The week after

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The week after: Do the effects of imagined contact last over time?

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Abstract

The vast majority of studies assessing the prejudice reduction properties of imagined contact have focused so far on the immediate effects of the intervention. In an attempt to contribute to the literature examining the long-term effects of imagined contact, the two studies reported in this paper tested the immediate and long-term effects of imagined contact on outgroup attitudes, intergroup anxiety, and behavioral intentions in Experiment 1, and also on contact self-efficacy in Experiment 2. Both studies were conducted in a context of entrenched intergroup conflict, Cyprus. The results supported the effectiveness of imagined contact in eliciting more positive attitudes, lower levels of anxiety, more positive behavioral intentions, and higher contact self-efficacy when these were measured immediately after contact. However, evidence for the endurance of these effects was systematically found only for outgroup attitudes and intergroup anxiety. While these results speak to the ability of imagined contact to lead to long-term changes in important and commonly studied intergroup outcomes, lack of consistent evidence regarding its ability to yield lasting changes on variables pertaining to intended behavior toward the outgroup compose a challenge for the intervention.

1 | INTRODUCTION

Intergroup contact, defined as a meaningful and positive interaction between members of different groups, can, as argued by Allport (1954), reduce prejudice toward the outgroup (see Pettigrew & Tropp, 2006, for a meta-analysis). Since then, a large number of studies have been carried out to explore whether, when, and how intergroup contact works to reduce prejudice. These studies have produced an important knowledge-base which forms one of social psychology's most important contributions to conflict resolution and to conflict prevention to date (see Al Ramiah & Hewstone, 2013; Hewstone, 2009).

Despite the "promise of contact," direct (face-to-face) contact is often not feasible in real world. The absence of direct intergroup contact might be a result of lack of opportunities for contact as is the case in geographically segregated settings such as segregated schools (e.g., Hewstone et al., 2018; Hughes, Campbell, Lolliot, Hewstone, & Gallagher, 2013) and segregated neighborhoods (e.g.,

Schmid, Hewstone, Hughes, Jenkins, & Cairns, 2009). Even when contact is (made) feasible, psychological obstacles like anxiety about meeting, or being around, members of the outgroup (Stephan & Stephan, 1985) and not having confidence in one's ability to successfully carry out an intergroup interaction (contact self-efficacy) (see Mazziotta, Mummendey, & Wright, 2011), may get into the way of pursuing and having intergroup contact.

Imagined contact, defined as the mental simulation of a positive intergroup encounter (Crisp & Turner, 2009) was proposed as a way of addressing the lack of direct contact challenge by catering for both the physical and the psychological constraints to contact. Merely simulating a positive interaction with a member of the outgroup does not rely on opportunities of meeting the "other" and it is psychologically safe by comparison to the potentially intimidating face-to-face contact.

While there is growing evidence demonstrating the possibility of imagined contact to yield prejudice reduction effects akin to the ones promoted by direct contact such as changes in attitudes

and behavioral intentions toward the outgroup and changes in intergroup emotions (see Miles & Crisp, 2014, for a meta-analysis of imagined contact studies), what is yet to be established is whether these effects endure in time.

The studies presented in this paper aim at testing the endurance of imagined contact effects and are in this way complementing the small number of existing studies that have already undertaken this task. They do so in a context of entrenched conflict, Cyprus, where opportunities for face-to-face contact are scarce due to long standing geographical segregation of the two adversary groups, Greek and Turkish Cypriots. Given the scarcity of direct contact in settings like this one, indirect forms of contact, such as imagined contact, that do not necessitate a face-to-face encounter with a member of the outgroup constitute, often times, the only way of connecting the two groups. In the remaining of the introduction I explicate why it is important for imagined contact research to pay more attention to establishing the endurance of imagined contact's effects and against this backdrop I present in further detail the scope of the research study reported in this paper and the hypotheses it sought to test.

2 | LONGITUDINAL TESTING IN IMAGINED CONTACT RESEARCH

Evidence for the prejudice reduction effects of imagined contact was first drawn out of Turner, Crisp, and Lambert (2007), who showed that a 1-min imagery of a positive intergroup encounter led young participants to report less intergroup bias toward the elderly (Experiments 1 and 2) and heterosexual participants to evaluate homosexuals more positively (Experiment 3) than participants in the respective control conditions. This first test of imagined contact gave way to multiple other studies which led Crisp and Turner (2009) to formally propose their imagined contact hypothesis according to which "simply imagining outgroup interactions can produce more positive perceptions of outgroups" (p. 231).

Imagined contact has received a fair amount of criticism since its inception. Critiques revolved around task demand characteristics (Bigler & Hughes, 2010), lack of meaningful real-world implications and more specifically the unlikeliness of deploying imagined contact in settings of full-blown intergroup conflict (Lee & Jussim, 2010), and weak replicability of its effects (Klein et al., 2014). Researchers studying imagined contact have responded to these critiques by: (a) carefully choosing control groups when putting imagined contact to test (see Crisp & Turner, 2012, 2013); (b) demonstrating imagined contact's effectiveness in promoting more positive intergroup attitudes and behavioral intentions in contexts of entrenched conflict like Cyprus (Husnu & Crisp, 2010a; Ioannou, Hewstone, & Al Ramiah, 2017) and the Turkish-Kurdish interethnic setting (Bagci, Piyale, Bircek, & Ebcim, 2017); (c) securing a small but significant effect of imagined contact on multiple intergroup measures like implicit and explicit attitudes, behavioral intentions, and emotions in a meta-analysis of over 70 studies from many different countries (Miles & Crisp, 2014).

Despite these noteworthy developments in the field of imagined contact, a topic that remains under-explored is the test of the duration of imagined contact's effects. The vast majority of studies on imagined contact merely test participants' attitudes, emotions, and behavioral intentions upon completion of imagery but almost never include a delayed measurement of the same variables to test whether changes on these variables last in time. The scarcity of studies on the long-term effects of imagined contact has also been identified by Miles and Crisp (2014) who, in their report of the meta-analysis of imagined contact studies, proposed that further research is directed toward examining the duration of imagined contact's effects.

Why is it important to include long-term measurements when putting imagined contact to the test? Various reasons exist that justify the need for such studies, which are discussed next. First, as is the case with any intervention, enduring effects speak to the capacity of the intervention to yield strong enough changes that do not fade away once the intervention is foregone. According to the persuasion and attitude-change literature, the stronger the attitudes formed after the persuasive message the more resistant they are to decay caused by time as well as to counter-persuasion messages (Eagly & Chaiken, 1995; Krosnick & Petty, 1995). If researchers were to regard (imagined) contact interventions as instances of persuasion then attitudes generated by the intervention would only be resistant to time and to attempts for counter-persuasion (e.g., friends not condoning contact, negative intergroup experiences succeeding the intervention), only if they are strong enough.

Second, given the criticism imagined contact has received that it is conducive to priming effects (i.e., participants exhibiting a more positive overall behavior as an effect of being primed with positivity), a delayed measurement could serve as an additional way (other than having a control group that controls for priming positive emotions) of ensuring that the effects generated by imagined contact interventions are not a result of priming. A delayed measurement of the outcome variables, as opposed to an immediate one, is less likely to be affected by priming given the temporary nature of priming effects (Higgins, 1996).

Third, imagined contact is widely accepted as a gateway to actual contact (Crisp & Turner, 2013), that is a way to prepare people for a future intergroup interaction, for example, by reducing their anxiety for contact, increasing their perceived capacity to deal with future instances of contact (contact self-efficacy) and by eliciting a greater willingness to approach the outgroup (positive behavioral intentions). For this preparatory function to be plausible, imagined contact should lead to a sustained change, for example, on intergroup anxiety, contact self-efficacy, and behavioral intentions so as for reduced anxiety, heightened self-efficacy, and positive intentions to still be in effect when the opportunity for direct contact arises.

There is, by now, a small number of studies that sought to assess the endurance of imagined contact's effects. Falvo, Capozza, Hichy, and Di Sipio (2014) deployed a longitudinal experimental design via which they tested the effects of imagining contact with an intellectually disabled individual on humanization (i.e., attributing more human than non-human emotions to individuals with intellectual disability) upon completion of imagery (t1) and a month later (t2).

They found that in both t1 and in t2 humanization was higher for the imagined contact condition as opposed to a control condition where participants imagined a positive scene.

The long-term effects of imagined contact were also demonstrated among Italian children who mentally simulated contact with immigrant children. Vezzali, Capozza, Stathi, and Giovannini (2012) asked children to imagine a positive interaction with an immigrant child over 3 weeks (different child and different setting every week). They tested the effects of this intervention against a control group that did not engage in any mental simulation task. They found that a week after the 3 week intervention, imagined contact participants reported more trust, more humanization, and more positive behavioral intentions toward immigrants. Similarly, in the same context, Vezzali, Stathi, Crisp, Giovannini, Capozza, and Gaertner (2015) asked elementary school children to imagine, over 4 weeks, working together with immigrant children and competing as part of a common team. Imagery improved helping intentions toward immigrants when compared with a control condition in which children imagined intragroup contact. The difference between the two conditions was registered 1 week after the last intervention session and also a week after that, when measurements on this variable were repeated. Finally, Vezzali, Stathi, Crisp, and Capozza (2015) staged another 3 week intervention using an orthogonal design: direct contact (intergroup vs. intragroup); imagined contact (intergroup vs. intragroup). This provided them with the opportunity to test the additive and interactive effects of direct and imagined contact on negative stereotypes and helping behaviors of Italian children toward immigrant children. The results of their study showed that both direct and imagined intergroup contact led to less negative stereotypes and stronger helping intentions recorded a week after the interventions.

The results of the aforementioned studies provide strong evidence for the longitudinal effects of imagined contact as they demonstrate that the positive intergroup effects of the intervention were retained or traced between a week and a month after the completion of the intervention. This evidence, however, is drawn from studies conducted in a single geographical context (Italy) that primarily used children as participants which is the age-group for which imagined contact, as shown by the meta-analysis of Miles and Crisp (2014), has so far produced the most pronounced effects. This raises the question whether the lasting effects of imagined contact can be observed in other contexts, especially contexts of entrenched conflict and segregation where imagined contact is needed the most and with older participants. This is precisely the question the research presented in this paper sought to answer.

3 | PRESENT STUDY

The present study set out to investigate the endurance of imagined contact's effects for Greek Cypriots in the context of the Greek Cypriot-Turkish Cypriot relations in Cyprus. Cyprus is a post-conflict society characterized by extreme levels of segregation due to a de-facto partition of the island into north and south following a Greek

military-led coup d'état succeeded by military intervention from Turkey in 1974. The two main communities, Greek Cypriots (majority: 77%) and Turkish Cypriots (minority: 18%) have been living in the south and north part of the island respectively ever since. This has restricted both opportunities for, as well as actual, intergroup contact between them which resulted in low overall levels of contact between the two communities till present times (Ioannou, Jarraud, & Louise, 2015). Such a low-contact and high-conflict setting offers itself for testing indirect forms of contact such as imagined contact. Previous studies of imagined contact conducted in Cyprus reported positive effects of imagined contact when compared to control conditions, on positive behavioral intentions toward Greek Cypriots, among Turkish Cypriots (Husnu & Crisp, 2010a) and lower intergroup anxiety, higher contact self-efficacy, and by extension more positive action tendencies toward Turkish Cypriots, among Greek Cypriots (Ioannou, Al Ramiah, & Hewstone, 2017). Neither of these studies, however, included a delayed measurement to check how resistant to time these positive effects were.

The two studies reported in this paper served as an additional test of the ability of imagined contact to produce positive intergroup outcomes among Greek Cypriots, but they also aimed at investigating whether these effects remained a week after imagery so as to test for the endurance of imagined contact's effects in this context. The outcome variables chosen for these studies represented three out of the categories of dependent variables identified by Miles and Crisp (2014): (a) *attitudes toward the outgroup*; by measuring attitudes using a feeling thermometer; (b) *emotions toward the outgroup*; by measuring intergroup anxiety; and, (c) *intended behavior toward the outgroup* by measuring behavioral intentions (willingness to pursue contact) as well as contact self-efficacy in the second experiment. The category not represented in the studies reported in this paper was *actual behavior toward the outgroup* such as the amount of positive direct contact following the intervention. This latter category of outcome variables was considered to be less relevant to the context where the studies were conducted given the very low chances of having regular actual intergroup contact in Cyprus.

4 | EXPERIMENTAL DESIGN

The design deployed for the purpose of this research was a pretest-intervention-posttest-delayed posttest experimental design with a control condition. The time-lag between measurement points was 1 week and was kept constant across participants of both conditions. In practice this meant that dependent variables were measured at three points in time, the first measurement took place 1 week before the intervention, the second immediately after the intervention, and the third measurement a week after the intervention. The 1 week lag was thought to be long enough for participants to not be influenced by their scores on prior measurements, and it also allowed time for participants to reconsider their initial reactions to the intervention between post and delayed posttest measurements. The choice to deploy a 1 week gap between intervention and delayed measurement of the dependent

variables was also influenced by the practice of prior studies testing the long-term effects of the imagined contact intervention (e.g., Vezzali, Stathi et al., 2015) the majority of which utilized the same time-lag.

5 | EXPERIMENT 1

The first experiment aimed to investigate whether imagined contact by comparison to a no-contact/no-imagery control group could lead to more positive attitudes toward Turkish Cypriots as well as less intergroup anxiety and more positive behavioral intentions toward the same group. It also aimed to assess whether changes registered immediately after the imagined contact intervention would be traceable a week after contact.

5.1 | Hypotheses

The main hypotheses of the study were the following:

> **Hypothesis 1:** The imagined contact would give way to more positive attitudes, less intergroup anxiety and more positive behavioral intentions at posttest by comparison to pretest and also by comparison to the control condition.
>
> **Hypothesis 2:** These differences would be detected at delayed posttest too.

5.2 | Participants

The sample of the study consisted of 46 individuals: 23 in the imagined contact condition and 23 in the control condition. Not all participants sent in their delayed posttest questionnaire, however, which reduced sample size by three in the imagined contact condition ($N_{\mathrm{imaginedcontact}}$ = 20) and by six in the control condition ($N_{\mathrm{controlcondition}}$ = 17). All of the participants were female due to the fact that participants were recruited from the Faculty of Social Sciences and Education of a public university which at the time recruitment took place, was almost entirely populated by females.

5.3 | Procedure

The recruitment of the participants was done through announcements before the commencement of class in a number of different courses. The researcher invited students to participate in a study on "human relations." Potential participants were informed that the study was comprised of three phases, that for the first and the third phase they would have to fill in an online questionnaire and that for the second phase half of them would complete a very short experiment and the other half would complete a task online. They were allowed to decide which of the two options they preferred: the experiment in the lab or the online task, which meant that there was no random allocation of the participants to the two conditions.

Participants were rewarded with a place in a raffle that would give four individuals, two in the control group and two in the imagined contact group, the prize of 30 euro. This was used as a means for providing an incentive for students to participate.

5.4 | Method

Pretest measurements were registered via an online questionnaire sent electronically to the participants 1 week before the experiment/ online task. The purpose of the pretest was to register baseline scores on the dependent variables and to also register prior levels of direct contact. A week after completing the pretest, the participants completed the imagery or the online task. Immediately after the experiment/online task, the participants completed the posttest questionnaire which served as a second measurement of the dependent variables and a week later, the participants were sent one more questionnaire which served as the delayed posttest. The participants were not provided with any information about the study throughout its duration but they were told that they would be debriefed upon completion of the last phase of the study (i.e., upon completing the delayed posttest). In those cases in which participants failed to complete the questionnaire on the day they had to, they were sent a reminder to complete it the day after. In nearly all cases the 1 week lag between measurements was achieved. Participants were electronically debriefed upon completion of the study.

5.5 | Experimental manipulations

The participants who had selected the "online task" automatically entered the control condition. The participants in the control condition did not in fact have to complete any real task. The "online task" was merely the posttest questionnaire and it was sent to them a week after having completed the pretest. The participants who had selected the task at the lab, that is, the imagined contact participants, were requested to come to the lab a week after having had completed the pretest. They were given a minute to imagine a positive contact scenario. The scenario was in line with the elaborated imagined contact scenario proposed by Husnu and Crisp (2010b) and which read as follows:

> I will give you one minute to close your eyes and imagine yourself sitting at a café in Ledras street [a popular street in the capital city of the country, Nicosia, in which the study took place, and which is situated right next to one of the crossing points] one Thursday afternoon. There you meet a Turkish Cypriot girl for the first time and you engage in a conversation. The conversation is very positive and throughout it you find out many interesting and unexpected things about her.

Upon completion of the minute-long imagery the participants completed the posttest. A question preceded the actual posttest

questionnaire requesting the participants to describe in as much detail as they wished the scene they had imagined. This was done for the purpose of strengthening the imagined contact intervention (see Crisp, Stathi, Turner, & Husnu, 2008).

5.6 | Measurements

To avoid the risk of task demand characteristics entailed in having multiple measurements of the same constructs, measurements of the main dependent variables were dispersed amidst other measurements which were not directly related to the study's goals: personality trait measurements, measurements of identification with ethnic and national identity, and measurements of attitudes toward other outgroups such as economic immigrants, asylum seekers, and gays and lesbians. The measurements of the main variables are presented next.

5.6.1 | Attitudes toward Turkish Cypriots

The participants rated their feelings toward Turkish Cypriots on a feeling thermometer (Converse & Presser, 1986) ranging from 1, *0 degrees* (very cold feelings) to 11, *100 degrees* (very warm feelings).

5.6.2 | Prior direct contact

One item asked participants to rate how much contact they had in the past with Turkish Cypriots on a scale ranging from 1, *no contact at all* to 5, *a lot of contact*.

5.6.3 | Intergroup anxiety

Was measured with an adapted six-item version of Stephan and Stephan's (1985) original intergroup anxiety measure asking participants to rate on a 5-point scale (1, *not at all*; 5, *extremely*), to what extent they would feel: "*threatened*," "*anxious*," "*awkward*," "*comfortable*," "*safe*," and "*at ease*" (the last three items were reverse-coded), if they were the only Greek Cypriot in a group of Turkish Cypriots. This measure was reliable over the three time measurements ($\alpha_{pretest}$ = 0.70, $\alpha_{posttest}$ = 0.84, $\alpha_{delayed\ posttest}$ = 0.62).

5.6.4 | Behavioral intentions

Participants were asked to report to what extent they would be willing to: (a) have a Turkish Cypriot friend; (b) have a Turkish Cypriot as a family member; (c) meet more Turkish Cypriots; and, (d) visit Turkish Cypriots, on a 5-point scale ranging from 1, *not willing at all* to 5, *very willing* ($\alpha_{pretest}$ = 0.93, $\alpha_{posttest}$ = 0.87, $\alpha_{delayed\ posttest}$ = 0.88).

5.7 | Results and discussion

As participants were not randomly allocated to the two conditions it was first tested whether participants in the two conditions differed significantly in their pretest scores on the main dependent variables

TABLE 1 Means and standard deviations for pre, post, and delayed posttest outgroup attitudes, intergroup anxiety, and behavioral intentions, and means and standard deviations for prior direct contact at pretest (Experiment 1)

		Imagined contact (N = 20)	Control condition (N = 17)
		M (SD)	M (SD)
Pretest	Outgroup attitudes	5.00 (1.31)	4.25 (2.27)
	Intergroup anxiety	3.07 (0.61)	2.69 (0.69)
	Behavioral intentions	2.55 (0.90)	2.49 (1.28)
	Prior direct contact	1.57 (0.84)	1.87 (1.04)
Posttest	Outgroup attitudes	5.35 (0.98)	4.33 (2.18)
	Intergroup anxiety	2.63 (0.71)	2.94 (0.83)
	Behavioral intentions	2.88 (0.67)	2.40 (1.10)
Delayed posttest	Outgroup attitudes	5.30 (1.84)	3.94 (2.66)
	Intergroup anxiety	2.75 (0.54)	2.79 (0.64)
	Behavioral intentions	2.54 (0.90)	

Note: Means are not adjusted for pretest levels of anxiety.

as well as their prior levels of direct contact. For this reason, a multivariate Analysis of Variance (MANOVA) using Pillai's trace, with Condition (imagined contact vs. control condition) as the independent variable and pretest outgroup attitudes, intergroup anxiety, behavioral intentions, and prior direct contact as the dependent variables, was conducted. The analysis yielded a significant multivariate effect, V = 0.99, F (4, 32) = 2.90, p = 0.038, partial η^2 = 0.27 and univariate tests showed that the difference between the two conditions was caused by differences in intergroup anxiety scores (F (1, 35) = 6.80, p = 0.013, partial η^2 = 0.16): control group participants reported significantly lower intergroup anxiety at pretest than imagined contact participants. Because of this pretest difference in the two groups, intergroup anxiety was inserted as covariate in subsequent analyses.

To test the hypotheses of this study, three mixed ANOVAs were conducted, one for each dependent variable. Time of measurement (pretest vs. posttest vs. delayed posttest) was the within-subjects variable, Condition (imagined contact vs. control condition) the between-subjects variable and pretest intergroup anxiety the covariate.[1] Table 1 lists the means and standard deviations of pretest, posttest, and delayed posttest scores of outgroup attitudes, intergroup anxiety, and behavioral intentions for each condition. The means for prior contact, also reported in Table 1, suggest that participants in both conditions had limited contact (i.e., between no contact at all to very little contact) with Turkish Cypriots.

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[1]The only analysis (mixed ANOVA) for which pretest intergroup anxiety was not inserted as a covariate was the mixed ANOVA for intergroup anxiety. In this case, pretest levels of intergroup anxiety were included in the analysis as one level of the Time of measurement variable.

5.8 | Outgroup attitudes

The mixed ANOVA yielded no main effect of Time of measurement, $F(2, 68) = 0.31$, $p = 0.74$, partial $\eta^2 = 0.01$, or an interaction effect, $F(2, 68) = 1.09$, $p = 0.34$, partial $\eta^2 = 0.003$, but there was a significant main effect of Condition $F(1, 34) = 9.78$, $p = 0.004$, partial $\eta^2 = 0.22$. A look into the pairwise comparisons for the differences of the two conditions over time indicated that while at pretest outgroup attitudes between the two conditions were not significantly different (mean difference = 1.27, SE = 0.67, $p = 0.07$, CI: [−0.09, 2.64]), outgroup attitude scores became significantly different at posttest (mean difference = 1.57, SE = 0.62, $p = 0.02$, CI: [0.31, 2.83]) and at delayed posttest (mean difference = 2.27, SE = 0.73, $p = 0.004$, CI: [0.79, 3.74]), with participants in the imagined contact condition reporting significantly more positive attitudes toward Turkish Cypriots than the control condition participants. There were no differences in attitudes across time within each condition: attitudes did not become more positive over time for neither imagined contact nor the control group. Yet, the results indicate that attitudes reported by participants of the imagined contact group became significantly more positive than the attitudes reported by the control group participants at posttest and this difference remained significant at delayed posttest. This finding provides support to both Hypotheses 1 and 2, at least as far as post and delayed posttest differences between imagined contact and control group are concerned, and thus to the ability of imagined contact to lead to more positive attitudes that can remain positive a week after contact.

5.9 | Intergroup anxiety

For intergroup anxiety, the mixed ANOVA yielded no main effects of Time of measurement $F(2, 70) = 0.12$, $p = 0.89$, partial $\eta^2 = 0.003$, or Condition $F(1, 35) = 0.02$, $p = 0.90$, partial $\eta^2 = 0.00$, but a significant interaction effect, $F(2, 70) = 13.35$, $p = 0.00$, partial $\eta^2 = 0.28$. Pairwise comparisons showed that while at pretest intergroup anxiety was lower in the control condition (mean difference = 0.52, SE = 0.20, $p = 0.013$, CI: [0.12, 0.92]), this difference was no longer present at posttest (mean difference = 0.40, SE = 0.24, $p = 0.10$, CI: [−0.89, 0.08]), or at delayed posttest (mean difference = 0.04, SE = 0.19, $p = 0.82$, CI: [−0.44, 0.35]). Pairwise comparisons between times of measurement within each condition showed that intergroup anxiety levels reported by imagined contact participants significantly dropped at posttest by comparison to pretest (mean difference = 0.50, SE = 0.12, $p = 0.001$, CI: [0.20, 0.80]) and remained lower than pretest a week after contact (mean difference = 0.32, SE = 0.12, $p = 0.03$, CI: [0.02, 0.61]). On the contrary, anxiety levels for the control condition increased at posttest (mean difference = −0.42, SE = 0.13, $p = 0.008$, CI: [−0.75, −0.095]) by comparison to pretest while at delayed posttest they were not significantly different to pretest scores (mean difference = −0.25, SE = 0.13, $p = 0.19$, CI: [−0.57, 0.08]). These results combined provide support for Hypotheses 1 and 2 according to which imagined contact would bring about a lasting reduction of intergroup anxiety levels.

5.10 | Behavioral intentions

The analysis for behavioral intentions yielded no effects for Time of measurement $F(2, 68) = 0.36$, $p = 0.70$, partial $\eta^2 = 0.01$, or an interaction effect, $F(2, 68) = 1.10$, $p = 0.34$, partial $\eta^2 = 0.003$, but there was a significant main effect of Condition $F(1, 34) = 5.33$, $p = 0.03$, partial $\eta^2 = 0.14$. Similarly with outgroup attitudes, pairwise comparisons showed that while at pretest behavioral intentions were not different in the two conditions (mean difference = 0.41, SE = 0.31, $p = 0.19$, CI: [−0.22, 1.04]), they became significantly different at posttest (mean difference = 0.70, SE = 0.22, $p = 0.003$, CI: [0.26, 1.15]) and delayed posttest (mean difference = 0.53, SE = 0.25, $p = 0.045$, CI: [0.01, 1.04]), with behavioral intentions being more positive in the imagined contact condition at both time points. The results showed also that there were no significant differences across time for either of the conditions apart from a marginal pre to posttest difference for imagined contact (mean difference = −0.28, SE = 0.12, $p = 0.089$, CI: [−0.60, 0.03]). Despite the fact that behavioral intentions did not change across time for the imagined contact condition as was expected, imagined contact yielding more positive behavioral intentions at post and delayed posttest by comparison to the control group gives partial support to Hypotheses 1 and 2 and hence it speaks to the effectiveness of imagined contact in eliciting long-term and positive changes on behavioral intentions toward the outgroup.

The first study tested whether imagining a positive interaction with an outgroup stranger could lead to more positive attitudes toward the outgroup and to less intergroup anxiety and more positive behavioral intentions when compared with a no-imagery/contact control group. The study also tested whether these effects lasted in time. The results showed that attitudes and behavioral intentions did not become more positive over time for imagined contact, but that imagined contact nevertheless significantly and positively differed from the control condition in post and delayed posttest measurements of both attitudes and behavioral intentions. The opposite trend was noted for intergroup anxiety as participants in the imagined contact condition reported less intergroup anxiety over time, with posttest and delayed posttest levels of anxiety being significantly lower than pretest levels. Also, initial between-condition differences on pretest anxiety, whereby control condition participants reported lower anxiety than imagined contact participants, were no longer significant immediately after contact or a week later. The results largely supported Hypotheses 1 and 2, and thus provided evidence for the impact of imagined contact on yielding positive intergroup outcomes that last for at least a week.

Following on these results, a second study was designed to serve as a replication and an extension of the first study and it also sought to correct the methodological shortfalls of it. In Experiment 1, there was no random allocation of participants to the two conditions. Even though this was mitigated by performing a random allocation test and controlling for baseline differences in Experiment 1, there could have been variables other than the ones inserted in the random allocation test that could have affected the behavior of participants in the

two conditions. For this reason, random allocation was strived for in Experiment 2. Moreover, the sample in Experiment 2 included both males and females as opposed to the female-only sample that was used in Experiment 1. Furthermore, the control condition in the first study controlled for contact but not for positive imagery and this was corrected in the second experiment using more apt and thus stricter control condition, namely a positive imagery/no-contact control condition which is commonly deployed in imagined contact studies. Finally, given recurrent claims in the literature that the primary function of imagined contact is to serve as a pre-contact tool (Crisp & Turner, 2013), that is to prepare individuals for more intimate types of contact, such as direct contact, by instigating their interest for contact and by psychologically preparing them for the contact situation, one more variable was added in the second study, contact self-efficacy, which taps onto preparedness for direct contact.

6 | EXPERIMENT 2

The second experiment aimed at replicating the results of the first experiment regarding outgroup attitudes, intergroup anxiety, and behavioral intentions and to also test for imagine contact's ability to yield higher contact self-efficacy that would be traceable a week after imagery. Experiment 2 was conducted in the same setting, (Nicosia, Cyprus) with Greek Cypriot participants and Turkish Cypriots as the outgroup.

6.1 | Hypotheses

The hypotheses were identical to those of Experiment 1:

> **Hypothesis 1:** Imagined contact will yield more positive attitudes, lower intergroup anxiety, more positive behavioral intentions, and higher contact self-efficacy at posttest and also by comparison to the control condition.
> **Hypothesis 2:** The effects of the imagined contact will last for a week.

6.2 | Participants

Participants were recruited from undergraduate classes at a public university. This time, recruitment efforts took place in more than one faculties in order to have a larger pool of male potential participants. As a result, 46 participants were recruited, 13 males and 29 females, and they were evenly and randomly distributed to either the control or the imagined contact condition. While all participants completed all three time points of measurement, four participants had to be excluded from the sample, three from the control condition and one from the imagined contact condition as they reported not being Greek Cypriot. Three of them were Greek and one of them was Pontian Greek. The final sample size per group was 22 for imagined contact and 20 for the control condition. The participants were

rewarded for their participation with a place in a raffle that would give four 30-euro prizes.

6.3 | Method

The method used in Experiment 2 was identical to the method used in Experiment 1 with the only exception being that all participants came to the lab for an experiment and they were then randomly allocated to one of the conditions. The participants who defined the control group were not to complete an online task but to imagine a positive scene. Specifically, they were provided with the following scenario and were asked to imagine the scene described in that for 1 min:

> I will now ask you to close your eyes and imagine that you are in an outdoor place in Cyprus that you like very much. The day is beautiful and you find yourself feeling good. Try to imagine the scene as vividly as possible.

The imagined contact participants were given exactly the same scenario as in Experiment 1 and 1 min to imagine it. Both control and imagined contact participants were then asked to write down in as much detail as they wished the scene they had imagined.

6.4 | Measures

Similarly to Experiment 1, the pretest, posttest, and delayed posttest questionnaires used in Experiment 2 included measurements of constructs that were not directly relevant to the main focus of the study (e.g., personality traits, attitudes toward other outgroups and identification measures). This was to prevent, as much as possible, task demand characteristics. The measurements of the main variables are presented next.

Outgroup attitudes, intergroup anxiety, and *prior direct contact* were measured using the same items as in Experiment 1. *Behavioral intentions* comprised of five items asking participants to what extent they would be willing to: "*meet more Turkish Cypriots*"; "*have Turkish Cypriot friends*"; "*live in the same neighborhood as Turkish Cypriots*"; "*have Turkish Cypriots in class*; (e) "*have Turkish Cypriots as family members,*" on a scale ranging from 1 (not willing at all) to 5 (extremely willing).

Contact self-efficacy was measured with four items that were adapted from Fan and Mak's (1998) measure of social self-efficacy. The items were the following: "*I think it will be easy for me to talk to Turkish Cypriots*"; "*I am sure I can resolve problems that may emerge during contact with Turkish Cypriots*"; "*I may not know enough about Turkish Cypriots to be able to have a good conversation with a member of this community*" (R); "*I am not entirely sure that I will have topics to discuss in a conversation with a Turkish Cypriot*" (R). All items were rated on a 5-point scale (1, strongly disagree; 5, strongly agree). The Cronbach's alpha for all the multi-item scales are reported in Table 2.

		Control condition (N = 20)	Imagined contact (N = 22)
		M (SD)	M (SD)
Pretest	Outgroup attitudes	4.95 (2.36)	6.80 (2.78)
	Intergroup anxiety (a = 0.80)	2.55 (0.77)	2.36 (0.78)
	Behavioural intentions (a = 0.91)	2.89 (0.98)	3.20 (0.99)
	Contact self-efficacy (a = 0.56)	3.06 (0.72)	3.39 (0.80)
	Prior direct contact	1.95 (0.95)	1.95 (1.10)
Posttest	Outgroup attitudes	6.27 (1.78)	6.70 (2.39)
	Intergroup anxiety (a = 0.85)	2.23 (0.80)	2.28 (0.89)
	Behavioural intentions (a = 0.91)	3.05 (0.94)	3.05 (1.16)
	Contact self-efficacy (a = 0.60)	3.40 (0.52)	3.25 (0.73)
Delayed posttest	Outgroup attitudes	5.73 (1.88)	6.50 (2.67)
	Intergroup anxiety (a = 0.83)	2.26 (0.71)	2.29 (0.84)
	Behavioural intentions (a = 0.93)	2.99 (0.88)	3.25 (1.08)
	Contact self-efficacy (a = 0.76)	3.25 (0.68)	3.26 (0.76)

TABLE 2 Means and standard deviations for pre, post, and delayed posttest outgroup attitudes, intergroup anxiety, behavioral intentions, and contact self-efficacy, and means and standard deviations for prior direct contact at pretest (Experiment 2)

6.5 | Results and discussion

To test for success in randomly allocating participants in the two conditions a MANOVA using Pillai's test was conducted with Condition (imagined contact vs. control condition) as the independent variable and with prior direct contact, outgroup attitudes intergroup anxiety, behavioral intentions, and contact self-efficacy, at pretest, as dependent variables. The multivariate effect was not significant, $V = 0.17$, $F (5, 36) = 1.52$, $p = 0.21$, partial $\eta^2 = 0.17$ thus showing that participants in the two groups did not significantly differ from each other with regards to the variables of interest.

In order to test the two hypotheses of Experiment 2, four mixed ANOVAs were conducted, one for each dependent variable (outgroup attitudes, intergroup anxiety, behavioral intentions, and contact self-efficacy). Time of measurement (pretest, posttest, and delayed posttest) was the within-subjects variable and Condition (imagined contact vs. control condition) as the between-subjects variable). The means and standard deviations for each variable at all times of measurement both for the imagined contact as well as for the control condition are presented in Table 2. The means for prior direct, also presented in Table 2, attest to the low direct contact levels of this study's participants.

6.6 | Outgroup attitudes

The analysis yielded a main effect of Time of measurement, $F (2, 80) = 3.21$, $p = 0.046$, partial $\eta^2 = 0.07$ and an interaction effect, $F (2, 80) = 4.66$, $p = 0.01$, partial $\eta^2 = 0.10$, but no main effect of Condition $F (1, 40) = 2.37$, $p = 0.13$, partial $\eta^2 = 0.06$. A look into the breakdown of the interaction starting with the pairwise comparisons

for the differences of the two conditions across time points, indicated that at pretest, outgroup attitudes between the two conditions were significantly different (mean difference = 1.85, SE = 0.79, $p = 0.025$, CI: [0.25, 3.44]) with control group participants reporting more positive attitudes. This difference was no longer there at posttest (mean difference = 0.43, SE = 0.65, $p = 0.51$, CI: [−0.88, 1.73]) and at delayed posttest (mean difference = 0.77, SE = 0.71, $p = 0.28$, CI: [−0.67, 2.20]). Furthermore, pairwise comparisons between times of measurement within each condition showed no pretest–posttest–delayed posttest differences for the control condition but significant pretest–posttest differences (mean difference = 1.32, SE = 0.40, $p = 0.006$, CI: [0.32, 2.31]) and pretest–delayed posttest differences (mean difference = 0.77, SE = 0.29, $p = 0.03$, CI: [−0.34, 1.58]) for the imagined contact condition. These results support both Hypotheses 1 and 2 according to which imagined contact would elicit more positive attitudes detected immediately after contact as well as a week later.

6.7 | Intergroup anxiety

The mixed ANOVA for intergroup anxiety showed a main effect of Time of measurement, $F (2, 80) = 5.52$, $p = 0.006$, partial $\eta^2 = 0.12$, but no interaction effect, $F (2, 80) = 2.15$, $p = 0.12$, partial $\eta^2 = 0.051$, or main effect of Condition $F (1, 40) = 0.03$, $p = 0.87$, partial $\eta^2 = 0.001$. To look into the main effect of time, separate Repeated Measures ANOVAs, one for each condition were conducted to test how anxiety scores changed within each condition over time. Analyses yielded a significant effect for the imagined contact $F (2, 42) = 8.21$, $p = 0.001$, partial $\eta^2 = 0.28$ but not for the control condition $F (2, 38) = 0.79$, $p = 0.46$, partial $\eta^2 = 0.04$. Both linear as well as

quadratic contrasts were significant, $F(1, 21) = 8.66$, $p = 0.008$, partial $\eta^2 = 0.29$ and $F(1, 21) = 8.00$, $p = 0.010$, partial $\eta^2 = 0.27$, respectively, essentially showing that anxiety levels within the imagined contact condition decreased over time but that anxiety levels were lowest at posttest. Both posttest and delayed posttest intergroup anxiety was significantly lower than pretest anxiety: mean difference$_{\text{pretest-posttest}} = 0.32$, SE = 0.11, $p = 0.015$, CI: [0.05, 0.59]) and mean difference$_{\text{pretest-delayed posttest}} = 0.30$, SE = 0.09, $p = 0.010$, CI: [0.06, 0.53]. There were no significant differences between conditions, however, at posttest or at delayed posttest. The results provide support for Hypotheses 1 and 2 as far as time-changes within each condition are concerned. They therefore provide some evidence for imagined contact's capacity to yield a sustained reduction of intergroup anxiety.

6.8 | Behavioral intentions

The analysis for behavioral intentions yielded no main effect of Time of measurement $F(2, 80) = 0.72$, $p = 0.49$, partial $\eta^2 = 0.01$ and no main effect for Condition, $F(1, 40) = 0.39$, $p = 0.53$, partial $\eta^2 = 0.01$, but a marginally significant interaction effect $F(2, 80) = 2.95$, $p = 0.058$, partial $\eta^2 = 0.01$. The breakdown of the interaction indicated that while no differences existed between the two conditions at each time of measurement, the two conditions behaved differently by comparison to each other across time. More specifically, no pretest–posttest–delayed posttest differences were detected for imagined contact but significant posttest to delayed posttest differences existed for the control condition. Behavioral intentions became marginally more positive at delayed posttest by comparison to posttest for the control condition (mean difference = 0.20, SE = 0.08, $p = 0.06$, CI: [−0.004, 0.40]). This was an unexpected finding that did not provide support to either hypothesis and was inconsistent with the results of Experiment 1 on this variable.

6.9 | Contact self-efficacy

The mixed ANOVA for contact self-efficacy yielded no main effect of Time of measurement $F(2, 80) = 1.63$, $p = 0.20$, partial $\eta^2 = 0.04$ or of Condition, $F(1, 40) = 0.20$, $p = 0.65$, partial $\eta^2 = 0.01$, but a significant interaction effect $F(2, 80) = 3.49$, $p = 0.035$, partial $\eta^2 = 0.08$. The breakdown of the interaction showed no difference between conditions in any of the times of measurement but different patterns within each condition across time. Contact self-efficacy levels significantly increased at posttest by comparison to pretest (mean difference = 0.34, SE = 0.13, $p = 0.011$, CI: [0.06, 0.62], only for imagined contact. No other differences between time points were detected for either of the conditions. These results support Hypothesis 1: that imagined contact would promote higher levels of contact self-efficacy. However, the results do not support Hypothesis 2 that changes in contact self-efficacy would still be traceable a week after contact. The results showed that the delayed posttest contact self-efficacy was not statistically higher than pretest efficacy.

Experiment 2 aimed at replicating the effects of Experiment 1 and extending them by adding one more dependent variable, contact self-efficacy. Emphasis was paid in amending the methodological weaknesses of Experiment 1 by introducing random allocation of participants to the imagined contact and control condition, while also using more appropriate control group and a sample consisting of both males and females. These results replicated the results of Experiment 1 as far as outgroup attitudes and intergroup anxiety are concerned. Both outgroup attitudes and intergroup anxiety changed over time with attitudes becoming more positive and anxiety becoming lower than pretest attitudes and anxiety respectively at both posttest and delayed posttest for imagined contact, but not for the control condition. Unlike the results of Experiment 1, no effects favoring imagined contact were found for behavioral intentions. In Experiment 2, behavioral intentions did not become more positive over time for imagined contact. Instead there was an odd post-to-delayed posttest shift toward more positive intentions in the control group. Finally, imagined contact led to higher contact self-efficacy at posttest but this change did not last in time as delayed posttest levels of contact self-efficacy were not statistically different than pretest levels of the same variable.

6.10 | General discussion

A review of the literature shows that there is a sizeable volume of studies putting imagined contact to the test, however, the vast majority of these studies have focused on the immediate effects of imagined contact. Research on the endurance of these effects has been scarce (Miles & Crisp, 2014) and the few studies forming the exception to this were all conducted in the same country with predominantly children as participants. Testing for the endurance of imagined contact's effects is important because it speaks to the capacity of the intervention to elicit effects that are resistant to time which would mean that they would be present when needed the most (i.e., at a future point in time when the opportunity for actual contact arises). Furthermore, sceptics of imagined contact have questioned the usefulness of imagined contact as a tool in settings of full-blown conflict (Lee & Jussim, 2010). The research reported here addressed both of the aforementioned points by testing not just the immediate but also the long-term effects of imagined contact in Cyprus, a context with a recent history of violent intergroup conflict and long-standing segregation of the two conflicting groups, Greek and Turkish Cypriots. The endurance of imagined contact's effects has never been tested in this kind of contexts.

The results of two experiments deploying the same experimental design but different control conditions showed that imagined contact can indeed cause changes in attitudes and emotions (intergroup anxiety) that can last in time. Despite outgroup attitudes and intergroup anxiety being two very commonly used variables in imagined contact studies, this is the first time that imagined contact's long-term effects on these variables were put into test. The importance of these results for imagined contact as a prejudice reduction intervention looms even larger in the context of Cyprus, where published

studies testing the long-term effects of direct as well as vicarious contact, another indirect form of contact, on these same variables (attitudes and intergroup anxiety) showed that both of these types of contact could cause anxiety reduction that lasted over time but no long-term attitude changes (Ioannou, Al Ramiah, & Hewstone, 2018). In this study, imagined contact was found to yield attitudes that were significantly more positive than the attitudes of a no-contact/no-imagery control group at both posttest and delayed posttest (Experiment 1) and significantly more positive than pretest attitudes at posttest and delayed posttest measurements in Experiment 2. While this difference in the results of these three types of contact in studies deploying the same design and the same outcome variables are noteworthy, to fairly compare and contrast different types of contact in terms of their prejudice reduction capacity these types of contact need to feature in the same study (experimental) design. Such a task could be undertaken by future studies in order to produce important knowledge, currently missing, in the research area of direct and indirect contact.

Interestingly, while the results on attitudes and intergroup anxiety were consistent and promising over two studies, the results for behavioral intentions and contact self-efficacy were less promising and not as consistent. The long-term effect of imagined contact on behavioral intentions was demonstrated only in Experiment 1 but in Experiment 2 no posttest or delayed posttest differences favoring imagined contact were registered. Contact self-efficacy was only tested in Experiment 2 to find that short-term changes (differences between pretest–posttest contact self-efficacy) did not last in time as at delayed posttest levels of self-efficacy regressed to pre-contact levels.

The discrepancy of the results within this set of variables is even more intriguing given the existing knowledge base. For example, the recent results of Miles and Crisp's (2014) meta-analysis showed that imagined contact has a larger effect on intended behavior toward the outgroup (a category of outcomes including variables such as behavioral intentions and contact self-efficacy) than on attitudes and emotions (such as intergroup anxiety). In addition, imagined contact as an intervention is thought to be better fit at paving the way for more intimate forms of contact like direct contact (Crisp & Turner, 2012, 2013). Preparing the ground for actual contact practically translates into imagined contact increasing intentions to pursue future contact and lifting the confidence of individuals to have contact (contact self-efficacy).

An explanation for the divergence in the results on outgroup attitudes and intergroup anxiety on one hand, and behavioral intentions and contact self-efficacy on the other hand, might be found in the particularities of the population under study: young Greek Cypriots, who are the segment of Greek Cypriot population consistently reporting the lowest levels of contact with the Turkish Cypriot community (UNDP-ACT & SeeD, 2015). The means of prior contact reported by participants in this study are low too and show that on average participants reported having had no contact at all to having had very little contact with Turkish Cypriots in the past. It seems that for these participants who have had limited to no contact with the outgroup, imagining a positive interaction with an outgroup stranger primarily resulted in urging them to re-think the way they feel about the outgroup, that is, caused them to feel overall more positive about Turkish Cypriots and to feel less threatened and overwhelmed in a potential scenario where they find themselves amidst outgroup members. A single instance of positive imagery on the other hand, does not seem to have equipped them with sustained self-confidence in pursuing actual contact (contact self-efficacy) and, perhaps as an effect of that, no consistent willingness to pursue contact in real life.

Granted that imagined contact should ideally function as an enabler of more intimate forms of contact, and not as a permanent alternative to direct contact, then the lack of strong support for the impact of imagined contact on enduring positive behavioral intentions and high-contact self-efficacy in this set of studies could be seen as an area of improvement for imagined contact interventions especially in segregated contexts, where neither contact or opportunities for contact are infrequent. This could be done via alterations in the form of elaborations to the imagined contact scenario, something that has been attempted in the past (e.g., Bagci, Piyale, & Ebcim, 2017; Husnu & Crisp, 2010b; Ioannou et al., 2017) but without the specific aim of strengthening imagined contact's impact on variables tapping onto the psychological preparation of individuals for future direct contact.

While this study provides support to the argument about the long-term effects of imagined contact which is robust for attitudes and intergroup anxiety, one could raise the question of whether a week-long interval is long enough to make an argument about lasting effects. Indeed, this is where one methodological limitation of the study is found. The time-lag of a week in this study was mainly chosen because it was already used in studies testing the long-term effects of imagined contact (Vezzali et al., 2012; Vezzali, Stathi et al., 2015). Whereas the time-lag of 1 week is rather arbitrary, of course, the period of 7 days can be considered to be long enough for participants to contemplate on their experience, discuss about it with friends, family, or other participants, or to even forget about it. It also provides participants with the opportunity to reconsider their posttest answers and revise them a week after contact. This can result in fading initial impressions/reactions as was the case with some variables in this study or in the reverse effect, a late onset of change, akin to the sleeper effect (see Hovland, Lumsdaine, & Sheffield, 1949; Kumakale & Albarracin, 2004, for a meta-analytic review of the sleeper effect in persuasion) which was, however, not observed for imagined contact in this set of studies. A longer time-lag would obviously serve as a stricter test of the longevity of imagined contact's effects. However, it is important to emphasize that the set of studies reported in this paper concerned a 1 min intervention. Hence, relatively, in terms of time, one could argue that 1 week might be considered as "long-term."

A second limitation of this study lies in the small sample size utilized in both experiments. Even though samples of around 20 participants per condition are not uncommon in imagined contact studies (see Miles & Crisp's, 2014, meta-analysis), it is worth acknowledging that small sample sizes come with the risk of compromised statistical

power. A third limitation is the use of identical items or scales to measure the outcome variables and thereby register change. This practice could have arguably given away the goal of the experiment and thus cause response bias, which in this case would take the form of participants responding in ways that would either please the experimenter or in ways via which they would show their objection to the purpose of the intervention. Neither of these possible biased behaviors seemed to have occurred in these studies. If participants were indeed interested in pleasing the experimenter, then they would have probably done so across time points which was not the case given the consistent regression to pretest scores in delayed posttest measurements. In addition, no negative reactions were registered in participants' scores (e.g., no sharp decrease in any of the outcome variables between pretest and posttest).

With these limitations noted, this study was useful in demonstrating that imagined contact's positive intergroup effects are not merely a positive reaction to a positive intervention but that some effects as for example, intergroup anxiety reduction, linger on. What the findings of this study do not reveal is how much longer these effects will be there for and for this reason future studies could invest in monitoring what sort of changes occur following the imagined contact intervention in terms of the attitudes, emotions, and intergroup practices of the participants a number of weeks after the intervention.

Absent from this study, as well as from the majority of studies in the area of (imagined) contact, was the monitoring of the actual incidence of contact and/or the internal and external processes that take place in between measurements that could explain participants' longitudinal behavior. The contact moment in particular, has, to date, been largely treated as a "black box" in which people enter and then exit as seemingly changed people (see Harwood, 2010, p. 164). Inquiring into what happens to people when they are asked to imagine something that they have, in most cases, never imagined before could shed light to how imagined contact works and also when its effects are more pronounced and more lasting. Hence, I conclude this paper by recommending that future research is carried out for the purpose of shedding light to the processes of (imagined) contact in order to unravel, possibly with the use of qualitative methods, what elements of people's imagery (e.g., the content or the process of the mental simulation), are key to a significant and lasting impact of imagined contact on positive intergroup outcomes. A closer look at the content and the detailed processes of mentally simulating a positive intergroup interaction will also provide valuable input for the designing or refining future interventions.

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