining our insights with the literature, mimicry crystallises as vigorous mechanism which influences not only individuals it is directed to, but also individuals who merely observe it. Additionally, this effect appears to be moderated by social identity in a way that allows out-group members to reconcile negative, context-related biases observers have against them. These key findings, we hope, advance the mimicry literature as a whole to provide both a more comprehensive explanation of social behavior and also a promising new mechanism for reducing intergroup bias.

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[1] We measured age in categories and then calculated the mean and the standard deviation.