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Experimental Evidence for the Secondary Transfer Effect: An Investigation of Direct and Vicarious Contact in the Laboratory

Hermann Swart[1] | Simon Lolliot[2] | George Thomas Berry[1] | Simone Strydom[1] | Miles Hewstone[3]

[1]Stellenbosch University, Stellenbosch, South Africa | [2]University of British Columbia, Vancouver, Canada | [3]University of Oxford, Oxford, UK

Correspondence: Hermann Swart (hswart@sun.ac.za)

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ABSTRACT

Positive intergroup contact promotes positive outgroup attitudes (primary transfer effect; PTE), and these primary outgroup attitudes can generalise to other outgroups (the secondary transfer effect; STE). However, experimental evidence demonstrating the STE is rare. We collected data from 116 White South African female participants using a pretest-posttest-delayed-posttest design with randomised allocation across three conditions (direct contact, vicarious contact, control), in the first simultaneous laboratory experimental test of the STE of both direct and vicarious contact. Participants in the vicarious contact condition observed another White South African female (an ingroup friend) in the direct contact condition completing a Relationship Closeness Induction Task with a female Black (African) South African (primary outgroup) confederate. We found: (1) a PTE for outgroup attitudes (but not trust) at immediate- and delayed-posttest; (2) an STE (for attitudes, but not trust) at immediate- and delayed-posttest: direct and vicarious contact with the primary outgroup improved attitudes towards Coloured South Africans (secondary outgroup) via changes in primary outgroup attitudes; and (3) an STE for a more demanding version of the STE, namely attitude-to-trust generalisation: changes in primary outgroup attitudes promoted changes in secondary outgroup trust at immediate- and delayed-posttest. We discuss the contrasting results we observed for the generalisation of outgroup attitudes versus the generalisation of outgroup trust and offer suggestions for future research. These encouraging results should support future interventions designed to reduce prejudice across multiple outgroups.

1 | Introduction

Dietrich Bonhoeffer, the anti-Nazi dissident and theologian, was sensitised to the discriminatory treatment of other groups, notably Jews, in his native Germany, by a formative period he spent in the USA. There he developed close relationships with worshippers at the Abyssinian Baptist Church in Harlem (Ziegler 2019), during which he became acquainted with the stigma faced by what were then called 'negroes' (today, African Americans). Bonhoeffer's experience illustrates how contact with one 'outgroup' (in this case, negroes) could affect attitudes towards other outgroups (in this case, Jews in Germany).

Pettigrew (1997) identified this effect (observed, initially, by Weigert 1976) as a distinct form of generalisation of intergroup contact effects. Pettigrew (2009) later termed it the *secondary transfer effect* (STE). This can be contrasted with the *primary transfer effect* (PTE) of intergroup contact, as described by Allport's (1954) 'contact hypothesis', whereby direct, face-to-face contact with one or more members of an outgroup is negatively

Hermann Swart, Simon Lolliot, and Miles Hewstone are joint lead authors.

Miles Hewstone is Emeritus Professor, University of Oxford.

associated with prejudice towards that same outgroup as a whole (for a meta-analysis, see Pettigrew and Tropp 2006).

Pettigrew (2009) wrote that the STE 'goes well beyond the primary transfer effect between the specific outgroup members with whom there is contact and the entire outgroup in that decreased prejudice from contact with one outgroup extends to another group not involved in the contact. That is, attitudes toward a noncontacted outgroup improve over and above any effect of contact with that outgroup following the attitude change that occurs with the contacted outgroup'. (55–56). Pettigrew (2009) did not use the term, *attitude generalisation*, but he cited Walther's (2002) work on evaluative conditioning, as showing a 'spreading attitude effect' (63) similar to transfer effects of intergroup contact. Thus, Pettigrew (2009) clearly saw the STE as *involving* attitude generalisation.

Although there is now extensive evidence supporting the STE (for reviews see Boin et al. 2021; Lolliot et al. 2013; Vezzali et al. 2021), evidence from experimental research, which is necessary to demonstrate the *causal* effect of contact with the primary outgroup on attitudes towards secondary outgroups, is rare. So, too, are studies that have investigated whether not only direct but also indirect forms of contact (in which there is no direct encounter with outgroup members) can produce this effect. We investigated the STE of direct and one form of indirect (vicarious) contact within the South African context. To our knowledge, this research offers the first simultaneous laboratory experimental test of the STE of direct and vicarious contact. Rather than provide a competitive test of direct versus indirect contact and the STE (which would ultimately depend on which operationalizations of each form of contact were, or were not, used), we sought to provide a laboratory test of whether both forms of contact could produce STEs under very similar conditions and strict experimental control.

Vezzali et al. (2021) located 43 studies of the STE, most of which used a measure of direct contact (as specified in Pettigrew's 2009, definition of the STE). They found supportive evidence across mainly cross-sectional correlational (e.g., Schmid et al. 2012), but also longitudinal (e.g., Tausch et al. 2010, study 4), and experimental (e.g., Shook et al. 2016) studies conducted with a wide range of different types of groups, and across several continents (mainly in Europe, but also in North America, Asia, South America, and Oceania, but not, as yet, in Africa). Evidence has also accrued from studies that controlled for other predictors of prejudice (e.g., Vezzali and Giovannini 2011) and socially desirable responding (Tausch et al. 2010), and that ruled out shared method variance between measures of the primary and secondary outgroups as an explanation for the STE (e.g., Schmid et al. 2012).

Notwithstanding the evidence supporting the STE reviewed by Vezzali et al. (2021), research does not always deliver strong support for the STE of direct contact, as observed in three three-wave longitudinal studies that were published after Vezzali et al.'s (2021) review. We highlight these studies here because three-wave longitudinal studies (compared with cross-sectional studies) offer a more appropriate test of the version of the STE involving attitude generalisation, because they allow for tests of full mediation (Cole and Maxwell 2003; Selig and Preacher 2009; e.g., Henschel et al. 2025). Henschel and Kötting (2023) failed to find evidence for the STE of positive intergroup contact in their three-wave longitudinal study. In contrast, Kauff et al. (2023) found support for the STE of positive intergroup contact in only one (Study 1b) of their two three-wave surveys. These inconsistent longitudinal results may point to the challenge of identifying the appropriate timescale for testing effects that require time to manifest (Hamaker 2023). This is likely to be the case with changing attitudes in general (e.g., Watts and Holt 1970), and the generalisation of attitudes across outgroups, in particular (Pettigrew 2009). If the timescale is too short, the effects might not have come to fruition; if the timescale is too long, any effects that do not last long enough in the absence of repeated contact might already have dissipated. This is one of the challenges of undertaking longitudinal research in this area.

Relative to the evidence for STEs arising from direct contact, evidence for STEs due to indirect contact is sparse. Vezzali et al. (2021) identified eight studies of which two were cross-sectional, but only one that used a discrete measure of extended contact (cf. Schmid et al. 2012) and detected an STE (Vezzali et al. 2020). Of the six experimental studies, only two manipulated vicarious contact (Andrews et al. 2018; Joyce and Harwood 2014), eschewing the minimalist form of imagined contact (e.g., De Carvalho-Freitas and Stathi 2017; Harwood et al. 2011), which is not based on anyone actually engaging in contact (see Paluck et al. 2021).

Vezzali et al. (2021) concluded that research on the STE had addressed methodological limitations and evidenced a robust effect. However, some conceptual confusion has crept into the literature on what is meant by an STE and how to test it, especially as researchers have sought to shed light on the processes that underlie it. Vezzali et al. (2021) wrote that attitude generalisation (the process by which attitudes towards one attitude object generalise to other, linked attitude objects; e.g., Walther 2002) was the most studied (and supported) mediator of the STE. Yet, attitude generalisation cannot, logically, be conceived as a potential *mediating mechanism* for an STE that is already defined as the phenomenon involving such generalisation (as per Pettigrew's 2009, definition). Thus, the appropriate test of STEs involving attitude generalisation is to confirm the presence of attitude generalisation by mediation analyses (e.g., Henschel and Derksen 2023).

Despite this burgeoning research on STEs, gaps remain. The following five sections consider: (1) the limited experimental research on direct contact and the STE; (2) the potential of indirect contact to produce STEs; (3) the limited experimental research on indirect contact and the STE; (4) the limited research on STEs that involve trust; before turning to (5) the present research.

1.1 | Experimental Studies of Direct Contact and the STE

Vezzali et al. (2021) reported seven experimental tests of direct contact and STEs, which we reduced to six studies because Jasinskaja-Lahti et al. (2021) did not manipulate but measured prior contact. We describe the best four studies below, each robust and high in external validity.

Berger et al. (2016) tested the efficacy of a class exchange programme in the ethnically mixed city of Jaffa, Israel, aimed at reducing prejudicial attitudes. Third and fourth grade students from both Israeli–Jewish and Israeli–Palestinian schools were randomly assigned to intervention (throughout the academic year) or control (a social–emotional learning program) classes. The intervention's outcomes were measured a week before, immediately after, and 15 months following termination. Even at 15 months after the program concluded, the effects of the intervention were generalised from contact with the primary outgroup (Israeli–Jewish or Israeli–Palestinian) to a non-contacted, secondary outgroup (Ethiopians).

The remaining evidence comes from three studies that all exploited an actual residential housing situation to provide (in some instances) a true (and in other instances a quasi-) field experimental test that allows researchers to draw causal inferences regarding whether living with same-race versus different-race roommates during the first year of university reduced prejudice. First, van Laar et al.'s (2005) study compared the effects of randomly assigned same-race versus mixed-race roommates and found a significant STE, particularly for the two most stigmatised ethnic groups (i.e., from contact with Black roommates to attitudes to Latinos, and vice versa). They also found an exception, whereby contact with Asian American roommates tended, especially for White respondents, to make attitudes towards other groups more negative.

Second, Mark and Harris (2012) assessed freshman students' ego networks in a quasi-experiment. Although this was not strictly a test of the STE (because it did not assess generalisation from primary to secondary outgroup attitudes), the authors did report that being assigned a different-race roommate increased the number of friends of the roommate's race (a form of PTE) but did not increase the number of different-race friends whose race differed from that of their roommate. An increase in the number of friends from secondary outgroups would have been consistent with an STE.

Third, Shook et al.'s (2016) semester-long study found that participants with different-race roommates reported more positive attitudes towards secondary groups (i.e., racial/ethnic groups other than their roommates' group), and lower levels of Social Dominance Orientation (SDO; Pratto et al. 1994), at the end of the semester than participants with same-race roommates (with SDO mediating the STE), whereas those with same-race roommates exhibited no change. Notwithstanding its strengths, this study is limited as a test of STEs. There was no common primary outgroup for students with different-race roommates (i.e., roommate race was not identical for all participants with different-race roommates). Thus, intergroup contact was collapsed across various outgroups and attitudes towards all racial/ethnic minorities were assessed for STEs.

One further note of caution should be sounded about these experimental studies on direct contact and the STE. None of them assessed contact during the period of the study, or at follow-up. Given that they ran over several months, students who received the intervention or were assigned a different-race roommate may have experienced different valence, quantity, or quality of contact, with other students of the same race as their roommate (as observed by Mark and Harris 2012, and by van Laar et al. 2005, who found that being assigned an African American roommate led to greater ethnic heterogeneity amongst the closest friends of both Asian American and White students). This points to a potential limitation of the roommate paradigm—when studies fail to control for other outgroup contacts than those with the roommate, the fidelity of the manipulation (i.e., a roommate of a specific outgroup) is weakened.

1.2 | The Promise of Indirect Contact for Producing STEs

Two major forms of indirect contact are reliably associated with reduced prejudice. The 'extended contact hypothesis' (Wright et al. 1997) proposes two distinct mechanisms beyond direct contact: (a) extended contact refers to *knowing* that an ingroup member is friends with an outgroup member, and (b) vicarious contact refers to *observing* an ingroup member interacting with an outgroup member. Both approaches have a compelling theoretical basis (see Vezzali et al. 2014), and there is empirical support for some of their key underlying mechanisms (e.g., Turner et al. 2008). The narrative review by Vezzali et al. (2014) summarises the empirical support for both extended and vicarious contact effects, while the meta-analysis undertaken by Zhou et al. (2019) found comparable effect sizes for extended and direct contact.

Both extended and vicarious contact may have special value for situations in which groups have recently been in conflict (Tausch et al. 2006), as demonstrated, for example, amongst Catholics and Protestants in Northern Ireland (Paolini et al. 2004), where knowing of positive interactions of an ingroup member with an outgroup member was associated with reduced intergroup anxiety (i.e., apprehension about future interactions with outgroup members; Stephan 2014). If cross-group friendship is generally discouraged by other ingroup members, then observing or knowing that an ingroup member has an outgroup friend can also change perceived ingroup norms (see Turner et al. 2008). This should be especially the case when the ingroup member is representative of one's group (for a review, see Vezzali et al. 2014). Finally, extended and vicarious contact may reduce prejudice for more people, as an individual does not need to *know personally* an outgroup member to benefit from its positive effects. This is important when there is stark segregation (e.g., Northern Ireland, South Africa), making direct contact impractical or very difficult (Christ et al. 2010).

In sum, indirect contact works via mechanisms that differ from those involved in direct contact (Turner et al. 2008) and might provide a different route to the same destination (attitude change and/or increased trust). It also has potential not so much as an alternative to direct contact, but a step towards direct contact (Turner et al. 2007), preparing people for actual contact (Mazziotta et al. 2011) and predicting future direct friendships (Wölfer et al. 2019). Given these benefits of indirect contact, it is worth considering the potential of indirect contact for producing generalised attitude change across outgroups (i.e., a version of the STE involving attitude generalisation) as an alternative to achieving this outcome via direct contact. This potential is supported by the

meta-analysis of contact interventions undertaken by Lemmer and Wagner (2015). They identified 16 studies that employed indirect contact and reported a reliable STE involving attitude generalisation across unspecified outgroups.

1.3 | Experimental Tests of Indirect Contact and the STE

Vezzali et al. (2021) reviewed five experimental tests of indirect contact and STEs, of which three manipulated imagined contact (i.e., involved no actual contact at all). Two studies investigated vicarious contact (Andrews et al. 2018; Joyce and Harwood 2014), of which the study by Joyce and Harwood (2014) yielded more persuasive evidence. They exposed participants to one of four experimentally constructed videos depicting either an intergroup interaction (between a volunteer border-patrolman from the United States and a family of illegal immigrants), which manipulated the valence of the vicarious intergroup contact (positive/mixed-neutral/negative), or they viewed a nature documentary (control condition). STEs were found when generalising attitudes from the primary outgroup (immigrants) to (a) secondary outgroups that were related to illegal immigrants (either as a sociopolitical group or as an ethnic minority group), but not (b) unrelated secondary outgroups (Whites and the Elderly).

Given the efficacy of both extended and vicarious forms of indirect contact in relation to the PTE of contact (Vezzali et al. 2014; Zhou et al. 2019), their relative rarity in STE research, and their potential value for reducing intergroup conflict, they merit further attention. We sought to investigate indirect contact and STEs using vicarious contact, specifically, for two reasons. First, it is typically operationalised by asking participants to watch video clips depicting interactions between ingroup and outgroup members (e.g., Mazziotta et al. 2011; Wright et al. 1997, Study 4), making it ideal for the experimental investigation of STEs. Second, given the promise of indirect contact in interventions (e.g., Turner et al. 2007), vicarious contact has the advantage that it can be easily scaled up by using it in the form of so-called parasocial contact, which describes contact with outgroup members through the media (for a meta-analysis, see Banas et al. 2020).

1.4 | Beyond Attitude Generalisation: Promoting Generalised Outgroup Trust

Especially following conflict, not only attitudes but also trust is a key outcome of interventions to improve intergroup relations and promote reconciliation (Gibson 2006; Kelman 2005). Developing trust, or positive expectations around the intentions and behaviours of the outgroup towards the ingroup (Lewicki et al. 1998), can be especially demanding in contexts characterised by a history of intergroup conflict (Paolini et al. 2007) because it requires a degree of vulnerability that risks being exploited by the outgroup (Kramer and Carnevale 2001). Positive intergroup contact, and especially cross-group friendships (see Swart 2021, for a review), can be especially important for the development of both positive attitudes and trust (see Turner et al. 2010; Swart, Hewstone, Turner, and Voci 2011). Importantly, however, trust and attitudes are separate and distinct constructs—positive outgroup attitudes do not necessarily translate to greater outgroup trust (Tam et al. 2009). Thus, it is important that STE research consider the potential of positive direct (or indirect) intergroup contact for the development of both positive outgroup attitudes *and* trust (the latter offering arguably a more demanding test of the STE).

Yet, the only reference to trust in Vezzali et al.'s (2021) review of studies on the STE is as a mediator (Zezelj et al. 2020) between contact and attitudes (see also Boin et al. 2021). Our research sought to investigate whether a brief, highly controlled contact manipulation impacts not only primary and secondary attitudes but also trust. Thus, we tested for an STE involving attitude-to-trust generalisation. One might expect that regular, positive (direct or indirect) intergroup contact that has no negative consequences for either party will help to build confidence amongst group members and help to perpetuate the belief not only that the primary group means them and their group no harm, but that outgroups in general might be more benign than previously thought. We expect, however, that changing trust after a single positive intergroup encounter will be more demanding than changing attitudes, as research has found that multiple positive encounters are required for the development of a trusting relationship (Rothbart and Park 1986).

1.5 | The Present Research

In the present research, we sought to address the lacunae identified in the literature reviewed above to make five key contributions to the existing contact literature. First, given the dearth of experimental contact research on the STE (relative to the abundance of correlational studies), we adopted an experimental design that included the random assignment of participants (considered the 'gold standard' in contact research by Paluck et al. 2019), to test the causal relationship between primary outgroup contact and secondary outgroup attitudes and trust. Second, we monitored the level of intergroup contact over time (which is especially important for studies that unfold over a period of time; e.g., Berger et al. 2016; Shook et al. 2016). Third, our investigation aimed to answer the call by Harwood (2010) and Paluck et al. (2019) for research studying the effects of different forms of contact at the same time—we undertook the first laboratory experimental study of the STE to explore the effects of both direct and indirect contact within the same experimental study. Fourth, we investigated the duration of contact effects over time (a rare occurrence in experimental contact research), which is important for evaluating the potential policy relevance of contact research (see Paluck et al. 2019). Finally, we undertook the present research within the South African context, offering the first STE research that we are aware of to emerge from the African continent.

The multicultural South African context is ideal for studying the STE of intergroup contact. South Africa has enjoyed three decades of democratic, majority rule since the end of almost 50 years of apartheid in 1994. Under apartheid, South Africans were categorised as members of one of four main population groups—White (Caucasian, of European descent), Black (African), Coloured (of mixed racial heritage), and Indian (of

sub-continental Indian descent) South Africans—labels that remain widely used, for reference and self-reference, in everyday South African vernacular, and in the National Census. Post-apartheid, 'Black South African' is a political label commonly used to refer collectively to those racial groups that were discriminated against under apartheid (including Black (African), Coloured, and Indian South Africans). In the present research, we refer throughout to the three historically disadvantaged groups as Black (African) South Africans, Coloured South Africans, and Indian South Africans.

Whereas 'Black (African)' in the South African context denotes membership of one of the indigenous ethno-linguistic groups in South Africa (Christopher 2003; Tewolde 2024), interpretations of Coloured identity in the South African context are complex and often contested (see Pirtle 2023; Tewolde 2024). On the one hand, Coloured South Africans often share socio-cultural ties with White South Africans (including language and religion; Thompson 2001) and, on the other, they share socio-political ties with Black (African) South Africans (given their shared history as targets of oppression and discrimination during apartheid).

The effects of decades of discrimination by White South Africans against other population groups under apartheid, and the resulting historical socio-economic disadvantage, remain evident across the South African landscape. Black (African) South Africans comprise the overwhelming majority of the South African population, yet they remain significantly economically disadvantaged relative to the typically privileged minority population of White South Africans (and, to a lesser degree, Coloured and Indian South Africans). Furthermore, South African neighbourhoods remain largely segregated (no longer by law, but often as a reflection of economic mobility or the lack thereof). Reviews of intergroup attitudes in South Africa, undertaken by Bornman (2011) and Durrheim et al. (2011) show that White South Africans have historically held distinct attitudes towards Black (African) and Coloured South Africans—generally less positive attitudes towards Black (African) South Africans relative to the attitudes held towards Coloured South Africans (showing that these two groups are viewed by White South Africans as separate groups). However, the reviews undertaken by Bornman (2011) and Durrheim et al. (2011) suggest that attitudes towards Black (African) and Coloured South Africans amongst White South Africans seem to have improved since 1994 (although there is evidence that ingroup bias and outgroup distrust persist, while cross-group friendships remain limited; see Durrheim and Dixon 2010; Gibson 2004). The 17th round of the South African Reconciliation Barometer (Institute for Justice and Reconciliation 2021) found that intergroup interactions are least likely to occur in the homes of respondents and most likely to occur in workplaces (or places of study). Thus, it is possible that the participants in our study (university students) are exposed to significantly more frequent opportunities for intergroup contact on campus than they would be in their home neighbourhoods. Importantly, where positive intergroup contact does (and cross-group friendships do) occur—even in this demanding social context for attempts to promote more positive intergroup relations—such contact is associated with more positive outgroup attitudes (e.g., Christ et al. 2014; Swart et al. 2010; Swart, Hewstone, Christ, and Voci 2011; Van Assche et al. 2023).

We undertook experimental research investigating the primary and secondary transfer effects of direct and vicarious contact amongst female White South African university students, exploring how their direct (or vicarious) contact with a female Black (African) South African confederate impacted change in both their pretest attitudes and trust towards Black (African) South Africans in general (the PTE of intergroup contact) at immediate- and delayed-posttest, and their attitudes and trust towards Coloured South Africans in general (the STE of intergroup contact) at immediate- and delayed-posttest.

The pretest, immediate-posttest, and delayed-posttest design of the present study allowed us to explore the temporal dimension of the STE of contact, whereby we tested whether any STEs observed at immediate-posttest could still be observed 7 days later at delayed-posttest. As far as we are aware, this is the first study to do so in relation to the STE. In his longitudinal model of primary contact, Pettigrew (1998) described how contact effects are likely to develop over time, which has been supported by subsequent research. Initial interactions with outgroup strangers (even when these are positive) are often characterised by heightened intergroup anxiety (MacInnis and Page-Gould 2015). Thus, these initial interactions may have a limited impact on attitudes or trust. However, after repeated positive intergroup encounters, it is possible to reach what MacInnis and Page-Gould (2015; see also MacInnis and Hodson 2019) describe as a 'contact threshold'. When this 'contact threshold' is reached, positive encounters have what Cook (1962) refers to as 'acquaintance potential' and begin to have a meaningful impact on attitudes and trust. Thus, under conditions of repeated positive encounters, one would expect contact effects to become more pronounced over time.

However, our research design included only a single intergroup encounter, carefully designed to be as positive as possible for maximal impact and to accelerate the development of acquaintance potential (by way of a Relationship Closeness Induction Task; adapted from Sedikides et al. 1999). Therefore, we would expect that any effect of this one-off encounter would be stronger when measured immediately after the encounter (i.e., immediate-posttest) as compared to 7 days later (i.e., delayed-posttest), at which point the effects might have weakened. Additionally, we would expect that any effects of this one-off encounter on attitudes would be larger (and last longer) than its effects on trust (because trust is a more demanding criterion that typically requires multiple positive encounters). In each case, we tested hypotheses of change in both attitudes and trust amongst White female South African participants towards Black (African) and Coloured South Africans (the primary and secondary outgroups, respectively) in relation to the baseline (pretest) and compared with the control condition.

1.6 | Hypotheses

Hypothesis 1. *Primary Transfer Effects. Direct or vicarious positive contact with a primary outgroup confederate, a female*

Black (African) South African (hereafter simply referred to as the intervention) will significantly improve attitudes and trust towards Black (African) South Africans in general (hereafter termed, the primary outgroup), both immediately after the intervention (immediate-posttest; H1a) and one week later (delayed-posttest; H1b).

Hypothesis 2. *Secondary Transfer Effects involving Attitude and Trust Generalisation. The intervention will significantly improve attitudes and trust towards Coloured South Africans (hereafter termed the secondary outgroup), via the improvement in attitudes and trust (respectively) towards the primary outgroup both immediately after the intervention (immediate-posttest; H2a) and one week later (delayed-posttest; H2b).*

Hypothesis 3. *Secondary Transfer Effects involving Attitude-to-Trust Generalisation. To undertake a stricter test of the STE, we investigated whether the key variable specified in the attitude generalisation account (i.e., change in attitude towards the primary outgroup) led to change in a different measure of secondary outgroup affect (trust). We hypothesized that the intervention would significantly improve trust towards the secondary outgroup, via improved attitudes towards the primary outgroup both immediately after the intervention (immediate-posttest; H3a) and one week later (delayed-posttest; H3b).*

2 | Method

2.1 | Background and Design

Given the historical context of South African intergroup relations, and that White South African students comprise the numerical majority (and Black (African) and Coloured South African students the numerical minority) at Stellenbosch University, we tested the STE amongst White South African students in relation to the Black (African) and Coloured South African outgroups. We estimated that we needed 35 participants per condition (i.e., 105 participants in total) to detect a medium effect size ($\eta_p^2 = 0.06$) with 80% power at alpha = 0.05 in a 3 (Condition: Direct vs. Vicarious contact vs. Control) by 3 (Phases: Time 1 vs. Time 2 vs. Time 3), mixed-model design, with repeated measures on the second factor (measures completed: 1 week before the intervention; immediately after the intervention; and 1 week after the intervention; for an analysis of clustering by friendship pairs, see Supporting Information: SOM, Section 3).

The initial data collection in 2011 left the study significantly underpowered and in 2019 we used an identical design and procedure to increase the sample (see Supporting Information: SOM, Section S2 for details about the 2011 and 2019 samples). The only difference in the procedures in 2011 and 2019 is that, by necessity, we could not use the same confederate in 2019 that we had used in 2011. Nevertheless, our confederate-recruitment procedures were identical in 2011 and 2019, designed to ensure that participants in the direct and vicarious contact conditions would almost certainly identify the confederate as a Black (African) South African and not as a member of another group. Ethics and institutional approvals were obtained from Stellenbosch University (2011 Project Number: 521/2011; 2019 Project Number: 8840).

2.2 | Participants and Procedures

White South African female students at Stellenbosch University were recruited in friendship pairs via in-class announcements during undergraduate Psychology lectures (comprising a majority of female students). The study was introduced to prospective participants, who were informed of their rights (e.g., voluntary participation, the right to withdraw from the study at any point), as a study on friendship formation. Prospective participants completed an intake interview where they indicated their age and gender and confirmed their ethnicity.

In 2011, we recruited 66 participants, 10 of whom were excluded because they reported their ethnicity as Coloured South African during their intake interview. Two participants in the vicarious contact condition were excluded from the analyses because they misidentified the confederate's ethnicity, resulting in a final 2011 sample of 54 participants. In 2019, we recruited 128 female White South African participants, of whom only 62 formally registered for the study and attended the lab session. Combining the 2011 and 2019 samples resulted in a final sample of 116 White female South African students ($M_{age} = 19.84$; $SD_{age} = 2.42$).

All participants completed the pretest (Time 1; T1) survey. Friendship pairs were randomly assigned to either the experimental or the control group ($N_{control\ (no\ contact)} = 32$). Participants in the pairs assigned to the experimental group were further randomly assigned to either the direct ($N_{direct\ contact} = 44$) or the vicarious ($N_{vicarious\ contact} = 40$) contact condition. A post hoc power analysis for a repeated measures analysis of variance (ANOVA) with a within-between interaction (two groups and three measurement occasions) confirmed that our combined sample was sufficiently powered to detect a medium effect size ($\eta_p^2 = 0.06$) at 85% power and alpha = 0.05 (see Supporting Information: SOM, Section S4).

One week after completing the pretest survey, participants in the control condition completed the second survey, while participants in the experimental group first participated in the intervention (direct or vicarious contact) before completing the second (immediate-posttest; T2) survey. For participants in the experimental group, this survey included a series of questions that assessed their (direct or vicarious) experience of the interaction task with the confederate. One week later, all participants completed the third survey (delayed-posttest; T3). Each survey took approximately 10 min to complete. Below, we report analyses on the combined sample, after first testing for any effects of the sub-samples (i.e., year-of-study) on the key results.

2.3 | Manipulation of Direct and Vicarious Contact

We manipulated direct and vicarious contact at T2 (following the method used by Ioannou et al. 2018), with a female Black (African) South African confederate who was introduced as a fellow participant. Participants in the direct contact condition

completed the Relationship Closeness Induction Task (RCIT; adapted from Sedikides et al. 1999) with the confederate. The RCIT is a nine-minute activity that facilitates reciprocal self-disclosure within an interaction that is designed to be both positive and intimate (see Supporting Information: SOM, Section S1). In the direct contact condition, the participant and the confederate were informed that they would be engaging in a structured conversation with one another that would help them to get to know each other better; unbeknownst to the participant in this condition, the participant in the vicarious contact condition watched the interaction over closed-circuit video link from an adjoining room.

Prior to the start of the study, the confederate was instructed to (i) introduce themself to the participant using indigenous names associated with their ethno-linguistic group, thereby underlining their ethnic identity (the confederate used a different indigenous name for each lab session); (ii) include mention of the ethno-linguistic group they belonged to (Xhosa) during the course of the RCIT; and (iii) mention that they lived in the local township of Kayamandi, Stellenbosch (a neighbourhood that is comprised of more than 90% Black (African) South African residents). These instructions were designed to firmly establish the ethnic identity of the confederate as Black (African) South African, as opposed to Coloured South African. For example, Coloured South Africans generally do not identify as belonging to one of the indigenous ethno-linguistic groups in South Africa (and generally have either English or Afrikaans, and not isiXhosa, as their home language). Moreover, only a very small percentage of Coloured South Africans reside in the township the confederate identified themselves as living in.

After completing the immediate-posttest survey, participants in the experimental conditions were informed that the friend with whom they had attended the lab session (and from whom they had been separated at the start of the lab session) had observed the RCIT interaction via closed-circuit video link. However, it was not revealed at this stage that the Black (African) interaction partner was a confederate. During this initial debrief, no participants identified the true hypotheses of the study, nor did any exercise their right to withdraw their participation. After completing the delayed-posttest survey 1 week later, all participants received an email with a full debrief of the study, its aims and hypotheses, and the disclosure that the interaction partner in the RCIT had been a confederate. No participants exercised their right to withdraw their participation after this final debrief.

2.4 | Measures

Participants answered different sets of questions at pretest, immediate-posttest, and delayed-posttest, as follows:

2.4.1 | Pretest Questionnaire

2.4.1.1 | Demographic Variables. Participants indicated their age and gender.

2.4.1.2 | Intergroup Contact. Participants answered four items per outgroup (adapted from Swart, Hewstone, Christ, and Voci 2011), 'How many of your friends at Stellenbosch University are [Black (African) South Africans/Coloured South Africans]' (0 = *None*; 6 = *All*). Participants then answered three items reporting how often they spent time with friend(s) from each outgroup in their residence, in the friends' residence, and socially (0 = *Never*; 6 = *Daily*). These items formed a reliable four-item scale (McDonald's ω for Black (African) South Africans: $\omega = 0.82$; McDonald's ω for Coloured South Africans: $\omega = 0.79$).

2.4.1.3 | Extended Contact. Participants answered two items per outgroup (adapted from Wright et al. 1997), 'How many of your close White South African friends have friends who are [Black (African) South Africans/Coloured South Africans]' and 'How many of your family members have friends who are [Black (African) South Africans/Coloured South Africans]' (0 = *None*; 6 = *All*). We calculated the Spearman-Brown coefficient (see Eisinga et al. 2013). These two items formed a reliable measure of extended contact (Black (African) South Africans: $\rho = 0.80$; Coloured South Africans: $\rho = 0.73$).

2.4.2 | Immediate-Posttest Questionnaire

To assess the nature of the interaction task with the confederate, for those who participated in (direct contact) or observed it (vicarious contact), we asked the following three items:

2.4.2.1 | Task Success. Participants indicated whether, 'The conversation you [just had/just observed] in the task is a good way to get to know somebody' (1 = *Completely disagree*; 5 = *Completely agree*).

2.4.2.2 | Reciprocal Self-Disclosure. Participants answered, 'When you think about the conversation you [just had/just observed] was it of a personal/private nature?' (1 = *Not at all personal/private*; 5 = *Extremely personal/private*).

2.4.2.3 | Attitude Towards the Confederate. Participants rated their attitudes towards the confederate they interacted with/observed using a feeling thermometer (0°–100°; Converse and Presser 1986; Lolliot et al. 2015). Higher scores represent more favorable attitudes.

2.4.3 | Pretest, Immediate-Posttest, and Delayed-Posttest Questionnaires

2.4.3.1 | Attitudes Towards the Primary and Secondary Outgroups. Participants rated their attitudes towards Black (African) and Coloured South Africans on feeling thermometers (0°–100°; Converse and Presser 1986; Lolliot et al. 2015). Higher scores represent more favourable attitudes.

2.4.3.2 | Intergroup Trust. Participants answered three items concerning intergroup trust per outgroup (adapted to the South African context based on Brehm and Rahn 1997; Tam et al. 2009): 'I can't trust [Black (African)/Coloured] South Africans', 'I cannot rely on [Black (African)/Coloured] South Africans to look out for my best interest as a White South African', and 'I am often suspicious when I am in the company of [Black

(African)/Coloured South Africans]' (1 = *Strongly disagree*; 7 = *Strongly agree*). These items were reverse-scored so that higher scores indicate greater outgroup trust. These items formed reliable scales at each time point (McDonald's ω for Black (African) South Africans: $\omega_{T1} = 0.83$, $\omega_{T2} = 0.87$, $\omega_{T3} = 0.90$; McDonald's ω for Coloured South Africans: $\omega_{T1} = 0.85$, $\omega_{T2} = 0.85$, $\omega_{T3} = 0.90$).

3 | Results

3.1 | Preliminary Analyses

All analyses were run in *R* (R Core Team 2024). All (multivariate) analyses of variance ((M)ANOVAs) were run in *jmv* (Selker et al. 2023), and all mediation analyses were run in *lavaan* (Roseel 2012).

3.1.1 | Differences by Sample

To investigate if there were any systematic differences in our key variables at pretest (primary and secondary outgroup direct contact, extended contact, attitudes, and trust), immediate-posttest (primary and secondary outgroup attitudes and trust), and delayed-posttest (primary and secondary outgroup attitudes and trust) by sample (year-of-study), we conducted three 2 (Sample: 2011 vs. 2019) by 3 (Condition: direct contact vs. extended contact vs. control) between-subjects MANOVAs (see Supporting Information: SOM, Section 2 for the full analysis).

3.1.1.1 | Differences by Sample at Pretest. At pretest, there was a significant main effect of sample, Pillai's Trace $V = 0.21$, $F(8, 79) = 2.60$, $p = 0.014$. Participants in 2019 reported more primary outgroup, Black (African) South African, friends ($M = 1.73$, SD = 1.40), but less extended contact with the secondary outgroup, Coloured South Africans ($M = 2.06$, SD = 1.35), than those in 2011 ($M_{\text{primary outgroup direct contact}} = 0.94$, SD = 1.14, $p = 0.007$, Cohen's $d = 0.62$; $M_{\text{secondary outgroup extended contact}} = 2.79$, SD = 1.44, $p = 0.008$, Cohen's $d = -0.52$). Importantly, the Sample × Condition interaction was non-significant ($p = 0.464$).

3.1.1.2 | Differences by Sample at Immediate-Posttest. The 2 (Sample) by 3 (Condition) MANOVA returned a main effect of sample, Pillai's Trace $V = 0.09$, $F(4, 107) = 2.57$, $p = 0.0.42$. Participants in the 2011 sample held more trusting attitudes towards the primary ($M = 5.26$, SD = 1.29) and secondary outgroup ($M = 5.52$, SD = 1.26) and more favourable attitudes towards the secondary outgroup ($M = 71.54$, SD = 14.35) than did participants in the 2019 sample ($M_{\text{primary outgroup trust}} = 4.71$, SD = 1.43, $p = 0.014$, Cohen's $d = -0.40$; $M_{\text{secondary outgroup trust}} = 4.97$, SD = 1.31, $p = 0.008$, Cohen's $d = -0.43$; $M_{\text{secondary outgroup attitudes}} = 66.85$, SD = 16.48, $p = 0.042$, Cohen's $d = -0.30$). There was no significant Sample × Condition interaction ($p = 0.56$).

3.1.1.3 | Differences by Delayed-Posttest. For the delayed-posttest dependent variables, there was a significant Sample × Condition interaction, Pillai's Trace $V = 0.18$, $F(8, 202) = 2.45$, $p = 0.014$. None of the follow-up univariate tests in both the main effect and interaction reached statistical significance ($0.052 < p < 0.994$).

3.1.1.4 | Differences by Sample in the Experience of the Intervention. We ran a 2 (Sample: 2011 vs. 2019) by 2 (Condition: direct contact versus vicarious contact) MANOVA on task success, reciprocal self-disclosure, and attitudes towards the confederate. There was a main effect for sample, Pillai's Trace $V = 0.35$, $F(3, 79) = 13.97$, $p < 0.001$. Participants in the 2019 sample reported greater task success ($M = 4.60$, SD = 0.53), disclosed more personal information ($M = 3.52$, SD = 0.61), and liked the confederate more ($M = 89.50$, SD = 8.25) compared to the 2011 sample ($M_{\text{task success}} = 4.31$, SD = 0.58, $p = 0.013$, Cohen's $d = 0.51$; $M_{\text{self-disclosure}} = 2.63$, SD = 1.11, $p < 0.001$, Cohen's $d = 1.04$; $M_{\text{attitudes towards confederate}} = 80.23$, SD = 11.14, $p < 0.001$, Cohen's $d = 0.97$).

Mean differences, such as those reported above, do not necessarily indicate that the underlying variances and covariances of constructs were unaffected (e.g., Goodman and Blum 1996). As such, we ran all the main repeated measures ANOVAs reported below to see if the samples (i.e., year-of-study) moderated any of the critical paths; we found no moderation effects indicating that the underlying pattern of relationships did not differ by sample (see Supporting Information: SOM, Section S2 for further details).

The differences between the 2011 and 2019 samples on the key variables were minimal and unsystematic, did not interact with condition, and did not impact the underlying relationships that were tested. As such, we combined the two samples to increase power. However, because the experimental manipulation was experienced as more positive in the 2019 sample, we included 'sample' (coded −1 = 2011, +1 = 2019) as a covariate in all analyses. A detailed analysis of sample differences is available in the Supporting Information.

3.1.2 | Tests of Random Assignment

To test if our random assignment across conditions was successful, we conducted a one-factor (Condition: Direct contact vs. Vicarious contact vs. Control) MANOVA with participant age, prior direct and extended contact with Black (African) and Coloured South Africans, as well as pretest measures of trust and attitudes towards Black (African) and Coloured South Africans as dependent variables. There was no significant multivariate effect, Pillai's Trace $V = 0.19$, $F(18, 164) = 0.98$, $p = 0.485$, indicating that random assignment was successful. See Table S6 (Supporting Information: SOM, Section S6) for the means and standard deviations of the pre-test variables by condition.

3.1.3 | Experience of the Intervention

A one-factor MANOVA (Condition: Direct contact vs. Vicarious contact), with task success, self-disclosure, and attitudes towards the confederate as dependent variables was run to confirm that participants in the direct and vicarious contact conditions did not differ significantly along task-related measures. This analysis returned a non-significant multivariate effect, Pillai's

Trace $V = 0.06$, $F(3, 81) = 1.66$, $p = 0.183$. This non-significant difference in scores between the two experimental conditions, together with the size of the combined mean scores across direct and vicarious contact conditions for task success ($M = 4.48$, SD = 0.57), self-disclosure ($M = 3.15$, SD = 0.96), and attitudes towards the confederate ($M = 85.68$, SD = 10.53), confirmed that participants in the experimental conditions experienced the intervention similarly and positively.

3.2 | Main Analyses

3.2.1 | Testing for Primary Transfer Effects (Hypothesis 1)

The means and standard errors for attitudes and trust towards Black (African) South Africans for each condition at pretest, immediate-posttest, and delayed-posttest are shown in Table 1 below (which also reports comparison between experimental and control conditions). We first tested for a PTE in relation to attitudes towards the primary outgroup. There were no main effects for condition, $F(2, 106) = 2.13$, $p = 0.124$, $\eta_p^2 = 0.04$, or time of measurement, $F(2, 212) = 2.50$, $p = 0.084$, $\eta_p^2 = 0.02$. The hypothesized Time × Condition interaction was significant, $F(4, 212) = 2.77$, $p = 0.028$, $\eta_p^2 = 0.05$.

Attitudes towards Black (African) South Africans improved from pretest to immediate-posttest measures in both the direct (mean difference = 9.04, SE = 2.10, $p < 0.001$, Cohen's $d = 0.64$) and vicarious (mean difference = 7.16, SE = 2.31, $p = 0.002$, Cohen's $d = 0.42$) contact conditions (but not in the control condition: mean difference = 0.56, SE = 2.52, $p = 0.824$, Cohen's $d = 0.15$), supporting H1a. At immediate-posttest, participants in the direct contact condition expressed significantly more favourable attitudes towards the primary outgroup than did participants in the control condition (mean difference = 11.80, SE = 4.18, $p = 0.006$, Cohen's $d = 0.66$). Attitudes towards the primary outgroup at immediate-posttest were higher (but not significantly so) in the vicarious contact condition compared to the control condition (mean difference = 7.97, SE = 4.37, $p = 0.071$, Cohen's $d = 0.42$). These results provide partial support for H1a. At immediate-posttest, attitudes towards the primary outgroup in the direct and vicarious contact conditions did not differ significantly (mean difference = 3.83, SE = 3.91, $p = 0.329$, Cohen's $d = 0.27$).

TABLE 1 | Means and standard errors for attitudes and trust towards primary outgroup (Black (African) South Africans) at Time 1 (pretest), Time 2 (immediate-posttest), and Time 3 (delayed-posttest).

Variable	Condition	Time 1	Time 2	Time 3
Attitudes towards primary outgroup: Black (African) South Africans	Direct Contact	$63.16_{a,1}$ (2.64)	$72.20_{b,2}$ (2.67)	$68.91_{b,1,2}$ (2.62)
	Vicarious Contact	$61.21_{a,1}$ (2.89)	$68.37_{b,1,2}$ (2.92)	$70.21_{b,2}$ (2.87)
	Control	$59.84_{a,1}$ (3.16)	$60.40_{a,1}$ (3.19)	$61.44_{a,1}$ (3.14)
Trust towards primary outgroup: Black (African) South Africans	Direct Contact	4.54 (0.20)	5.09 (0.21)	4.94 (0.22)
	Vicarious Contact	4.86 (0.22)	5.17 (0.23)	5.26 (0.25)
	Control	4.48 (0.24)	4.62 (0.25)	4.47 (0.27)

Note: Means that do not share an alphabetical subscript within a row or a numerical subscript within a column are significantly different at $p < 0.05$. Standard errors are reported in parentheses.

Compared to pretest attitudes, attitudes towards the primary outgroup, Black (African) South Africans, remained significantly higher at delayed-posttest in both the direct (mean difference = 5.75, SE = 2.03, $p = 0.006$, Cohen's $d = 0.41$) and vicarious (mean difference = 9.00, SE = 2.22, $p < 0.001$, Cohen's $d = 0.74$) contact conditions (but not in the control condition: mean difference = 1.60, SE = 2.43, $p = 0.511$, Cohen's $d = 0.12$), supporting H1b. At delayed-posttest, attitudes towards the primary outgroup were more positive (but not significantly so) in the direct contact condition compared to the control condition (mean difference = 7.47, SE = 4.11, $p = 0.072$, Cohen's $d = 0.43$). Primary outgroup attitudes were significantly more favourable at delayed-posttest in the vicarious contact condition compared to the control group (mean difference = 8.77, SE = 4.30, $p = 0.044$, Cohen's $d = 0.53$). These results provide partial support for H1b. Attitudes towards the primary outgroup in the two experimental conditions did not differ from each other at delayed-posttest (mean difference = 1.30, SE = 3.85, $p = 0.736$, Cohen's $d = 0.09$).

We then tested for a PTE in relation to trust towards the primary outgroup. There were no main effects for condition, $F(2, 103) = 1.54$, $p = 0.219$, $\eta_p^2 = 0.03$, or time, $F(2, 206) = 1.51$, $p = 0.223$, $\eta_p^2 = 0.01$, and the Time × Condition interaction was non-significant, $F(4, 206) = 1.75$, $p = 0.140$, $\eta_p^2 = 0.03$. Thus, we did not detect a PTE in relation to trust. Given the absence of a PTE for primary outgroup trust, there could not be, by definition, an STE involving trust generalisation across outgroups (which requires primary outgroup trust to serve as the mediating mechanism between the intervention and secondary outgroup trust). Thus, we did not run the analyses to test for an STE involving trust generalisation across outgroups and we only report below on the analyses we ran to test for STEs characterised by attitude generalisation and STEs involving attitude-to-trust generalisation.

3.2.2 | Testing for Secondary Transfer Effects

We next tested for the presence of secondary transfer effects involving (i) attitude generalisation (Hypothesis 2) and (ii) attitude-to-trust generalisation (Hypothesis 3). Adopting Pettigrew's (2009) definition of the STE, we tested whether changes in primary outgroup attitudes were associated with changes in secondary outgroup attitudes (or trust) both immediately after the intervention (i.e., from T1 to T2) and a week later (i.e., from T1 to T3). We tested for mediation using bootstrapping with bias-corrected confidence intervals and 10,000 resamples (see Table S7, SOM, Supporting Information: Section S7 for correlation tables, as well as information on the effect size of the STE and achieved power).

We ran two mediation analyses. The first analysis treated change in attitudes towards Black (African) South Africans

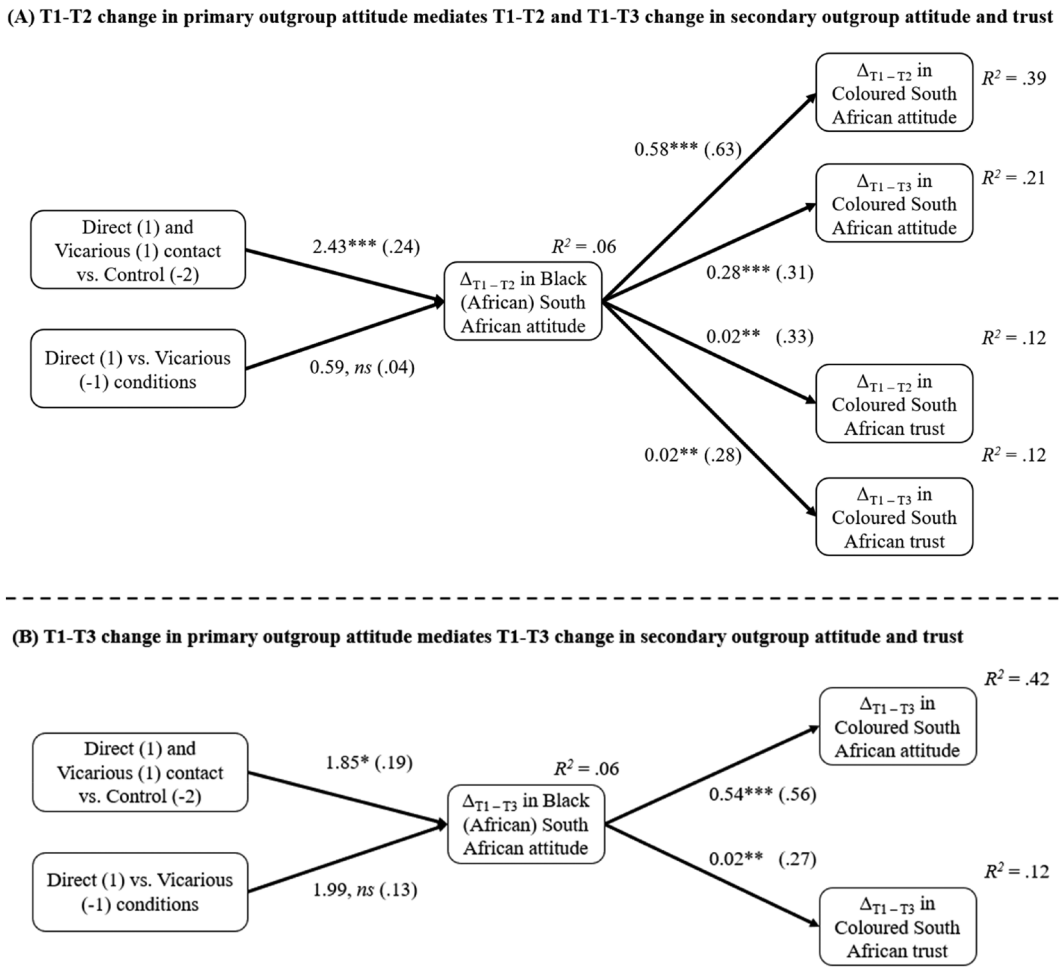

FIGURE 1 | Two mediation models showing the STE across time treating change in attitudes and trust towards Coloured South Africans as the outcome variables. We report unstandardized regression weights (standardized regression weights in parentheses). T1 = pretest, T2 = immediate-posttest, T3 = delayed-posttest. $^{**}p < 0.01$, $^{***}p < 0.001$.

from T1 to T2 as the mediator between the experimental intervention and T1-T2 and T1-T3 outcome variables (see Figure 1, Panel A). The second analysis treated T1-T3 change in attitudes towards Black (African) South Africans as the mediator between the experimental intervention and T1-T3 outcomes (see Figure 1, Panel B).

We created two orthogonal contrast codes to represent the intervention conditions. The first contrast code compared participants in the direct and vicarious contact conditions, both coded as 1, against those in the control condition, coded as −2. The second contrast code compared participants in the direct contact condition, coded as 1, to those in the vicarious contact condition, coded as −1, with the participants in the control group coded as 0.

For the mediation model treating changes in T1-T2 attitudes towards Black (African) South Africans as the mediator (see Figure 1, Panel A), constraining paths from the mediator to the respective outcome variables (i.e., secondary outgroup attitudes and trust towards Coloured South Africans) to equivalence resulted in a model with significantly poorer fit than the fully unconstrained Model, $\Delta x^2(2) = 24.55$, $p < 0.001$. Freeing the equality constraint between the paths from the mediator to changes in T1-T2 and T1-T3 secondary outgroup attitudes, while leaving the paths from the mediator to changes in T1-T2 and T1-T3 secondary outgroup trust constrained to equality, resulted in a model with equivalent fit to the unconstrained model, $\Delta x^2(1) = 2.81$, $p = 0.094$.

The mediation analyses testing for STEs involving attitude generalisation showed that T1-T2 change in primary outgroup attitudes mediated the relationship between the intervention and T1-T2 change in secondary outgroup attitudes (PE = 1.40, 95% CI [0.51, 2.51]; $\beta = 0.15$), supporting H2a (Figure 1, Panel A). Furthermore, T1-T2 change in primary outgroup attitudes mediated the relationship between the contact intervention and T1–T3 change in secondary outgroup attitudes (PE = 0.69, 95% CI [0.21, 1.45]; $\beta = 0.07$), supporting H2b (Figure 1, Panel A). Finally, T1-T3 change in primary outgroup attitudes mediated the relationship between the intervention and T1–T3 change in secondary outgroup attitudes (PE = 0.99, 95% CI [0.09, 2.11]; $\beta = 0.11$), further supporting H2b (Figure 1, Panel B).

The mediation analyses testing for STEs involving attitude-to-trust generalisation (i.e., the generalisation from primary outgroup attitudes to secondary outgroup trust), which is arguably a stricter test of the STE (because it rules out potential shared

method variance as an explanation for the STE), showed that T1-T2 change in primary outgroup attitudes mediated the relationship between the intervention and both T1-T2 and T1-T3 change in secondary outgroup trust (PE = 0.05, 95% CI [0.01, 0.12]; $\beta = 0.08$), supporting H3a (see Figure 1, Panel B). We also found that T1-T3 change in primary outgroup attitudes mediated the relationship between the intervention and T1-T3 change in secondary outgroup trust (PE = 0.04, 95% CI [0.01, 0.10], $\beta = 0.05$), supporting H3b (see Figure 1, Panel B). Thus, our analyses confirmed the presence of STEs involving attitude generalisation and STEs involving attitude-to-trust generalisation. We did not, however, find evidence of STEs involving trust generalisation, due to the absence of a PTE for trust.

4 | Discussion

We investigated the secondary transfer effect (STE) of contact amongst female White South African participants, testing the causal relationship between primary outgroup contact and secondary outgroup attitudes and trust over time. Moreover, our study investigated the STE of both direct and indirect contact simultaneously under identical laboratory-experimental conditions. Our robust methodology also included a pretest-posttest design (including both immediate- and delayed-posttest) with a control group, and we tested for effective randomization across our three conditions. We showed, first, that a brief (structured) positive interaction with a female Black (African) South African confederate (primary outgroup member) led to a primary transfer effect (PTE) for attitudes (but not trust) amongst White South African participants in both the direct and vicarious contact conditions immediately after the intervention. These effects persisted 1 week later (supporting H1a and H1b for attitudes, but not trust), with no movement in attitudes in the control condition. Second, we demonstrated STEs of small-to-medium effect sizes (Kenny 2024) following both direct and vicarious contact, and at immediate- and delayed-posttest via primary outgroup attitudes (supporting H2a and H2b for attitudes, but not trust) and using distinct measures of primary and secondary outgroup affect (supporting H3a and H3b). These results derive, moreover, from the first study of the STE of intergroup contact from the African continent, addressing a gap highlighted in a review of the STE literature by Vezzali et al. (2021). Below, we discuss the STEs observed for direct and vicarious contact, especially the contrasting results for outgroup attitudes and outgroup trust. We conclude by reviewing four important limitations of our study that temper the broader generalisation of our results.

4.1 | The Effects of Direct and Vicarious Contact on STEs

The effect sizes of our PTEs (which were mostly small-to-medium in size; Kenny 2024) were comparable to those found in meta-analyses on intergroup contact (e.g., Paluck et al. 2019; Pettigrew and Tropp 2006). We also found a PTE for attitudes in both the direct and vicarious contact conditions immediately after the intervention, but the effect was larger for the direct compared to the vicarious contact condition when considering attitude change from pretest to immediate-posttest (see also Ioannou et al. 2018, Study 1). However, the delayed-posttest measure yielded a reversed pattern of results: there was greater change in primary outgroup attitudes in the vicarious compared to the direct contact condition. For STEs, however, we found that we could constrain these paths to equivalence (i.e., any differences between conditions were statistically equivalent).

We found consistent evidence for STEs involving the generalisation of attitudes across outgroups in both the direct and vicarious contact conditions immediately after the intervention. Encouragingly, these results persisted 1 week after the intervention, exceeding the standard (of 1 day) recommended by Paluck et al. (2019) for studies testing contact interventions (a key point to which we return below). These results contribute to the small (but growing) body of experimental support for Pettigrew's (2009) original definition of the STE, whereby primary outgroup attitudes mediate the relationship between contact with the primary outgroup (the intervention) and secondary outgroup attitudes.

Notwithstanding some evidence in our results that suggests a stronger STE, especially over time, after vicarious contact, we emphasise that this cannot be a definitive comparison between the two forms of contact. Our experiment used only one operationalization to *represent* each form of contact, and vicarious contact, specifically, was operationalised as observing a close ingroup friend interacting with the outgroup member. We did this, in part, to ensure that the ingroup member was seen as a credible source of (indirect) influence (e.g., Sechrist and Milford-Szafran 2011). We set up the interaction to be cooperative and positive, which it was, as shown by the findings that participants rated both the interaction and the confederate very positively. Here we were guided by an ethical duty (especially sensitive in post-conflict South Africa) to make the interracial interaction go well, because of the potential implications of a negative interaction beyond the study's confines. Recent studies comparing the impact of positive versus negative contact have highlighted that even neutral, non-positive contact may be perceived as negative, simply because it is distinct from the normally somewhat positively toned nature of social interactions (see Schäfer et al. 2025), hence the need for the interaction to be unambiguously positive. Nonetheless, further research is required, using multiple operationalisations and variations of the valence of contact that can be compared, as in our case, within the same paradigm.

Taken together, it is encouraging that both direct and vicarious forms of contact yielded STEs involving attitude generalisation, because in some circumstances we might want to exploit known advantages of vicarious contact, or it might be preferred as a milder, safer option. We know, moreover, that indirect contact in general, and vicarious contact specifically, may be useful in preparing people for later direct contact (i.e., as a pre-contact tool; e.g., Mazziotta et al. 2011; Turner et al. 2007; Wölfer et al. 2019), especially in post-conflict societies like South Africa. Finally, it is worth noting once again that in contrast to research on vicarious contact where participants typically observe intergroup interactions between a fellow ingroup member (who is a stranger to them) and an outgroup member (e.g., Mazziotta et al. 2011), participants in the vicarious contact condition in our study observed an *ingroup friend* engaging in positive intergroup contact. This is an important distinction considering the experimental research reported by Sechrist and Milford-Szafran (2011). They

showed that information, or feedback, shared between interdependent partners (e.g., friends) tends to influence racial attitudes significantly more than information, or feedback, shared between independent partners (i.e., strangers). Thus, it is likely that observing an ingroup friend is likely to yield stronger PTEs and STEs for vicarious contact relative to observing interactions between an ingroup stranger (or an ingroup acquaintance) and the outgroup. Testing whether relationship closeness (between ingroup actor and ingroup observer) moderates the strength of STEs for vicarious contact is an important avenue for future research.

Whereas we observed both a PTE and STE for outgroup attitudes, we observed neither a PTE for trust nor any STEs involving trust generalisation at either immediate-posttest or delayed-posttest (1 week later). These results are not wholly surprising. Indeed, we anticipated that it may be more challenging for even a single positive intergroup encounter to impact outgroup trust relative to outgroup attitudes (Tam et al. 2009). Nevertheless, it is important to keep in mind that the absence of a significant *p*-value does not necessarily indicate the absence of an effect (Bonovas and Piovani 2023). While our study was adequately powered to detect medium-sized effects, we were underpowered to detect small-to-medium effects.

Outgroup trust may be more difficult to achieve than outgroup liking because building trust often requires multiple positive encounters, or 'trustworthy' behaviours, and only a single 'untrustworthy' act or betrayal to arouse distrust that is very resistant to change (Rothbart and Park 1986). Our single-contact encounter with the participant (lasting only nine minutes) offered the outgroup confederate limited opportunities to demonstrate the kind of trustworthy behaviours that could sufficiently shift the outgroup trust felt by the direct or vicarious contact participants towards Black (African) South Africans more generally. This would explain the lack of a PTE for outgroup trust in either contact condition and, by definition, the absence of a PTE for trust rules out the possibility of trust generalisation across outgroups (i.e., an STE involving trust generalisation).

However, we did observe evidence of STEs involving attitude-to-trust generalisation; direct and vicarious contact with the confederate were significantly associated with secondary outgroup trust indirectly, via changes in primary outgroup attitudes. Changes in primary outgroup attitudes from Time 1 to Time 2 mediated the relationship between direct and vicarious contact and changes in secondary outgroup trust measured both from Time 1 to Time 2 and from Time 1 to Time 3. Similarly, changes in primary outgroup attitudes from Time 1 to Time 3 mediated the relationship between direct and vicarious contact and changes in secondary outgroup trust from Time 1 to Time 3. These findings are encouraging for two reasons. First, we observed the STEs involving attitude-to-trust generalisation both immediately after the intervention and *7 days* after the intervention. Thus, we demonstrate that even a single positive intergroup interaction has the potential to promote trust towards a secondary outgroup (albeit indirectly, via improved attitudes towards the primary outgroup) and that this effect appears strong enough to persist well beyond the intergroup encounter. Second, this stricter test of the STE allows us to rule out potential shared method variance by using distinct measures for primary versus secondary outgroup orientation (i.e., attitudes and trust, respectively; see Podsakoff et al. 2003; Tausch et al. 2010).

Our research adds to the limited literature on the STE of trust by identifying an additional way in which secondary outgroup trust might be impacted by contact with the primary outgroup. While previous STE research has reported that primary outgroup trust mediated the relationship between primary outgroup contact and primary outgroup attitudes (Zezelj et al. 2020), this conclusion is overstated given the cross-sectional design of the study. Our results allow us to cautiously suggest that within the post-conflict South African context, promoting more positive primary outgroup attitudes might offer a means of encouraging greater secondary outgroup trust in the absence of (or presence of limited) secondary outgroup contact. Our findings are, however, unable to shed light on just how long more positive primary outgroup attitudes (after positive primary outgroup contact) can sustain secondary outgroup trust in the absence of positive contact with the secondary outgroup itself. Nevertheless, these findings add yet further support to the potential broader benefits of promoting positive intergroup contact with acquaintance potential (Cook 1962). Not only can such contact promote more positive attitudes towards the primary outgroup (i.e., the outgroup whose members are encountered), but it has the potential to promote more positive attitudes and trust towards secondary (unencountered) outgroups.

A key contribution of our research is that we tested for the STE of both direct and vicarious contact at two time points—immediately after the experimental manipulation and then again 1 week later. In Paluck et al.'s (2019) assessment of the potential policy impact of contact interventions, they highlighted studies that measured outcomes at least 1 day after the intervention began. We used a more demanding criterion, with a delayed-posttest measurement at *7 days* after the intervention. The elapsed time is important because, as Pettigrew (2009) notes, there are likely similarities between the processes underlying the STE and other effects for which there is a time delay (e.g., attitude change towards logically connected attitudes; Watts and Holt 1970). This suggests that the STE might, in some instances, require time to manifest. We did not find a statistically stronger STE at delayed versus immediate-posttest; in fact, we found the opposite. One explanation for why elapsed time was not associated with a greater effect 7 days later might be that our intervention included only a single positive intergroup encounter. Repeated positive intergroup encounters are more likely to strengthen STEs over time than are one-off encounters. Our results notwithstanding, it would not be surprising if, in some cases, the change involved in the secondary transfer (e.g., between two very different primary and secondary outgroups) initially evoked strong cognitive dissonance and that, in Pettigrew's words, 'it takes time for the inconsistency 'to sink in' before the transfer effect is established' (Pettigrew 2009, 62).

4.2 | Limitations

We acknowledge four limitations of our study that warrant caution in the interpretation of the results. First, the laboratory experiment we conducted may seem to use a simplified and rather barren, even contrived, setting. We adopted this experimental paradigm to test the STE of direct and vicarious contact

simultaneously under identical conditions, allowing us to achieve high internal validity in our demonstration of causality between primary outgroup contact and secondary outgroup outcomes. However, we would like to emphasize the value of both more comprehensive, multi-wave longitudinal research (e.g., van Laar et al. 2005) and highly controlled laboratory and field experimental methods to better understand STEs and their dynamics.

Second, and relatedly, our single, nine-minute interaction with an outgroup confederate does not adequately reflect the variety of interactions that people might have with the outgroup outside of such a carefully controlled laboratory environment. As such, future research should investigate whether a longer, richer experience of contact, or perhaps a sequence of more demanding interactions with more than one outgroup member, can produce STEs. This would be an especially important consideration for testing PTEs and STEs as they relate to primary and secondary outgroup trust, respectively.

Third, given the limitations of the relatively simple experimental design we implemented (as highlighted above), together with the fact that we included a limited range of measures for testing the STE, we cannot discount the possibility that there are alternative explanations that might account for our results. An anonymous reviewer suggested that improvements in mood following direct (or vicarious) contact with the confederate might offer one alternative explanation for our results. We provide statistical tests of this alternative account (see Supporting Information: SOM Section S8) and while we cannot completely rule out that participant mood is a mechanism by which the intervention affected attitudes towards the outgroups, an analysis of the variances across time points and conditions shows the pattern of variances is inconsistent with predictions derived from a mood-based account. A second suggested alternative explanation was that the intervention might have made salient participants' own positive experiences with the secondary outgroup, thus explaining the observed STE. Two robustness checks help rule out this alternative account (see Supporting Information: SOM, Section 8). Indeed, these robustness checks suggest that the intervention was most successful for those participants reporting *fewer* outgroup contact experiences (see the meta-analysis by Van Assche et al. 2023 who report similar findings). Adding direct and extended contact with the secondary outgroup as covariates in the mediation analyses did not result in any significant changes to findings and our conclusions in support of the STE remain unchanged.

Fourth, our convenience sample of female White South Africans is neither representative of that subgroup of South Africans, nor representative of the diverse populations to be found in South Africa. As encouraging as our findings are, they cannot be generalised beyond this specific study and sample. Thus, it would be important for the pattern of results we observed to be replicated amongst other populations, with different primary and secondary outgroup combinations, to determine just how robust these findings are.

5 | Conclusion

Pettigrew (1998) stated that generalisation from the immediate (contacted) outgroup to other (noncontacted) outgroups was a 'higher-order form of generalization' (75) that was 'seldom studied because many regard it as highly unlikely' (75). Vezzali et al.'s (2021) review of the now numerous studies confirms that the STE is not unlikely. There are, however, methodological weaknesses with many of the studies and a laboratory experiment is best suited to overcoming most of them. The present study, including demanding tests of the STE at one-week post-intervention and with distinct measures of affect towards the primary and secondary outgroups, confirms that the STE is a robust effect, of medium-to-large effect size, on average, in this research. Moreover, the present study demonstrates that STEs might be expressed in a variety of ways (including, as we found, attitude-to-trust generalisation) that go beyond Pettigrew's (2009) original description of the STE as a process involving attitude generalisation. This overall positive empirical picture should support future research on the value of this form of generalisation in interventions designed to reduce prejudice across multiple outgroups.

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Ethics Statement

The necessary ethics and institutional approvals were obtained from Stellenbosch University (2011 Project Number: 521/2011; 2019 Project Number: 8840).

Conflicts of Interest

The authors declare no conflicts of interest.

Data Availability Statement

The data that support the findings of this study are available from the corresponding author upon reasonable request.

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Supporting Information

Additional supporting information can be found online in the Supporting Information section. **Data S1:** Supporting Information.