

**Can imagination reduce prejudice over time? A preregistered test of the imagined
contact hypothesis**

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Abstract

Research on the imagined contact hypothesis suggests that simply imagining a positive interaction with an out-group member can reduce prejudice towards stigmatized social groups. To date, however, it remains unclear whether imagined contact has transient or long-lasting effects. This preregistered study (N = 153) tested the hypothesis that a single session of imagined contact is sufficient for reducing explicit and implicit prejudice towards a stigmatized social group and intergroup anxiety over several days. Highlighting the power of imagination, the results suggest that imagined contact could have long-lasting effects on explicit prejudice and intergroup anxiety.

Keywords: imagined contact, positive interaction, explicit and implicit prejudice, intergroup anxiety, long-lasting effects

Introduction

Contemporary research in social psychology suggests that simply imagining a positive interaction with an out-group member can improve intergroup tolerance and reduce prejudice (Crisp & Turner, 2009). This notion—the imagined contact hypothesis—has been tested in more than 70 studies with a mean effect size of Cohen’s $d = 0.35$ (for a meta-analysis, see Miles & Crisp, 2013). Interestingly, imagined contact has been found to improve explicit and implicit prejudice (Crisp & Turner, 2009), emotions (West, Holmes, & Hewstone, 2011), physiological reactions (West & Turner, 2014), and behavior towards out-group members (West, Turner, & Levita, 2015). Thus, imagined contact can be considered a highly simple and efficient strategy for reducing prejudice.

Limitations of previous studies

There are at least two important limitations of prior research that may dampen enthusiasm for this promising new intervention. First, most studies conducted so far have been under-powered and have not controlled for the false discovery rate. In a notable exception, Klein et al. (2014) conducted a high-powered ($N = 6344$) preregistered replication of Husnu and Crisp (2010, Study 1), but found only weak support for the imagined contact hypothesis. Although the effect was in the expected direction, it was much weaker than in the original study, and it would not have been reported as significant with a small sample size. The results of Klein et al. (2014) suggest that the true effect size of imagined contact on prejudice is overestimated in the extant literature. Similarly, a recent large comparative investigation of 17 interventions for reducing implicit racial prejudice found no evidence that imagined contact could reduce racial prejudice in two studies with relatively large sample sizes ($N = 216$ and

267; Lai et al., 2014). These independent studies cast doubt on the robustness of the imagined contact hypothesis.

Second, most studies conducted to date have focused on the immediate and short-term effects of imagined contact (Miles & Crisp, 2013), and very few studies have tested whether these effects are maintained over time (Brown & Paterson, 2016). This is an important issue because an immediate change in attitudes following a brief psychological intervention may not necessarily translate into long-term change (Lai et al., 2016). Thus, further research should examine whether imagined contact can reduce prejudice towards stigmatized social groups over a relatively long period of time while controlling for the false positive rate. To address this issue, in this preregistered study, we examined whether a single session of imagined contact could reduce prejudice towards a highly stigmatized social group (persons with schizophrenia) over two to three weeks.

Imagined contact and long-lasting effects

Although the idea that a single brief session of imagined intergroup contact could have effects that persist over more than a few minutes might seem surprising at first glance (Lai et al., 2016), some recent data hint that this may, indeed, be the case. In particular, a recent study suggests that a single session of imagined contact could successfully improve one's perception of people with intellectual disabilities and the effects could last for one month (Falvo, Capozza, Hichy, & Di Sipio, 2014). In this study, participants were asked to mentally simulate a positive encounter with an individual with an intellectual disability (in the imagined contact condition) or a pleasant landscape (in the control condition). Then, participants ascribed primary and secondary emotions to persons with intellectual disabilities. One month later, they

completed the same emotion attribution measure. The results showed greater attribution of primary emotions compared to secondary emotions regarding persons with intellectual disabilities. It is thought that the tendency to deny these people's humanity was reduced in the imagined contact condition compared to the control condition. Interestingly, the beneficial effect of imagined contact persisted at the one-month follow-up.

In another study, international students who imagined contact with a citizen of their host country before leaving their own country reportedly spent more time with native-born citizens during their stay and were associated with more positive out-group evaluations seven months after the experimental manipulation (Vezzali, Crisp, Stathi, & Giovannini, 2015). These findings, if replicable, are impressive as they suggest that a single session of imagined contact lasting about five minutes can drastically change one's out-group attitude for several days, weeks, or even months.

This study and others¹ have provided encouraging evidence that a single session of imagined contact could have long-lasting effects. However, to paraphrase Laplace (1812), this is a very strong claim than requires extraordinarily strong evidence. Given some recent failures to replicate reports in the literature (Klein et al., 2014; Lai et al., 2014) and the well-known problem of publication bias in scientific fields (Open Science Collaboration, 2015), it seems safe to say that more research is needed to test the long-lasting effects of imagined contact on prejudice.

The present (preregistered) study

This study aimed to replicate a previous experiment investigating the effects of imagined contact—Study 4 performed by West et al. (2011)—with three important differences. First, we preregistered all the material, design, and hypotheses before data

collection to control for the false positive rate (and ensure flexibility in data analysis). Second, we included a delay of two to three weeks between the imagined contact intervention and the dependent measures. This was done to enable a critical test of the hypothesis that simply imagining a positive interaction with a person belonging to a stigmatized social group can improve one's attitudes towards other members of that social group. Third, we included measures of explicit as well as implicit prejudice, as imagined contact has been found to have a large effect on implicit prejudice (Crisp & Turner, 2009; see Lai et al., 2014). A secondary aim of the present study was to test the hypothesis that a single session of imagined contact reduces implicit as well as explicit prejudice.

In the present study, we tested the hypothesis that a single five-minute session of imagined contact could reduce explicit and implicit prejudice towards people with schizophrenia for two to three weeks. This period of time was used for both practical and theoretical reasons. It is quite long compared to the few-minute-long imagined intervention and the very short delay between the intervention and outcome measures that is typically used in most studies on imagined contact. Thus, the use of a delay of two to three weeks considerably extends the realm of effects that are typically examined in the literature. We also tested the hypothesis that a single session of imagined contact could reduce intergroup anxiety, that is, anxiety regarding the prospect of future interaction with persons with schizophrenia. Previous research suggests that intergroup contact reduces prejudice by attenuating intergroup anxiety (Swart, Hewstone, Christ, & Voci, 2011; Vezzali et al., 2015).

Method

The study was preregistered on the Open Science Framework

(<https://osf.io/sqtjm/registrations/>) using a template recommended for social psychology studies (Van't Veer & Giner-Sorolla, 2016). The preregistered protocol (<https://osf.io/zf5ke/>) included a detailed description of the hypotheses and planned analyses; the materials for the imagined contact and control conditions; the experiment script used to measure the dependent variables and collect the data; and the algorithm for computing the implicit measure of prejudice.

Participants

The participants were 153 undergraduate students (116 women and 37 men, $M_{\text{age}} = 20.24$, $SD = 2.51$) who took part in the study in exchange for course credit. Following the preregistered protocol, one participant who did not complete the contact intervention and one who was an outlier according to the measure of anxiety were excluded from the main analyses. However, the results are similar when these participants are included in the analyses.

Procedure

Two female experimenters (one for each session) collected the data in a laboratory (for a video of the lab set-up, see <https://osf.io/4vy3w/>). Participants went to the laboratory two times, separated by 15.18 days ($SD = 2.84$). The first time, participants took part in an unrelated experiment. At the end of that experiment, the participants were asked to complete another brief experiment with the control or imagined contact intervention. The experimenter was blind to the condition and hypothesis. When the participants came to the laboratory the second time, they completed the dependent measures. The second experimenter was also blind to the condition.

The procedure was modeled after that used by West et al. (2011) in Study 4.

Participants were randomly allocated to the control condition ($n = 75$) or the imagined contact condition ($n = 76$). In the two conditions, participants first read the following text:

We would like you to take a minute to imagine the following scenario. Imagine that you are waiting at a crowded train station for a train to Paris. Shortly after you find a seat, you see two other people enter the train station—*Tom Harrell* and *Arturo Sandoval*. *Tom Harrell* is a jazz trumpeter and composer who loves the music of Louis Armstrong. After being diagnosed with schizophrenia, he continued to compose and play music, releasing several chart-topping albums. He stopped taking anti-psychotic drugs, finding that his music helps him cope with his illness. *Arturo Sandoval* is a jazz trumpeter and composer who owns a jazz venue in Bordeaux. He has played with many well-known jazz artists and wants to be remembered as a man who loved music.

Then, participants in the imagined contact condition read the following text:

We would like you to take five minutes to imagine the following scenario. Shortly after arriving at the train station, *Arturo Sandoval* catches his train and leaves. *Tom Harrell* takes the seat beside you. Imagine yourself having a conversation with *Tom Harrell* at the train station. Imagine that the interaction is positive, relaxed, and comfortable. We would like you to spend the time thinking, but please write down, from time to time, the things that you imagine. Feel free to write whatever springs to mind.

Participants in the control condition read the following text:

We would like you to take five minutes to imagine the following scenario. Shortly after arriving at the train station, *Tom Harrell* catches his train and

leaves. *Arturo Sandoval* takes the seat beside you. Imagine yourself having a conversation with *Arturo Sandoval* at the train station. Imagine that the interaction is positive, relaxed, and comfortable. We would like you to spend the time thinking, but please write down, from time to time, the things that you imagine. Feel free to write whatever springs to mind.

Participants wrote on a sheet of paper what they imagined for each scenario. Afterwards, the experimenter thanked the participants and made appointments with them for the second session. Approximately 15 days later, the participants returned to the laboratory to complete three measures used to assess prejudiced attitudes towards persons with schizophrenia.

Implicit prejudice

The first measure was an Implicit Association Test (IAT; Greenwald, McGhee, & Schwartz, 1998). We used a single IAT (Bluemke & Friese, 2008; Karpinski & Steinman, 2006) to measure the strength of participants' mental associations of the concept of schizophrenia with negative and positive words. Six words were negative (anger, war, agony, failure, grief, resentment), and six were positive (love, peace, enjoyment, happiness, glory, success). Four words were related to schizophrenia (paranoia, delirium, confused, and hallucination). We used the improved D600 algorithm to compute IAT scores (Greenwald, Nosek, & Banaji, 2003). Positive scores on this measure indicate more negative implicit attitudes towards schizophrenia (i.e., stronger negative associations with schizophrenia relative to positive associations).

Intergroup anxiety

The second measure was a 14-item scale of intergroup anxiety (West et al., 2011; adapted from Stephan & Stephan, 1985; Corrigan et al., 2002). Representative

items included the following: “If I were to meet a person with schizophrenia in the future, I think I would be relaxed” (reverse-coded) and “I would feel threatened by a person with schizophrenia.” For each item, participants indicated their response using a 7-point Likert scale (1 = not at all, 7 = yes, absolutely; Cronbach’s $\alpha = .80$).

Explicit prejudice

Explicit attitudes towards schizophrenia were measured with six questions (West et al., 2011; adapted from Wright, Aron, McLaughlin-Volpe, & Ropp, 1997).

Representative items included the following: “I think people with schizophrenia are warm” and “I think people with schizophrenia are positive” (1 = not at all, 7 = yes, absolutely; Cronbach’s $\alpha = .81$).

The three dependent variables were coded such that higher scores indicate higher levels of prejudice and anxiety. The two last measures were successfully used in previous studies on imagined contact and showed moderate to large effect sizes (West et al., 2011). In the present study, we also added an implicit measure of prejudice to allow our hypothesis to be tested on both explicit and implicit measures of prejudice.

At the end of the experiment, participants were thanked and fully debriefed.

Data analysis

Because the null hypothesis significance testing procedure creates a number of problems (e.g., questionable research practices, publication bias) and provides very little basis upon which we can draw conclusions about the hypotheses (Trafimow, 2003; Trafimow & Marks, 2015; Wasserstein & Lazar, 2016), we refrained from using this approach. Rather, we analyzed the data using a variety of descriptive statistics, effect size measures and effect size translations (Valentine, Aloe, & Lau, 2015). We complemented these analyses with robust descriptive statistics (Wilcox & Keselman,

2003) and Bayesian statistics (Wagenmakers, Morey, & Lee, 2016). Readers interested in the results of our significance tests (preregistered analyses) can refer to the OSF page for this project, as we do not report them in this article.

Results

We planned to include 150 participants. However, 153 students signed up for the study. All participants are included in the following analyses.

Testing of preregistered hypotheses

Implicit prejudice

The top panel of Figure 1 presents box and violin plots of the distribution of implicit prejudice split by condition. As shown in this figure, the distribution was quite similar in the two conditions, even though the median and upper quartiles (representing 75% of the observations) were somewhat lower in the imagined contact condition than in the control condition. In the control condition, the mean implicit prejudice was 0.16 ($SD = 0.40$). In the imagined contact condition, the mean was 0.10 ($SD = 0.42$). Thus, there was a very small mean difference in the expected direction.

Table 1 presents robust statistics: the median, trimmed mean (with a 20% trim proportion), Winsorized mean (with a 0.2 Winsorizing level), and M-estimator (with a Bending constant of 1.28). Contrary to the arithmetic mean, these statistics provide robust measures of central tendency, which are not sensitive to extreme observations and departure from normality. The robust descriptive statistics showed that the difference between the two conditions, although small, is very consistent on the different indices of central tendencies.

Table 2 presents the effect sizes and effect size translations of the effect of the conditions on implicit prejudice. As shown in this table, the mean effect size was small

(Cohen's $d = .13$, Glass's $\Delta = .12$). The condition accounted for less than 1% of the variance in implicit prejudice. In the imagined contact condition, 55% of the observations were lower than the mean implicit prejudice in the control condition. A Bayesian analysis was run to quantify the evidence in favor of the preregistered directional hypothesis that imagined contact reduced implicit prejudice compared to the control condition. We used a median effect size of 0.50 (Cohen's d) as a prior because it is close to the mean effect size found in the meta-analysis of Miles and Crisp (2013). Consistent with the results of the effect size translations, we found a Bayes factor (BF_{10}) of 0.48, indicating that the data were inconclusive. To sum up, the data showed that imagined contact has a very small effect on implicit prejudice. Although this effect is in the expected direction, the very small effect size does not sufficiently support our hypothesis.

Intergroup anxiety

Parallel analyses of intergroup anxiety were conducted. As shown in the middle panel of Figure 1, the box plot of intergroup anxiety was much lower in the imagined contact condition than in the control condition, even though the medians in the two conditions did not differ much. Indeed, the distribution of intergroup anxiety was less dispersed in the imagined contact condition than in the control condition. In the control condition, the mean intergroup anxiety was 3.48 ($SD = 1.03$). In the imagined contact condition, the mean was 3.13 ($SD = 0.80$). The mean difference was in the expected direction and comparable on most indices of robust descriptive statistics: the trimmed mean, Winsorized mean, and M-estimator (Table 1). As shown in Table 1, the effect size of the condition was small (Cohen's $d = 0.37$, Glass's $\Delta = 0.32$), but it was comparable to the mean effect size found in Miles and Crisp's (2013) meta-analysis and

substantially larger than that reported in Klein's et al. (2014) high-powered preregistered replication. The effect of the condition accounted for a significant part of the variance in intergroup anxiety (3.37%). In the imagined contact condition, 65% of the observations featured lower intergroup anxiety than the mean in the control condition. The results of a Bayesian analysis with a median effect size of 0.50 (Cohen's d) as a prior found substantial support for the hypothesis that imagined contact reduced intergroup anxiety compared to the control condition ($BF_{10} = 4.37$). This analysis showed that the preregistered directional hypothesis was about four times more likely to be true than the null hypothesis.

In sum, imagined contact had a small effect on intergroup anxiety. This effect was in the expected direction. The effect size was comparable to the mean effect size found in a previous meta-analysis of imagined contact (Miles & Crisp, 2013), even though intergroup anxiety was assessed about two weeks after the imagined contact intervention in the present study.

Explicit prejudice

As shown in the bottom panel of Figure 1, the median and lower quartile of the box plot of explicit prejudice were lower in the imagined contact condition than in the control condition. Participants in the imagined contact condition reported lower levels of prejudice towards persons with mental illness ($M = 3.68$, $SD = 1.09$) than those in the control condition ($M = 4.04$, $SD = .81$). The robust descriptive statistics (Table 1) indicated that the difference between the two conditions was very consistent in terms of the different measures of central tendency. The effect size was small (Cohen's $d = 0.38$, Glass's $\Delta = 0.43$) but comparable to the mean effect size found in a recent meta-analysis (Miles & Crisp, 2013) and much larger than that found in a high-powered

preregistered replication (Klein et al., 2014). The effect of the condition accounted for a significant part of the variance in explicit prejudice (3.53%). In the imagined contact condition, 59% of the observations indicated lower explicit prejudice than in the control condition. The results of a Bayesian analysis with a median effect size of 0.50 (Cohen's d) as a prior indicated substantial support for the hypothesis that imagined contact reduced intergroup anxiety compared to the control condition ($BF_{10} = 4.96$). The results were about five times more likely under the preregistered directional hypothesis than under the null hypothesis.

In sum, imagined contact had a small effect on explicit prejudice. This effect was in the expected direction, and the effect size was comparable to the mean effect size found in a previous meta-analysis on imagined contact (Miles & Crisp, 2013), even though explicit prejudice was assessed about two weeks after the imagined contact intervention in the present study.

Complementary (non-preregistered) analyses

In subsequent analyses, we tested the hypothesis that intergroup anxiety mediated the effect of the condition on explicit prejudice, as suggested in the literature on imagined contact (Swart et al., 2011; Vezzali et al., 2015). The data yielded support for this mediation model. However, we also found evidence for the reverse mediation model, which postulated that explicit prejudice mediated the effect of imagined contact on intergroup anxiety. Given the important criticisms regarding mediation analyses that have recently been raised in the literature (Bullock, Green, & Ha, 2010; Kline, 2015; Spencer, Zanna, & Fong, 2005; Tate, 2015; Thoemmes, 2015; Trafimow, 2015), it seems safe to conclude that the results from our mediation analyses were inconclusive.

Discussion

The aim of the present study was to test the hypothesis that a single session of imagined contact has long-lasting effects on prejudice towards stigmatized social groups. We assessed explicit and implicit prejudice towards persons with schizophrenia and intergroup anxiety two to three weeks after a single session of imagined contact. The hypotheses were preregistered. The descriptive statistics were generally consistent with our hypotheses; imagined contact improved explicit attitudes towards persons with schizophrenia, and it reduced anxiety regarding the prospect of meeting members of this stigmatized social group. The mean values for implicit prejudice were in the expected direction, but the effect size was descriptively smaller. Actually, the data suggested that imagined contact can have a small effect on implicit prejudice on the long term, but they were not really conclusive. In complementary (non-preregistered) analyses, we found very little evidence that intergroup anxiety mediated the effect of imagined contact on explicit prejudice. This finding was at odds with prior research on imagined contact.

To the best of our knowledge, the present work is the first preregistered study suggesting that the effects of imagined contact on prejudice can persist over time. The findings have important theoretical implications for the imagined contact hypothesis. Past research has focused mainly on immediate and short-term effects, which has led researchers to question how long the effects last (Brown & Paterson, 2016; Miles & Crisp, 2013). Very few studies have investigated the effects of imagined contact after a delay of several days, and the studies conducted on this issue did not control for the false discovery rate using preregistration (Falvo et al., 2014; Vezzali et al., 2015). Thus, the present study represents an important step in addressing this issue.

The present findings contradict the claim that brief prejudice interventions are

transient and short-lived (Lai et al., 2016). Rather, the findings are consistent with the idea that the effects of imagined contact can persist over several days (Falvo et al., 2014; Vezzali et al., 2015). Briefly imagining a positive interaction with a person with schizophrenia at the end of an unrelated experiment was sufficient to lower explicit prejudice towards members of this stigmatized social group 15 days after the experiment. The effect of imagined contact was similar according to both explicit and implicit measures of prejudice, although it was descriptively more pronounced on explicit prejudice than on implicit prejudice.

The present study has some limitations that deserve to be addressed in future studies. In particular, the effect of imagined contact on implicit prejudice was very small in the current study. A Bayesian analysis indicated that our data yielded no more evidence supporting a positive effect than a null effect. The present study is the first to examine the effect of a single session of imagined contact on implicit prejudice after a delay. Further preregistered studies using larger samples are needed to gauge the true effect size of the effect we are investigating. Previous studies have shown that a multi-session intervention based on imagined contact can reduce implicit prejudice among children after a delay of one week (Vezzali et al., 2012). Thus, it is possible that a single session of imagined contact has a very small but still reliable effect on implicit prejudice after a delay of two weeks. A larger sample might provide further evidence supporting this effect.

Another limitation of our findings is that we were not able to completely rule out that the reported effects are due to demand effects. This is a limitation of not only the present study but also the body of literature on the imagined contact hypothesis. The effects of imagined contact could be explained by demand characteristics when

participants guess the hypothesis and tend to confirm it. To rule out this explanation, researchers have mostly relied on implicit and physiological measures, which are difficult to control (West & Turner, 2014; West et al., 2015). For example, a subliminal priming paradigm was used to determine that imagined contact affects implicit bias (Schuhl, Lambert, & Chatard, 2018). Because participants are not aware of what is measured by this task, they cannot control their response. More generally, research indicates that the effects of imagined contact extend to a variety of implicit measures of prejudice (Miles & Crisp, 2013). Thus, considering the available evidence, there are few reasons to believe that the present findings may not be generalized to other indicators of prejudiced attitudes and behaviors.

As suggested by an anonymous reviewer, another limitation is that our experimental setup was not optimal since there were some (relatively small) differences between the control and imagined contact conditions. In particular, the participants may have inferred a difference in the nationality/ethnicity of the target person (Tom Harrell or Arturo Sandoval) in the two experimental conditions as the names denoted different cultural origins (English and Spanish). However, we did not see this as problematic because France is a very multicultural and inclusive society and many French nationals, including famous French jazz artists (e.g., Laurent de Wilde, Ibrahim Maalouf, Enzo Carniel), do not have typically French names. Thus, we considered the target names to be credible to our participants. Moreover, the target names were taken from a previous study (West et al., 2011) that we attempted to replicate with the addition of a delay, and we were reluctant to change the original materials. Further studies could try to control the presumed nationality of the two target names to more fully address this issue.

In spite of its limitations, the present study has some important practical

implications for the design of effective diversity programs and prejudice-reduction interventions. In the near future, an empirically based intervention like the one tested in the present study could be implemented in companies to promote diversity and inclusion practices in the workplace, boost creativity, and achieve a competitive business advantage (Antonio et al., 2004; Butera, Mugny, Legrenzi, & Pérez, 1996; Konan, Chatard, Selimbegović, & Mugny, 2010; McLeod, Lobel, & Cox, 1996; Moreland, Levine, & Wingert, 1996; Sommers, Warp, & Mahoney, 2008). At the time we revised this article, we were at the midpoint of data collection for a large-scale field study conducted on one of the largest energy supplier companies in France. The aim of this field study, informed by the findings of the present lab experiment, is to test the long-term effectiveness of a brief (single session) personalized imagined-contact intervention to reduce the stigmatization of employees suffering from a mental illness. This and other field studies are both important and necessary to test the long-term effects of imagined contact interventions outside the laboratory.

An important question, from an applied perspective, is whether a single session of imagined contact is worth implementing in natural settings given its small effect size in the laboratory. It seems obvious that the effect size would not be larger in less controlled environments, such as natural settings. On this basis, some may argue that the present intervention should not be used in applied settings, as it would fail to reduce prejudice on the long term. Should this intervention be used or not in natural environments? This is an important question and clearly we do not have a definitive answer. We believe this issue should be investigated scientifically. Although we have no data to back up the claim that imagined contact can have societally important effects in natural settings, we think the present study provides important insights that may

contribute to inform researchers and decision makers.

First, it is important to realize that the notion of effect size is relative rather than absolute. The present effects were small, according to Cohen's (1988) classification of small, medium, and large effect sizes, but comparable to the mean effect size found in social psychology (Cohen's $d = 0.42$, cf. Richard, Bond, & Stokes-Zoota, 2003). Thus, the effects found in the present study are not particularly small in terms of effect size. Indeed, they are best characterized as average effects when compared to other effects typically investigated in the social psychological literature. Because the present effects were observed after a delay of several days, rather than immediately after the intervention, and caused by a 5-minute intervention, they appear to be more pronounced than the typical short-term effects studied in social psychology. This suggests that the current effects are not trivial and that they may have some observable effects in natural environments.

Second, statisticians and social scientists have long argued that small effects in the laboratory can actually have societally important effects in real life (Abelson, 1985; Greenwald, Banaji, & Nosek, 2015; Messik, 1995; Rosenthal & Rubin, 1982). This is commonly referred to as "the variance explanation paradox" (Abelson, 1985). Rosenthal and Rubin (1982) have showed that a small effect in terms of variance (say, an effect accounting for 3.5% of variance, as in the present study) corresponds to a success rate increased of about 20% from the experimental condition to the control condition. In the present study, the 3.5% of variance in explicit prejudice accounted for by imagined contact might thus be equivalent to reducing explicit prejudice rate from 60% to 40%. This measure of effect size (the binomial effect size display, see Rosenthal & Rubin, 1982) along with other measures of effect size we reported in Table 2 (the

Cohen's U3 and Descriptive U3), suggest that our effects are not trivial and that they can have societally important effects.

According to Messick (1995), small effects could have societally important effects if they apply to many people or if they apply repeatedly to the same person (see also Greenwald et al., 2015). One of the major strengths of the imagined contact intervention is its simplicity and brevity (Turner, Crisp, & Lambert, 2007). It can be used in many different societal contexts, even when people have little opportunity for real contacts. It can also be delivered online to target a potentially large audience. Therefore, the present intervention may satisfy Messick's (1995) two conditions for a societally important effect. Decision makers and companies may be reluctant to use multi-session prejudice reduction programs because these programs may be difficult to implement and time-consuming. In contrast, the present single-session intervention offers an interesting tool to briefly reduce prejudice on the long term. The intervention may be repeated over time, ensuring a cumulative effect. To avoid lassitude, the intervention may also be associated with a short video as prior studies suggest that facilitating elaboration during imagination enhances the imagined contact effect (Husnu & Crisp, 2010). Because the present intervention can be easily implemented online, it may also target a lot of persons. Thus, there are reasons to believe that this ready-to-be-used intervention may be useful for reducing prejudice on the long term in applied settings. Future studies are needed to examine this issue.

In conclusion, the present findings are important because they add to the extant literature, showing that a single session of imagined contact is sufficient to attenuate intergroup anxiety and reduce explicit prejudice over time. Highlighting the power of imagination, the present findings reveal that a single brief session of imagined contact

has a more persistent effect on prejudice than previously thought. This study and others suggest that imagined contact is one of the most promising interventions for reducing prejudice given its simplicity, brevity, and long-lasting effects.

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Footnotes

¹ A series of studies have shown that a three-week intervention involving imagined contact could reduce explicit and implicit prejudice among children after a delay of approximately one week (Stathi, Cameron, Hartley, & Bradford, 2014; Vezzali, Capozza, Stathi, & Giovannini, 2012; Vezzali, Capozza, Giovannini, & Stathi, 2012). Although fascinating, these studies depart from the typical imagined contact study in that they target children and use a multi-session intervention. In the present preregistered study, we were interested in testing whether the single-session intervention that is commonly used in the literature on imagined contact could affect adults' explicit and implicit prejudice after a delay. For that reason, we did not include these studies in our literature review.

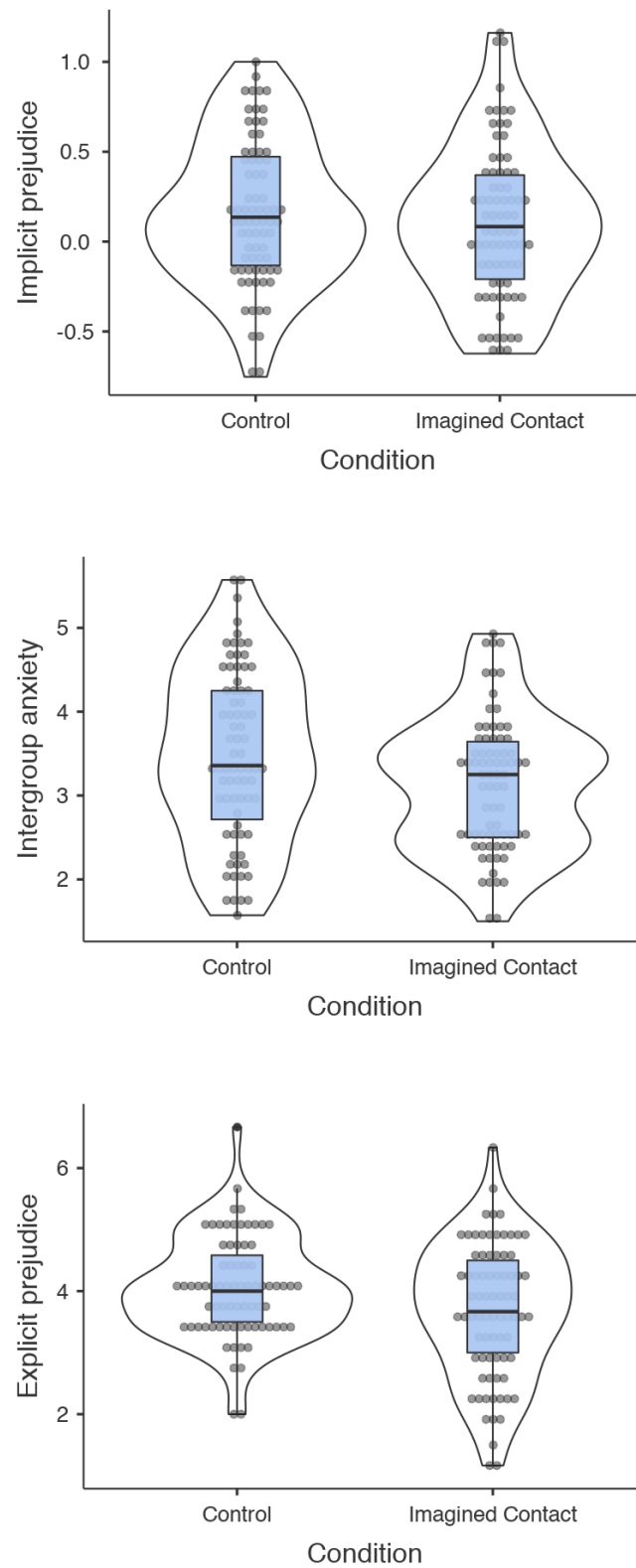


Figure 1. Box and violin plots for each condition.

Table 1

Robust Descriptive Statistics

		Control		Imagined contact	
		Robust statistics	SE	Robust statistics	SE
Implicit prejudice	Trimmed mean	0.1427	0.0498	0.0877	0.0535
	Winsorized mean	0.1531	0.0502	0.0886	0.0531
	M-estimator	0.1593	0.0514	0.0930	0.0536
	Median	0.1359	0.0529	0.0835	0.0529
Intergroup anxiety	Trimmed mean	3.4952	0.1478	3.1040	0.1008
	Winsorized mean	3.4971	0.1491	3.0912	0.1008
	M-estimator	3.4738	0.1272	3.1103	0.0935
	Median	3.3571	0.1664	3.2500	0.1664
Explicit prejudice	Trimmed mean	3.9852	0.1094	3.7482	0.1446
	Winsorized mean	4.0244	0.1104	3.7165	0.1434
	M-estimator	4.0275	0.1031	3.6974	0.1284
	Median	4.0000	0.0971	3.6667	0.1618

Table 2

Effect Sizes and Effect Size Translations

Statistics	Interpretation of statistics	Implicit prejudice	Intergroup anxiety	Explicit prejudice
ES_{um}	Unstandardized mean difference between the contact and control groups	+0.05	+0.34	+0.36
d	Mean difference between the contact and control groups in terms of standard deviation	+0.13	+0.37	+0.38
Glass's Δ	Mean difference divided by the standard deviation in the control condition	+0.12	+0.32	+0.43
r_{pb}	Association between the assignment condition and dependent variable	-.06	-.18	-.18
r^2	Proportion of variance in the dependent variable explained by the experimental manipulation	0.42%	3.37%	3.53%
Descriptive U_3	Proportion of the contact group under the mean of the control group	55.84%	65.78%	59.74%
Cohen's U_3	Proportion of the contact group under the mean of the control group, assuming normal distribution	55.17%	64.43%	64.80%
Common Language Effect Size	Probability that a randomly selected member of the contact group will score below a randomly selected member of the control group	0.53	0.61	0.61