

**Indirect Contact Predicts Direct Contact:
Longitudinal Evidence and the Mediating Role of Intergroup Anxiety**

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Abstract

While the effects of direct and indirect forms of contact on intergroup relations are well documented, little is known about their longitudinal co-development. Based on the social-psychological literature, we hypothesize that indirect contact predicts future direct contact by reducing intergroup anxiety. Across five longitudinal studies (Study 1: German adults, $N = 560$; Study 2: German, Dutch, and Swedish school students, $N = 6,600$; Study 3: Northern Irish children, $N = 1,593$; Study 4: Northern Irish adults, $N = 404$; Study 5: German adults, $N = 735$), we systematically examined this effect, and further tested the mediating role of intergroup anxiety in Studies 3 to 5. Cross-lagged models provided consistent evidence for the positive effect of indirect contact on future direct contact, while a reduction in intergroup anxiety mediates this effect in most models. Results highlight the importance of indirect contact, which has the potential to increase direct contact, and thus promote social cohesion in diverse contexts, over time.

Keywords: intergroup contact, extended contact, electronic contact, intergroup anxiety

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It is well-known that direct contact with the outgroup, as originally proposed by Allport (1954), improves intergroup relations and represents an effective means to overcome prejudice and reduce intergroup conflict between different groups. That is, positive face-to-face interactions with outgroup members trigger psychological processes such as learning about the outgroup, generating affective ties, changing behavior, and reappraising the ingroup, which together promote attitude change over time, as evidenced over six decades of research (Pettigrew, 1998; Pettigrew & Tropp, 2006). It is also well-known that not only direct contact can improve intergroup relations, but also indirect forms of outgroup contact, such as extended contact (Wright, Aron, McLaughlin-Volpe, & Ropp, 1997) or electronic contact (White & Abu-Rayya, 2012). That is, positive outgroup contact experienced by fellow ingroup members, such as friends or family, or via electronic means, such as computers or digital media, can have a positive impact on intergroup attitudes by fostering inclusion of the other in the self, promoting positive norms for engaging in intergroup contact, increasing outgroup knowledge, or reducing intergroup anxiety (Turner, Hewstone, Voci, & Vonofakou, 2008; Vezzali, Hewstone, Capozza, Giovannini, & Wölfer, 2014; White & Abu-Rayya, 2012).

Despite this evidence base, it is not yet clear if and how indirect and direct contact are longitudinally related to each other and co-evolve over time. Are indirect contact and direct contact two separate social processes that develop independently of each other? Does indirect contact encourage individuals to take up direct intergroup contact? Or does indirect contact satisfy one's interest in outgroup experiences and decrease motivation for future direct contact? A better understanding of the longitudinal interplay between indirect and direct contact would advance efforts and interventions aimed at promoting intergroup contact and

social cohesion in order to overcome segregation, prejudice, and intergroup conflict. Since most research suggests that direct contact is more powerful and effective than indirect contact for improving intergroup relations (e.g., Christ et al., 2010; Feddes, Noack, & Rutland, 2009; Schofield, Hausmann, Ye, & Woods, 2010; but see the recent meta-analysis by Zhou, Page-Gould, Aron, Moyer, & Hewstone, in press), it remains a key strategy for improving intergroup relations. However, in many contexts, especially in segregated or conflictual areas, indirect contact is easier to implement. Ascertaining the presence of a longitudinal effect of indirect contact on direct contact is thus particularly relevant and should offer practical insights into the wider effectiveness of contact as an intervention to reduce conflict. In this paper we aim to extend our knowledge regarding the co-development of direct and indirect contact by (a) testing across five longitudinal studies if indirect contact predicts direct contact, and (b) further examining in three of these studies why this hypothesized relationship occurs.

The Longitudinal Effect of Indirect Contact on Direct Contact

While much of the focus of previous work on indirect contact has, understandably, been on assessing its impact on prejudice and related outcomes (Vezzali et al., 2014; Zhou et al., in press), we propose that indirect contact could also present a developmental platform for future direct contact, a link that has been somewhat neglected in prior research. This idea was initially put forward by Turner, Hewstone, Voci, Paolini and Christ (2007) and has, since then, been echoed by others (see Eller et al., 2012; Gomez et al., 2011). We further propose that a contributing process underlying this hypothesized longitudinal effect of indirect contact on future direct contact is the reduction of intergroup anxiety as a result of indirectly experiencing a positive interaction between an ingroup and outgroup member. That is, indirect contact reduces intergroup anxiety which, in turn, encourages the formation of future outgroup contact.

Intergroup anxiety is a form of social anxiety that is restricted to intergroup contexts and includes, for example, intergroup uncertainty, concerns about being rejected by outgroup members, and negative outgroup expectations. Research indicates that this variable negatively predicts future direct contact (e.g., Shook & Fazio, 2011), and different theories highlight the role of intergroup anxiety in discouraging individuals from taking up opportunities for direct contact. Based on integrative threat theory (Stephan & Stephan, 2000), intergroup anxiety is a likely response to perceived threat by an outgroup and, consequently, an important antecedent of prejudice that may discourage individuals from engaging in outgroup contact. Relatedly, in his most recent theoretical model of intergroup anxiety, Stephan (2014) conceptualizes this construct as a central mediator of contact effects (also confirmed by Pettigrew & Tropp's, 2008, meta-analysis of contact mediators), situated between a set of antecedents, such as personal experiences (e.g., previous contact), and a set of outcomes, such as behavioral variables (e.g., future contact). That is, a lack of previous outgroup contact is likely to increase intergroup anxiety which, in turn, decreases positive intergroup behavior in an ensuing vicious circle. Furthermore, Turner and Cameron (2016) recently introduced the idea of contact confidence to explain the development of intergroup contact, which can be understood as a contact-specific form of self-efficacy (Bandura, 1997) or "a state of readiness for positive contact" that provides people with the necessary skill set for successful intergroup contact (Turner & Cameron, 2016, p. 218). According to this account, intergroup anxiety constitutes a crucial component of contact confidence or even its opposite.

One effective way to reduce intergroup anxiety is indirect contact, for example in the form of extended or electronic contact. Because extended or electronic forms of contact are experienced indirectly they, in contrast to direct outgroup contact, are hypothesized to minimize the arousal that occurs due to possible negative outgroup expectations (Wright et al., 1997). That is, individuals can experience anxiety-reducing outgroup contact without

being directly confronted with an outgroup member, a situation that they may tend to avoid in the case of high intergroup anxiety levels. When Wright and colleagues (1997) outlined the idea of extended contact, they proposed four possible underlying mechanisms, namely perceived ingroup norms regarding contact with the outgroup, perceived outgroup norms regarding contact with the ingroup, the inclusion of the outgroup in the self, and intergroup anxiety reduction. All of these mechanisms have been empirically supported as mediators between extended contact and attitudes, albeit mainly in cross-sectional studies (e.g., Turner et al., 2008), but intergroup anxiety seems to be a particularly important mediator of extended contact effects, because it was found to explain extended contact effects across a wide range of outcomes such as intergroup expectancies (Gomez, Tropp, & Fernandez, 2011), perceived outgroup variability (Paolini, Hewstone, Cairns, & Voci, 2004), beliefs and stereotyping (Cernat, 2011), and behavioral intentions (Hutchison & Rosenthal, 2011; Mazziotta, Mummendey, & Wright, 2011). The underlying mechanisms of electronic contact, in turn, are hypothesized to be intergroup anxiety and outgroup knowledge (White & Abu-Rayya, 2012) and, although the empirical evidence for this more recent form of indirect contact is less extensive, the existing research similarly indicates that intergroup anxiety is the most important mediator for electronic contact effects (White & Abu-Rayya, 2012; White, Abu-Rayya, & Weitzel, 2014). Altogether, the literature on both extended and electronic contact indicates that the anxiety-reducing effect of indirect contact is particularly powerful, making it is also likely to predict the development of actual intergroup friendships.

Furthermore, reflecting an argument that was initially proposed by Pettigrew and Tropp in their meta-analysis of contact mediators (2008), it is plausible to assume that intergroup anxiety has to be reduced first before other potential mediators (i.e., inclusion of the other in the self, perceived ingroup and outgroup norms, outgroup knowledge) can themselves come into play effectively. Given the self-fulfilling nature of stereotypes, negative

expectations such as intergroup anxiety shape individuals' perceptions of interpersonal interactions (Snyder, Tanke, & Berscheid, 1977). Thus, even in the case of indirect contact experiences, high levels of intergroup anxiety could initially block the beneficial effect of other mediators. Therefore, in the present paper we focus on intergroup anxiety as a potential mediator for the theorized longitudinal effect of indirect on direct contact.

The Current State of Research

A rich body of literature in the field of intergroup contact has consistently demonstrated moderately high cross-sectional associations between direct and indirect contact, as reviewed by Vezzali and colleagues (2014) and meta-analytically summarized with an average correlation of $r = .48$, 95% CI [.44, .52], $t(179) = 23.16$, $p < .001$, by Zhou and colleagues (in press). Only a few studies, however, have examined the longitudinal effects of indirect contact, even fewer its longitudinal role in the formation of future direct contact; and, to our knowledge, no research has examined potential mediators of this longitudinal effect from indirect contact on future direct contact.

We identified a total of seven studies that provide initial but weak support for the longitudinal effect of indirect contact on direct contact. Three studies offer indirect evidence by demonstrating a longitudinal indirect contact effect on a proxy of direct contact, such as behavioral intentions (Christ et al., 2010), engagement with the outgroup culture (Eller, Abrams, & Gomez, 2012), or outgroup expectancies (Gomez et al., 2011). The remaining four studies indicate that indirect contact might have a longitudinal effect on future direct contact. Schofield and colleagues (2010) found that extended contact significantly predicted future direct contact while controlling for initial direct contact, but without controlling for future extended contact, so that the missing residual correlation between extended and direct contact at time 2 produced a potentially biased estimate. Mallett and Wilson (2010) investigated electronic contact, and found that participants who watched a video of an inter-racial

interaction formed a higher proportion of outgroup friends, but only if they also engaged in an additional intervention and wrote about a positive outgroup experience that explicitly connected their indirect and personal experiences. Aboud, Friedmann, and Smith (2015) found marginal evidence that extended contact was associated with higher future direct contact while controlling for initial direct contact, but only in a small sample ($n < 100$). Finally, Wölfer, Schmid, Hewstone, and van Zalk (2016), who examined the developmental dynamics of intergroup contact and intergroup attitudes between adolescents and young adults using longitudinal growth curve models, found that extended contact explained the change in direct contact over time, but only at a younger age.

In sum, a few existing studies provide preliminary evidence for the idea that indirect contact predicts future direct contact, but they each have either a notable limitation or the effect of indirect on direct contact is qualified. More importantly, in all previous studies, potential mediators for the longitudinal indirect contact effect on direct contact remain unclear. That is, no prior research has tested *how* a putative longitudinal effect of indirect contact on direct contact occurs.

Research Objectives and Overview of the Current Studies

The present paper reports a systematic investigation of the longitudinal effect from indirect contact to future direct contact. In doing so, it extends the current state of research by testing this link across five longitudinal studies based on large samples, conducted in different settings and countries, focusing on different ingroup-outgroup constellations, examining different age groups, and by using different methods for measuring intergroup contact. In all five studies, we investigate the hypothesized longitudinal effects over at least a one-year time lag in order to examine the long-term effects from indirect contact to direct contact; this is necessary as contact does not improve intergroup relations immediately, but stimulates – via underlying processes such as reduction of intergroup anxiety – a positive change over a longer

period (Pettigrew, 1998). Moreover, in all five studies, we use the same statistical approach by analyzing cross-lagged models that test the effect from indirect contact to future direct contact, while considering the reverse path from direct contact to future indirect contact as well as the autoregressive effects of and covariations between direct and indirect contact. Besides this systematic test of the effect of indirect on direct contact, we also test whether intergroup anxiety mediates this potential longitudinal effect.

Specifically, Study 1 starts by testing whether extended contact with foreigners in a German adult sample is associated with future direct contact. Study 2 replicates this test in the context of intergroup relations between non-immigrant and immigrant students in a large-scale, international dataset (involving data from Germany, the Netherlands, and Sweden) by using an optimized social network operationalization of extended contact. Study 3 investigates the hypothesized link in the context of intergroup relations between Protestant and Catholic school students in Northern Ireland by simultaneously examining different forms of indirect contact, including extended contact and electronic contact, and by further testing the mediating role of intergroup anxiety. Study 4 examines the link between extended contact, direct contact, and intergroup anxiety in a sample of Protestant and Catholic adults in Northern Ireland using latent variable models. Finally, Study 5 examines the relationship between extended contact, intergroup anxiety and direct contact between Germans and foreigners using a three-wave model.

Notwithstanding the hypothesized longitudinal effect from indirect to direct contact, we also test a reverse effect. That is, direct contact may increase the level of indirect contact over time, as positive outgroup experiences (i.e., direct contact) may increase the preference for ingroup friends with more outgroup contact (i.e., extended contact) or the willingness to interact with outgroup members via digital media (i.e., electronic contact). Nonetheless, while it is important to statistically control for the effect of direct contact on indirect contact, we

believe that this link is less relevant for the reduction of prejudice and related outcomes than the effect from indirect contact to direct contact. That is, a potential effect from indirect to direct contact would help to maximize the effect of intergroup contact interventions, because this effect would, in contrast to the reverse link, lead to a more powerful antecedent of favorable intergroup attitudes (i.e., direct contact), while having a higher treatment efficiency (i.e., focusing on a few key individuals who then provide indirect contact for many) and lower treatment resistance (i.e., triggering less intergroup anxiety). Therefore, our main hypothesis predicts – based on the above reviewed literature – that indirect contact increases future direct contact across all studies, which will be mediated by a reduction in intergroup anxiety in Studies 3 to 5.

Study 1

Method

Sample. Data were collected as part of a multi-wave panel study representative of the German adult population with no migration background, which included a total of six waves (2002, 2003, 2004, 2006, 2008, and 2010; for further details see: Hohlweg, Derr, & Salentin, 2014). For our analyses, we used all respondents with a German background who participated in 2006 ($N = 1,120$) and 2008 ($N = 616$), because measures of indirect contact were available only in these two waves. A total of 560 participants (56% female; $M_{AGE} = 48.40$ years, $SD = 14.31$) provided complete longitudinal information. The formal level of education was high (university degree or high school diploma) for 47% of the sample, medium (secondary school) for 32%, and low (below secondary school) for 19%, while 2% provided no or non-classifiable information. Comparing study variables of participants who participated in both waves with those who dropped out provided only marginal to moderate differences regarding gender ($d = 0.08$), age ($d = 0.20$), education ($d = 0.04$), extended contact ($d = 0.03$) and direct contact ($d = .04$) indicating a drop-out of younger respondents.

Measures. Data from both waves were collected with the help of a survey company using computer assisted telephone interviewing (CATI), from which we analyzed the measures described below.¹

Intergroup Contact. At both waves, participants reported the quantity of direct contact ("How many of your friends are foreigners living in Germany?") and extended contact ("How many of your German friends have friends who are foreigners living in Germany?") using a four-point scale (1 = *none* to 4 = *many*). Participants also reported the quantity of direct intergroup contact in their neighborhood ("How often do you have direct contact to foreigners in your neighborhood?") as well as the frequency of pleasant encounters with foreigners (mean of three items: "How often did you receive help from a foreigner?"; "How often did you have an interesting conversation with a foreigner?"; "How often did you have pleasant experiences with foreigners?") using a four-point scale (1 = *never* to 4 = *often*), which served as alternative direct contact indicators.

Control Variables. We controlled for relevant socio-demographics (i.e., sex, age, and education), as well as for contact opportunity at wave 1 with two separate, empirically unrelated items ("How many foreigners are living in your neighborhood?" and "How many foreigners are working at your workplace?"; $r = .01$) rated on the same four-point scale used for the contact items. In contrast to other studies in this paper, we did not control for participants' group status, because this study focuses on the majority only.

Statistical Analyses. To test the cross-lagged relationship between extended and direct contact, we used cross-lagged path analysis (Mplus 7; Muthén & Muthén, 1998-2012). Because of a small amount of missing data (< 4% in all items) and non-normality, we used robust full information maximum likelihood estimates. Moreover, given the hierarchical data structure (participants nested in districts), we used the Mplus procedure COMPLEX that corrects the standard error of estimation resulting from the potential violation of non-

independence in hierarchical datasets. We will not report any fit statistics, because the analyzed path model is fully saturated.

Results & Discussion

Table 1 reports descriptive statistics and intercorrelations of direct and extended contact at waves 1 and 2. Across both waves, the means of direct and extended contact revealed moderate scores, which were moderately significantly associated with each other. Results of the cross-lagged analysis are summarized in Figure 1.

In line with our main hypothesis, results showed that extended contact at wave 1 positively predicted direct contact at wave 2 ($b = .21, p < .001$), while considering the reverse path, relevant autoregressive effects and covariations, and important control variables. Moreover, direct contact at wave 1 also positively predicted extended contact at wave 2 ($b = .12, p < .001$). As expected, direct contact thus also increased the likelihood of forming new ingroup friends who had outgroup contact (i.e., extended contact) and/or of maintaining ingroup friends who increased their level of outgroup contact. Findings of this main model can be replicated across both alternative direct contact parameters.² These results provide preliminary support for our assumption that indirect contact predicts future direct contact.

Study 2

In Study 2, we aimed to replicate the previous findings by addressing three central limitations of the first study. First, we systematically tested the link from extended contact to future direct contact across different contexts using a large, international dataset, allowing us to probe whether the results obtained in Study 1 generalize to other socio-political contexts. Second, we considered both majority and minority groups, allowing us to test whether the results of Study 1 (obtained for majority respondents only) also generalize to minority groups. And third, we used an optimized social network operationalization to measure extended

contact, allowing us to examine whether results generalize to other operationalizations of our main predictor, extended contact.

The social network approach has unique psychometric advantages, because it (a) is based on a comprehensive number of peer nominations that provide more information which, compared to self-reports, enables researchers to capture reciprocal relationships; (b) assesses connections without emphasizing individuals' group membership and, thereby, reduces participants' tendency to give socially desirable responses (e.g., to claim more outgroup contact than they actually have); and (c) considers direct and indirect relationships within the entire social network (for an introduction see Wölfer, Faber, & Hewstone, 2015, or Wölfer & Hewstone, 2017). In particular, for the assessment of extended contact, SNA advances traditional survey methods. While individuals are able to accurately report their direct contact, they are likely to have difficulties in recalling and precisely reporting indirect outgroup connections located two steps away from their network position. A more optimal approach is to analytically decompose these two steps using a multi-methodological procedure that combines SNA and self-reports by (a) identifying individuals' ingroup friends within their social network, and (b) assessing the direct contact that these ingroup friends report to have (for an empirical validation see Wölfer et al., 2016).

Method

Sample. Participants were part of the "Children of Immigrants Longitudinal Survey in Four European Countries" (CILS4EU; Kalter et al., 2014, 2015). The presented data include the first two waves, collected by the middle of the school years 2010/2011 and 2011/2012, of this ongoing collaboration between England, Germany, the Netherlands, and Sweden. At wave 1, 14-year-old school students were recruited and followed-up approximately one year later. Further waves could not be included, because the structural transition of some students to ongoing schools or the job market after wave 2 affected the composition of social

networks, which were used for calculating extended contact. A school-based sample selection design was applied to systematically oversample ethnically diverse schools with a high proportion of immigrant minority groups. Participation rate was high for schools (84%) and students (85% within participating schools). From the four initial countries, England had to be excluded due to a technical problem during the assessment of the social network data. In the remaining three countries, classes with fewer than 15 students (11%) were excluded in order to conduct meaningful analyses in sufficiently large networks.

These exclusion criteria left a total of 12,988 students from 616 ethnically mixed school classes at wave 1. Longitudinal data at waves 1 and 2 was available from 10,145 students (78%), while drop-out analyses revealed only marginal to moderate differences between students who participated in both waves versus the rest, regarding gender ($d = 0.00$), ethnicity ($d = 0.21$), and age ($d = 0.24$), favoring a drop-out of immigrants and older students. Completed longitudinal data was available from 8,790 students, of whom 75% were in classes with at least 15 participating students. Missing data analyses demonstrated that this final analytic sample again differed only marginally to moderately from respondents who dropped out regarding gender ($d = 0.07$), ethnicity ($d = 0.21$), and age ($d = 0.34$), favoring a missingness of immigrants and older students. The final analytic sample comprised 6,600 students ($M_{AGE} = 14.87$; 52% girls) including 2,210 immigrant minority students, from Turkey (5%, $n = 355$), Iraq (2%, $n = 121$), Serbia (2%, $n = 103$), Morocco (2%, $n = 100$), Poland (2%, $n = 98$), Bosnia and Herzegovina (1%, $n = 68$), Lebanon (1%, $n = 64$), and a variety of other countries (each < 1% of the current sample).

Measures. Data for both waves were collected in the regular school setting. With the help of trained test administrators, participants answered standardized questionnaires that primarily assessed migration-specific characteristics, core dimensions of integration, and social network data, from which we analyzed the measures described below.³

Direct Contact. At waves 1 and 2, participants reported the quantity of outgroup friends ("Thinking now about all of your friends. How many of them have a [OUTGROUP] background?") using a five-point scale (1 = *none of or very few* to 5 = *almost all or all*). While minority students reported their outgroup contact with the country-specific majority group (e.g., for Germany: contact with Germans), majority students reported their outgroup contact with the country-specific largest minority groups. These minority groups were determined based on their numerical representation in each country during the preparation of the survey in order to tailor items to the ethnic composition within each context (all selected country-specific minority groups range between 1 and 2% compared to the overall population in each country). For the present study, we averaged all contact scores for the main minority groups in each country (e.g., for Germany: contact with Turks, Russians, Poles, and Italians) into a general contact scale to provide a more reliable measure of direct contact, comparable across countries. Exploratory factor analysis revealed a one-factor solution for all minority contact scores in each country, supported by satisfactory Cronbach's α s ranging between .64 and .78. In this study, no longitudinal data regarding alternative direct contact indicators were available.

Extended Contact. We utilized a combination of self-reports and social network data to calculate extended contact at waves 1 and 2. To do this, we applied a two-step analytic procedure (cf., Wölfer et al., 2016): First, we elicited friendship networks in each class based on a peer nomination procedure that asked each student to nominate up to five classmates in response to "Who are your best friends in class?" (see Figure 2). These social networks allowed us to capture each student's reciprocally connected ingroup friends by determining mutually connected network members with the same group status (i.e., same-colored boxes linked with double-arrows, e.g., student #7 and #13). In a second step, we averaged the self-reported direct contact that these identified ingroup friends reported having, as measured with

separate survey items (the direct contact measures described above). In this way, we decomposed the complex two-step concept of extended contact into two separate analytical procedures; the mean self-reported contact score of participants' network-analytically identified ingroup friends represents each respondent's measure of extended contact.

Control Variables. At both waves, we controlled for relevant socio-demographics (i.e., sex and age), the group status, and the number of ingroup friends as a proxy for contact opportunities. The latter control is particularly important given the applied social network approach, because the quantity of participants' ingroup friends determines the number of potential agents through which participants can potentially experience extended contact. In contrast to other studies in this paper, we did not control for participants' educational background, because this study includes a student sample from one year-group that is still in the process of gaining an educational degree.

Statistical Analyses. We tested the main hypothesis that extended contact increases future direct contact by using a cross-lagged model that analyzed the effect from extended contact to future direct contact, while considering the reverse path as well as the autoregressive effects of and covariations between direct and extended contact. This main model further controlled for sex, age, ingroup friends at waves 1 and 2, as well as the group status (majority vs. minority group), and it accounted for the country clustering of the data. Thereafter, we examined the robustness of results by replicating the main model with separate country-specific models using the same model specification and control variables. For the analyses, we used the packages 'lavaan' (Rosseel, 2012) and 'lavaan.survey' (Oberski, 2014) within the R environment (R Core Team, 2015). We will not report any fit statistics, because the analyzed path models are fully saturated.

Results & Discussion

Social network analysis (SNA) was based on a total of 24,351 nominations at wave 1 (per student: $M = 3.69$, $SD = 1.38$) and 23,849 nominations at wave 2 (per student: $M = 3.61$, $SD = 1.49$). At wave 1, 16,916 friendship nominations were reciprocated, of which 66% were from the ingroup; at wave 2, 13,131 friendship nominations were reciprocated, of which 73% were from the ingroup. These results indicate some network dynamics between both waves and the expected ethnic homophily (i.e., preference for ingroup friendships) which increased over time, $t(5277) = 14.83$, $p < .001$. Table 1 reports the descriptives and intercorrelations of direct and extended contact, separately for the majority and minority group, at both waves. The self-reported means of direct contact revealed low scores for the majority and moderate scores for the minority, while extended contact was low across both groups. Finally, the zero-order bivariate correlations demonstrate low to moderate associations, apart from the autocorrelations of the same measures across different waves, highlighting the need to control for autoregressive effects within the following path model.

The main model revealed, in line with our hypothesis, that extended contact at wave 1 positively predicted future direct contact at wave 2 ($b = .06$, $p = .013$), while considering the reverse path ($b = .08$, $p < .001$), relevant autoregressive effects (direct contact: $b = .46$, $p < .001$; extended contact: $b = .35$, $p < .001$) and covariations between direct and extended contact (wave 1: $b = .17$, $p < .001$; wave 2: $b = .07$, $p = .062$) as well as all control variables.⁴ In an additional moderation analysis, we tested if contact effects differ as a function of individuals' majority- vs. minority-group status by centering continuous contact predictors, creating two cross-product terms of direct contact as well as extended contact multiplied with group status (majority = '0', minority = '1'), and adding them to the model. Results revealed a negative interaction effect of extended contact X group on future direct contact ($b = -.11$, $p < .001$) as well as a negative interaction effect of direct contact X group on future extended

contact ($b = -.06, p < .001$). We followed up these interaction effects with simple slope analyses (Preacher, Curran, & Bauer, 2006) indicating stronger contact effects for the majority (EC_{w1} to DC_{w2}: $b = .16, SE = .01, p < .001$; DC_{w1} to EC_{w2}: $b = .12, SE = .01, p < .001$) compared to the minority (EC_{w1} to DC_{w2}: $b = .01, SE = .03, p = .686$; DC_{w1} to EC_{w2}: $b = .05, SE = .00, p < .001$), which highlights the need to perform the following country-specific robustness checks separately for the majority and minority group.

Figure 3 summarizes the respective cross-lagged models that replicated the effect from extended contact to future direct contact (bold-marked paths), separately for the majority and the minority group in Germany, the Netherlands, and Sweden, respectively. In accordance with the main model, extended contact at wave 1 positively predicted future direct contact at wave 2 in five out of six models (Maj_{GER}: $b = .09, p = .003$; Maj_{NL}: $b = .07, p = .007$; Maj_{SWE}: $b = .10, p < .001$; Min_{GER}: $b = .14, p < .001$; Min_{SWE}: $b = .08, p = .029$). Only in the Dutch minority model did the hypothesized link fail to reach the conventional level of statistical significance ($p = .715$). Similar to what we found in the previous study, all models also yielded a significant path from direct contact to future extended contact (Maj_{GER}: $b = .13, p < .001$; Maj_{SWE}: $b = .08, p = .001$; Min_{GER}: $b = .08, p = .006$; Min_{NL}: $b = .08, p = .031$; Min_{SWE}: $b = .09, p < .001$), except for the Dutch majority model ($p = .477$).

In sum, results of Study 2 confirm those of Study 1 in a large, cross-national dataset including both majority and minority group members, and by using social network data. Thus, findings across different countries, groups (majority and minority members), and research methods (i.e., self-reports and SNA) in these first two studies indicate that indirect contact has a positive effect on future direct contact.

Study 3

We continued to examine the effect of indirect contact on direct contact in Study 3 by extending the previous two studies in two ways. First, we considered a new form of indirect

contact, electronic contact, as well as extended contact, to test whether the longitudinal effect of indirect on direct contact generalized across operationalizations of indirect contact. And second, we tested whether intergroup anxiety mediated the main effect from indirect to direct contact.

Method

Sample. Participants were part of a five-wave study in Northern Ireland conducted annually from the school year 2010/11 to 2014/15, from which we used the first two waves, collected in 2011 and 2012, before a large number of participating students received a contact intervention (i.e., 'Shared Education'), which confounds longitudinal and intervention effects in the final three waves. A total of $N = 2,905$ students from 56 schools provided complete data on all relevant study variables at wave 1. Given our research question, we focused on Protestant and Catholic students ($n = 2,657$; 92%). Complete longitudinal data at waves 1 and 2 was available from 1,593 students (60% retention rate; 58% female; $M_{AGE} = 10.71$ years, $SD = 0.30$; 33% Protestants, 67% Catholics) from 48 schools, while analyses revealed only marginal differences between students that participated in both waves versus those who dropped out regarding gender ($d = 0.06$), religious community ($d = 0.08$), and age ($d = 0.03$).

Measures. Participants answered standardized questionnaires that primarily assessed intergroup relations between Protestants and Catholics, from which we analyzed the measures described below.⁵

Direct Contact. At waves 1 and 2, participants reported the quantity of intergroup friends ("About how many of your close friends are Protestants/Catholics?") using a five-point scale (1 = *none* to 5 = *almost all friends*). Moreover, participants reported the quantity of intergroup contact in schools (mean of four items: "During class, how often do you do activities with Protestant/Catholic pupils?"; "During class, how often do you talk with Protestant/Catholic pupils?"; "During break, how often do you spend time with

Protestant/Catholic pupils?"; "During break, how often do you talk with Protestant/Catholic pupils?") as well as outside of schools ("Outside of school, how often do spend your free time with Protestant/Catholic children?") using a five-point scale (1 = *never* to 5 = *very often*), which served as alternative direct contact indicators. Protestant students reported their direct contact with Catholic students and vice versa.

Indirect Contact. At waves 1 and 2, participants reported the quantity of their extended outgroup contact ("How many of your close Catholic/Protestant friends have Protestant/Catholic friends?") using a five-point scale (1 = *none* to 5 = *ten or more friends*) and the quantity of their electronic outgroup contact ("How often do you send emails or link up on MSN with Protestant/Catholic children?") using a five-point scale (1 = *never* to 5 = *very often*). Protestant students reported their indirect contact with Catholic students and vice versa.

Intergroup Anxiety. At waves 1 and 2, participants reported their level of intergroup anxiety (mean of two items with common stem: "Now think of a situation where you might meet Protestant/Catholic children."; item-specific completion: "Would you feel nervous towards them?" and "Would you feel uncomfortable around them?"; (wave 1: $r = .62$; wave 2: $r = .65$) using a five-point scale (1 = *not at all* to 5 = *very much*). Protestant students reported their intergroup anxiety towards Catholic students and vice versa.

Control Variables. We controlled for relevant sociodemographics (i.e., sex and age), group status (Protestant or Catholic), and contact opportunity (i.e., proportion of Catholic students in school). In contrast to other studies in this paper, we did not control for participants' educational background, because this study includes a student sample from one year-group that is still in the process of gaining an educational degree.

Statistical Analyses. Cross-lagged models analyzed the effects from indirect contact (i.e., extended and electronic contact) to future direct contact via intergroup anxiety, while

considering the reverse paths from direct contact to future indirect contact via intergroup anxiety, the autoregressive effects of and covariations between direct contact, indirect contact, and intergroup anxiety as well as all above described control variables. Thereafter, we examined the robustness of results by replicating this main model with alternative direct contact indicators (i.e., intergroup contact in schools and intergroup contact outside of schools) using the same model specification and control variables. For all analyses, we used the package 'lavaan' (Rosseel, 2012) within the R environment (R Core Team, 2015). We will not report any fit statistics, because the analyzed path models are fully saturated.

Results & Discussion

Table 1 reports descriptive statistics and intercorrelations of all study variables at waves 1 and 2. Across both waves, the means of direct, extended, and electronic contact revealed moderate scores that were positively associated with each other, while the means of intergroup anxiety revealed low scores that were negatively associated with all contact variables. Results of the cross-lagged analysis are summarized in Figure 4.

As expected, results showed that both forms of indirect contact at wave 1 uniquely predicted direct contact at wave 2 (extended contact: $b = .10, p < .001$; electronic contact: $b = .11, p < .001$), while considering the reverse paths, autoregressive effects, covariations, and important control variables. Similar to Studies 1 and 2, direct contact at wave 1 also positively predicted indirect contact at wave 2 (extended contact: $b = .16, p < .001$; electronic contact: $b = .27, p < .001$). Contrary to our hypothesis, however, neither indirect contact at wave 1 predicted intergroup anxiety at wave 2 (extended contact: $b = -.02, p = .477$; electronic contact: $b = -.02, p = .513$), nor did intergroup anxiety at wave 1 predict direct contact at wave 2 ($b = -.02, p = .398$). A plausible explanation for the unexpected findings of intergroup anxiety is the floor effect in the present sample, as most participants reported having no intergroup anxiety at all (64% at wave 1 and 67% at wave 2) which might be due to the

unusually mild assessment of this variable in the present study. Traditionally, intergroup anxiety is assessed with a solo status scenario by asking participants for their feelings in an intergroup situation in which they would be the only member of their group (Stephan & Stephan, 1985). This scenario increases group salience, threat reactions, and rejection concerns (Frey & Tropp, 2006; Shapiro, Baldwin, Williams, & Trawalter, 2011), which decreases the likelihood of floor effects revealed in this study and increases the likelihood that participants agree with an item which, in turn, increases the overall variance and helps to differentiate between participants with high versus low levels of intergroup anxiety. Findings of this main model can be replicated across both alternative direct contact parameters.⁶

In sum, results of Study 3 confirm the previous two studies to the extent that indirect contact predicts future direct contact, while this effect was found for two different forms of indirect contact (i.e., extended contact and electronic contact). However, we revealed no support for the hypothesized mediational role of intergroup anxiety, which might be due to the unusually mild assessment of this variable and deserves further investigation using the traditional intergroup anxiety scale by Stephan and Stephan (1985) in the following studies.

Study 4

After confirming the longitudinal effect of indirect contact on direct contact across different forms of indirect contact, we focused again on extended contact in the remaining two studies to examine the main hypothesis in further detail. In Study 4, we continued to further examine the main effect of indirect contact on direct contact by testing the mediating role of intergroup anxiety. For this, we used the established intergroup anxiety scale by Stephan and Stephan (1985), which was psychometrically validated in previous research (cf., Lolliot et al., 2014). Moreover, although the previous three studies tested the main hypothesis using different methods, they were largely based on single-item measures. In Study 4, we used multi-item measures of all key constructs that allowed us to model latent variables and

thereby to consider the measurement error and to test longitudinal measurement invariance, an important criterion of cross-lagged analysis (Little, Preacher, Selig, & Card, 2007).

Method

Sample. Participants were randomly drawn from four neighborhoods in Belfast, Northern Ireland. The first wave included $N = 984$ adults who completed the survey between March and July 2006. The second wave was conducted approximately one year later. A total of 404 individuals completed the relevant measures at both time points, and constituted the sample for our longitudinal analyses (65% female; $M_{AGE} = 51.51$ years, $SD = 16.65$; 56% Protestants, 44% Catholics; formal level of education, based on the same classification explained in Study 1, was high for 31%, medium for 15%, and low for 54%). Missing data analyses demonstrated that this analytic sample differed only marginally from respondents who dropped out with regard to gender ($d = 0.12$), age ($d = 0.06$), education ($d = 0.00$), and religious community ($d = 0.01$).

Measures. Data for both waves were collected using face-to-face interviews conducted by a survey organization, from which we analyzed the measures described below.⁷

Intergroup Contact. At both waves, participants reported the quantity of direct contact and extended contact. Direct contact was measured with three items ("How many of your *close* friends are Protestants/Catholics?", "How often do you visit your close Protestant/Catholic friends in their home?", "How often do your close Protestant/Catholic friends visit you in your home?"; wave 1: $\alpha = .90$, wave 2: $\alpha = .91$) using a six-point rating scale (1 = *none* to 6 = *all*). Extended contact was measured with two items ("Please think about your close Catholic/Protestant friends: About how many of them have close friends who are Protestants/Catholics?"; "Please think about members of your immediate family (your parents, children, siblings, or partner): About how many of them have close friends who are Protestants/Catholics?"; wave 1: $r = .66$, wave 2: $r = .67$) using the same rating scale. On

average, 20% of the extended contact data (i.e., two items across both waves) was missing, which was mainly due to the fact that participants rated these questions as 'not applicable' given a lack of knowledge about family members' outgroup friends. In all other measures, the amount of missing data was negligible (< 1%).

Participants also reported their quantity of intergroup contact in their neighborhood (mean of three items: "In your neighbourhood, how often do you greet people who are Protestant/Catholic?"; "How often do you chat to people who are Protestant/Catholic in your neighbourhood?"; "How often do you do something social together with your Protestant/Catholic neighbours?") using a five-point scale (1 = *never* to 5 = *very often*) as well as the quality of intergroup contact in their neighborhood (mean of three items with common stem: "If you have contact with Protestants/Catholics in the area where you live"; item-specific completion: "is the contact generally friendly?"; "do you generally meet as equals?"; "is the contact generally cooperative?") using a five-point scale (1 = *never* to 5 = *always*), which served as alternative direct contact indicators.

Intergroup Anxiety. We measured this variable with items adapted from Stephan and Stephan's (1985) scale. At both waves, participants reported their feelings in response to the item: "If you were the only Protestant/Catholic interacting with Catholics/Protestants, to what extent would you feel [...]?" on six items, *nervous*, *anxious*, *comfortable* (reverse coded), *awkward*, *safe* (reverse coded), and *at ease* (reverse coded), using a six-point rating scale (1 = *not at all* to 6 = *extremely*). These items yielded reliable scale scores (wave 1: $\alpha = .92$, wave 2: $\alpha = .93$).

Control Variables. We controlled for relevant socio-demographics (i.e., sex, age, education, and group status) as well as contact opportunity measured at wave 1 with two separate, marginally related items ("In the area where you live, how many of the people do

you think are Protestants/Catholics?" and "About how many of your work colleagues are Protestants/Catholics?"; $r = .18$) using a six-point rating scale (1 = *none* to 6 = *all*).

Statistical Analyses. To test the cross-lagged relations between extended contact, intergroup anxiety, and direct contact, we used structural equation modeling with latent variables using Mplus 7 (Muthén & Muthén, 1998-2012). Because of missing data and non-normality, we again used robust full information maximum likelihood estimates.

Results & Discussion

Table 1 reports the descriptives and intercorrelations of all study variables at waves 1 and 2. Results followed the expected pattern: Across both waves, the means of direct contact, extended contact, and intergroup anxiety revealed low to moderate scores, while both contact scores were positively associated with each other and negatively associated with intergroup anxiety.

Model Specification. Longitudinal measurement invariance for extended contact, intergroup anxiety, and direct contact is a necessary condition for any meaningful interpretation of the relationships between these constructs over time (Brown, 2006; Vandenberg & Lance, 2000). As we were solely interested in the relation between the variables over time, metric invariance (i.e., equality of factor loadings over time) was the sufficient form of measurement invariance that we tested as described below.

Applying the step-up approach proposed by Brown (2006), we first tested the configural invariance of the measurement model at waves 1 and 2. We allowed for covariances between the residuals of the two reverse-coded intergroup anxiety items in order to model method variance, and constrained the variances of the latent variables to 1 for scale-setting. This enabled us to freely estimate all factor loadings, which yielded a satisfactory model fit at both wave 1 ($\chi^2(38) = 101.70, p < .001$; CFI = .973; RMSEA = .064; SRMR =

.077) and wave 2 ($\chi^2(38) = 74.77, p < .001$; CFI = .984; RMSEA = .049; SRMR = .049), indicating configural invariance of the measurement model at both waves.

We then performed a longitudinal confirmatory factor analysis that simultaneously estimated the measurement models at both waves. For this purpose, we compared an unconstrained model (i.e., no constraints besides those needed for scale-setting) with a metric invariance model (i.e., factor loadings of corresponding items were constrained to be equal over time and the covariances between their residuals were allowed) using the Satorra-Bentler scaled chi-square statistic (Satorra & Bentler, 2001). The fit of the metric invariance model was satisfactory ($\chi^2(188) = 342.03, p < .001$; CFI = .973; RMSEA = .045; SRMR = .080), but significantly worse ($\Delta\chi^2_{\text{corr}}(11) = 68.47, p < .001$) than the unconstrained model ($\chi^2(177) = 272.34, p < .001$; CFI = .983; RMSEA = .037; SRMR = .066). After gradually relaxing the equality constraints on the factor loadings of the two reverse-coded intergroup anxiety items (i.e., successively unconstraining one item at a time), the fit of this modified model was acceptable ($\chi^2(186) = 293.557, p < .001$; CFI = .981; RMSEA = .038; SRMR = .068), but still significantly worse than the unconstrained model ($\Delta\chi^2_{\text{corr}}(9) = 20.95, p < .013$). Further relaxation of constraints did not, however, lead to a significant improvement in model fit.

Given the bias of the chi-square difference test against invariance (i.e., more restrictive models) in large samples (MacCallum, Browne, & Cai, 2006) and the smaller Bayesian information criterion of the modified metric invariance model (BIC = 20530) compared to the unconstrained model (BIC = 20563), we concluded that partial measurement invariance could be assumed (Byrne, Shavelson, & Muthén, 1989).

Extended Contact to Direct Contact. To test our first hypothesis, we estimated a structural equation model including all possible cross-lagged effects between the measures of extended contact, intergroup anxiety, and direct contact at waves 1 and 2. Moreover, we controlled for age, sex, education, and ethno-religious group of participants as well as contact

opportunity. The model fit of this cross-lagged model was acceptable ($\chi^2(370) = 556.659, p < .001$; CFI = .976; RMSEA = .035; SRMR = .067). We then constrained all non-significant cross-lagged paths between the main constructs to zero. The fit of this modified cross-lagged model was still acceptable ($\chi^2(373) = 558.034, p < .001$; CFI = .976; RMSEA = .035; SRMR = .068) and comparable to the cross-lagged model without constraints ($\Delta\chi^2_{\text{corr}}(3) = 0.90, p = .825$).⁸ Results of the modified cross-lagged model are summarized in Figure 5.

In line with our main hypothesis, extended contact at wave 1 was significantly negatively related with intergroup anxiety at wave 2 ($b = -.22, p = .001$), indicating that participants who reported having more extended contact at wave 1 reported less intergroup anxiety at wave 2. Moreover, we found a negative and significant relationship between intergroup anxiety at wave 1 and direct contact at wave 2 ($b = -.10, p = .038$), indicating that participants who reported having lower intergroup anxiety at wave 1 reported more direct contact at wave 2. Finally, direct contact at wave 1 was significantly positively linked to extended contact at wave 2 ($b = .19, p = .008$), indicating that participants who reported having more direct contact at wave 1 reported more extended contact at wave 2.

In order to evaluate the mediational effect of intergroup anxiety longitudinally, we estimated the longitudinal indirect effect of extended contact at wave 1 on direct contact at time 2 (Cole & Maxwell, 2003). This indirect effect is, in the case of two waves, the product of both bold-marked paths in Figure 5: from extended contact at wave 1 to intergroup anxiety at wave 2, and from intergroup anxiety at wave 1 to direct contact at wave 2. Results showed that this indirect effect was positive and significant ($b = 0.023, p = .029$ [one-tailed significance]). Estimating the 95% bias corrected bootstrap confidence interval for the indirect effect using 5000 bootstrap samples confirmed the mediational effect in that the interval does not include zero (.004 – .057). Findings of this main model can be replicated for

the quality of intergroup contact in the neighborhood and, in part (i.e., replicated main effects, but no significant indirect effect), for the quantity of intergroup contact in the neighborhood.⁹

In summary, the results of Study 4 suggest that the longitudinal effect of indirect contact on direct contact is, as hypothesized, mediated by intergroup anxiety. That is, indirect contact reduces intergroup anxiety, which, in turn, increases direct contact with members of outgroups.

Study 5

In the final study, we sought to replicate the longitudinal effect of extended contact on future direct contact via intergroup anxiety by employing a three-wave mediation design. The estimation of longitudinal data with only two waves (or cross-sectional data) relies on the stationarity assumption (Cole & Maxwell, 2003), which implies an unchanging causal structure among the variables (i.e., the effect of extended contact at wave 1 on anxiety at wave 2 is the same as the effect of extended contact at wave 2 on anxiety at wave 3), while non-stationarity can result in biased estimates. In all previously reported two-wave models, we were not able to test for stationarity, which means that we cannot rule out a possible bias in our estimated effect of indirect contact on direct contact and the respective indirect effect via intergroup anxiety. The availability of three waves in Study 5, however, enables us to test for stationarity between the constructs, which helps us to put the previous two-wave models into perspective. Moreover, in Study 5, we were able to test full longitudinal mediation (i.e., the effect from extended contact at wave 1 to intergroup anxiety at wave 2, followed by the effect of intergroup anxiety at wave 2 to direct contact at wave 3).

Method

Sample. Participants were sampled from German neighborhoods with a varying proportion of foreigners, resulting in a hierarchical data structure with participants nested in neighborhoods (see Schönwälder et al., 2016). Fifty neighborhoods (minimum size $N = 2,800$

residents, average size $N = 7,500$ residents) from sixteen different cities in Germany were randomly drawn. In this study, we focused on German majority respondents only (i.e., those who had German citizenship and no migration background), because the size of the various minority groups in the sample was too small to consider them analytically. Data collection took place from May to July 2010 for wave 1 ($N = 1,976$), from May to August 2011 for wave 2 ($N = 1056$), and from May to August 2012 for wave 3 ($N = 735$). The final analytic sample is based on participants who took part in all three waves ($N = 735$). Missing data was negligible ($< 5\%$) as was systematic panel mortality. Comparing study variables of participants who were re-interviewed at waves 2 and 3 with those who only participated at wave 1 provided only marginal to moderate differences regarding gender ($d = 0.12$), age ($d = 0.26$), education ($d = 0.23$), extended contact ($d = 0.09$) and direct contact ($d = 0.19$) indicating a drop-out of younger and higher educated respondents.

Measures. Data from all three waves were collected with the help of a survey company using CATI, from which we analyzed the measures described below.¹⁰

Intergroup Contact. At all three waves, participants reported the quantity of direct contact ("In your neighbourhood, how often do you talk to people who are themselves not native Germans or whose parents are not from Germany?") and extended contact ("Now please think of your close friends and family members, who are native Germans. How many of them have friends who are themselves not native Germans or whose parents are not from Germany?") using a five-point rating scale (1 = *never/no one* to 5 = *daily/all*).

Moreover, participants reported the number of weak outgroup ties ("Think about acquaintances to whom you have rather loose contact. How many of your loose acquaintances are not native Germans?") using a five-point rating scale (1 = *no one* to 5 = *all*), as well as the number of strong outgroup ties ("Please think of people you feel very close to. How many of

these people are not native Germans?") using an open answer format, which served as alternative direct contact indicators.

Intergroup Anxiety. We measured this variable with items adapted from Stephan and Stephan's (1985) scale. At all three waves, participants reported their feelings in response to: "Imagine that you are the only German in a group of people with a different background. To what degree would you feel?" on two items, *anxious* and *uncomfortable* (wave 1: $r = .75$; wave 2: $r = .73$; wave 3: $r = .75$), using a five-point rating scale (1 = *not at all* to 5 = *very much*).

Control Variables. We controlled for relevant socio-demographics (i.e., sex, age, and education) as well as contact opportunity (i.e., foreigner proportion of sampled neighborhoods; as official statistics were not available for the neighborhoods we sampled, these data were obtained by contacting officials in each area; see Petermann et al., 2012). In contrast to other studies in this paper, we did not control for participants' group status, because this study focuses on the majority only.

Statistical Analyses. To test the cross-lagged relations between extended contact, intergroup anxiety, and direct contact, we used cross-lagged path analysis (Mplus 7; Muthén & Muthén, 1998-2012). Because of missing data and non-normality, we used robust full information maximum likelihood estimates. Moreover, given the hierarchical data structure (participants nested in neighborhoods), we used the Mplus procedure COMPLEX that corrects the standard error estimation resulting from the potential violation of non-independence in hierarchical datasets.

Results & Discussion

Table 1 reports the descriptives and intercorrelations of all study variables at waves 1, 2, and 3. Across all three waves, the means of direct contact and extended contact revealed, similarly to the three previous studies, at best moderate scores and low to moderate

associations, while intergroup anxiety also revealed, similar to Study 4, moderate scores and the expected negative associations with both contact measures.

To test our hypothesis, we estimated a path model including all possible cross-lagged effects between the measures of extended contact, intergroup anxiety, and direct contact at waves 1 and 2, as well as waves 2 and 3. Moreover, we controlled for age, sex, education, and contact opportunity. To test for stationarity, we compared a model with freely estimated autoregressive and cross-lagged paths to a more restrictive model in which we constrained all autoregressive and cross-lagged paths between wave 1 and wave 2 to be equal with the autoregressive and cross-lagged paths between wave 2 and wave 3. The fit of the more restrictive model was significantly worse compared to the less-restrictive model ($\Delta\chi^2_{\text{corr}}(9) = 31.34, p < .001$). After unconstraining the autoregressive effect of extended contact, the fit of this modified stationarity model still differed significantly from the less restrictive model ($\Delta\chi^2_{\text{corr}}(8) = 8.13, p = .412$). Furthermore, this difference was small in terms of practical relevance (w effect size $< .1$), while further relaxations did not result in an improved model fit. Thus, although the stability of extended contact varied, results generally support the stationarity assumption, indicating an unchanging causal structure of autoregressive and cross-lagged paths over time (i.e., the effect from wave 1 to wave 2 is the same as the effect from wave 2 to wave 3). The fit of this model was mediocre ($\chi^2(17) = 174.143, p < .001$; CFI = .932; RMSEA = .112; SRMR = .032), but overall still acceptable (Hu & Bentler, 1999).

All cross-lagged paths were significant (see Figure 6). In line with our main hypothesis, extended contact was significantly negatively related with intergroup anxiety at the subsequent wave ($b = -.07, p = .001$), indicating that participants who reported having more extended contact at a previous wave reported less intergroup anxiety at the subsequent wave. Moreover, we also found the expected significant negative relation of intergroup anxiety with direct contact at a subsequent wave ($b = -.04, p = .041$), indicating that

participants who reported having less intergroup anxiety at a previous wave reported more direct contact at the subsequent wave. Finally, there was also, as expected, a positive and significant relationship between earlier direct contact and extended contact at the subsequent wave ($b = .10, p < .001$), indicating that participants who reported having more direct contact at a previous wave reported more extended contact at the subsequent wave.

In addition, we tested the mediational effect of intergroup anxiety longitudinally (e.g., Cole & Maxwell, 2003). For this purpose, we estimated the longitudinal indirect effect of extended contact at wave 1 on direct contact at wave 3 via intergroup anxiety at wave 2. Results showed that this indirect effect was positive and significant ($b = 0.003, p = .030$ [one-tailed significance]). Estimating the 95% bias corrected bootstrap confidence interval for the indirect effect using 5000 bootstrap samples confirmed the mediational effect in that the interval did not include zero (.001 – .007). Findings of this main model can be replicated for the number of weak ties and, in part (i.e., replicated main effects, but no significant indirect effect), for the number of strong ties.¹¹

In summary, the results of Study 5 replicate those of Studies 1 to 4 by confirming the longitudinal effect from indirect to direct contact, and by providing further evidence that the longitudinal effect of indirect contact on direct contact is mediated via intergroup anxiety. That is, indirect contact at wave 1 reduces intergroup anxiety at wave 2, which, in turn, increases direct contact at wave 3.

General Discussion

The two main aims of the present paper were (a) to systematically test the longitudinal effect of indirect contact on future direct contact, and (b) to further examine this main effect by testing the mediating role of intergroup anxiety.

Indirect Contact Predicts Direct Contact

The present paper provides evidence across five longitudinal studies that indirect contact predicts future direct contact. This main finding goes some way beyond existing cross-sectional data (Turner et al., 2008), and overcomes some limitations of previous longitudinal research that offered initial evidence of this effect (Aboud et al., 2015; Mallett & Wilson, 2010; Schofield et al., 2010; Wölfer et al., 2016). The presented evidence was accrued from four different countries (Germany, the Netherlands, Northern Ireland, and Sweden), two different settings (school classes and neighborhoods), two different age groups (school students and adults), three different ingroup-outgroup constellations (natives vs. foreigners, natives vs. immigrants, and Protestants vs. Catholics), and three different contact measures (i.e., self-reports, social network data, and latent variables). All five studies included a large number of respondents from different samples, which – considering the usual effect size of contact on intergroup relations outcomes ($r = -.215$; Pettigrew & Tropp, 2006) and a desired power of 80% – exceeded by far the estimated minimum of $N = 167$ participants. Moreover, given the current discussions regarding the replicability of research in our field (Open Science Collaboration, 2015), which seems to be even more important for contextually sensitive research topics (Van Bavel, Mende-Siedlecki, Brady, & Reinero, 2016) such as intergroup relations, a largely consistent finding across five studies from different contexts should be considered convincing evidence with high external validity.

This key finding is promising, for at least three reasons. First, indirect contact is easier to implement in interventions than direct contact, for example by fostering intergroup friendship between a few individuals who model and diffuse this outgroup experience to their ingroup friends afterwards (Wright et al., 1997, Study 3) or by electronically presenting real or fictional role models for intergroup contact (Cameron & Rutland, 2006; Paluck, 2009; Vezzali, Stathi, Giovannini, Capozza, & Visintin, 2015). Second, indirect contact should

trigger less intergroup anxiety and, therefore, provides effective contact opportunities, in particular for biased individuals. And third, in segregated areas that provide limited opportunities for direct contact, people have a higher chance of experiencing indirect than direct contact. In this way, indirect contact and its tendency to promote direct contact over time has the potential to be a useful antagonist that works against segregation and homophily in diverse societies. This tendency might be particularly useful given the low to moderately high self-reported direct contact scores across all studies, as well as the demonstrated network homophily in Study 2.

Importantly, this main finding seems to be generalizable to different forms of indirect contact. In line with our theoretical reasoning, the longitudinal effect from indirect to direct contact was not restricted to one specific form of indirect contact, but found for different forms of indirect contact, and seems to be applicable to indirect contact as a whole. That is, experiencing outgroup contact indirectly, via ingroup friends or electronic devices, provides in general a gentle but nonetheless effective form of contact that does not require the possibly stressful direct confrontation with an outgroup member and can, therefore, increase the likelihood of engaging in future direct contact. This evidence is encouraging, not only because it offers a more unified understanding of this longitudinal indirect contact effect, but also because it increases the possibilities of intervention strategies for improving direct contact using different forms of indirect contact.

Comparing the effects of indirect contact on direct contact across the different studies indicates some noteworthy differences, including an indirect-only mediation in Study 4, moderate effects in Studies 2, 3, and 5 ($b = .07 - .14$), and a strong effect in Study 1 ($b = .21$). While it remains unclear if these differences are based on the different contexts, samples, measures, or time intervals across studies, it is important to note that we find robust evidence for this indirect effect in every study. Even the indirect-only mediation still supports the

overall effect, albeit only indirectly, as well as the mediating process in line with the hypothesized theoretical framework (cf., Zhao, Lynch, & Chen, 2010). Relatedly, the revealed longitudinal effects of indirect contact on future direct contact, as well as on intergroup anxiety (discussed below), are of rather small size. These effects are, however, in line with the usual effect sizes obtained for direct intergroup contact (Pettigrew & Tropp, 2006), which tend to be even smaller for indirect contact (Vezzali et al., 2014). We contend, however, that these effects are nonetheless of practical importance, for two reasons. First, small effects matter, as they can accumulate into more powerful changes over time. That is, small differences in intergroup relations can have important influences on decision-making processes and behavioral choices individuals make, which – in the form of an initial impulse – can further alter intergroup relations (Abelson, 1985; Van Laar, Levin, Sinclair, & Sidanius, 2005). Second, the reported results revealed in our models are naturally occurring effects, which happened in the absence of any contact-promoting strategies. It is likely that these indirect contact effects can be maximized in intervention programs that focus, for example, on encouraging indirect contact in individuals with high intergroup anxiety levels or who are located in the center of a social network and can spread indirect contact experiences to a large number of network members.

Based on the results of Study 2, it is also worth mentioning that the effect from indirect to direct contact seems to differ between the majority and minority groups. In line with the existing literature (Pettigrew & Tropp, 2005; but see also Wölfer, Jaspers, Blaylock, Wigoder, Hughes, & Hewstone, in press), our additional moderation analysis indicates that the hypothesized effect is stronger for the majority compared to the minority. In general, there are several possible explanations for weaker contact effects in the minority, including minority members' higher levels of outgroup contact that could make additional contact less effective, and their higher likelihood of experiencing more negative contact, discrimination,

and group devaluation, all of which reduce positive contact effects. This might add to the explanation for why we reveal no significant effect in one country-specific minority model in Study 2 (i.e., Dutch minority model). In addition, this particular model was characterized by a relatively strong auto-correlation of direct contact over time ($b = .57$), compared to the other models ($b = .40 - .44$), which leaves little change in direct contact that can be explained by hypothesized predictors, such as indirect contact.

Independently of the size of effect, once positive intergroup contact has been successfully initiated, a self-reinforcing process seems to take place in that direct contact and indirect contact bi-directionally enhance each other. Given the positive paths from indirect contact to direct contact and from direct contact to indirect contact revealed in most models reported in this paper, it does not appear to be necessary that individuals experience indirect contact before engaging in direct contact. Unfortunately, the available measures make it difficult to estimate the precise order in which people typically experience direct and indirect contact. However, based on the descriptives across all five studies, indirect contact seems to be more frequent than direct contact (see Table 1), except for Study 2 in which individuals' network-based extended contact does not capture the frequency of indirect contact but the inevitably lower mean contact of their friends. This higher frequency of indirect contact suggests that more individuals may well have indirect contact first, given the higher likelihood and opportunity of this type of contact, before experiencing direct contact.

While extended contact requires some minimal level of mixing in order to improve future direct contact, electronic contact opportunities are almost unlimited in most countries of the world and, thereby, offer alternative indirect contact experiences even in highly segregated environments where individuals' ingroup friends have no outgroup friends. However, the mere opportunity for contact is a crucially important factor for the longitudinal process from any form of indirect contact to direct contact. Although our evidence suggests

that this effect happens naturally, co-occurring levels of segregation, individuals' powerful tendency towards homophily, and the consequential lack of contact opportunities work against it (McPherson, Smith-Lovin, & Cook, 2001). Therefore, the implication of this paper is not that we should be complacent, but rather that we should use indirect contact more explicitly in intergroup contact interventions in order to improve intergroup relations, as demonstrated, for example, in the pioneering studies by Paluck (2009) or Vezzali and colleagues (2015).

The Mediating Role of Intergroup Anxiety

In accordance with several theoretical approaches, our research suggests that intergroup anxiety seems to mediate the effect from indirect contact to direct contact and can improve our understanding of why some individuals take up existing contact opportunities in increasingly diverse societies and others do not. Although results of Study 3 did not support our hypothesis – most likely because of the unusually mild measurement of intergroup anxiety, the following two longitudinal studies consistently demonstrated that indirect contact reduced intergroup anxiety, which, in turn, increased future direct contact.

The mixed evidence regarding the longitudinal link from direct contact to future intergroup anxiety in Studies 3 and Study 4 (non-significant) versus Study 5 (significantly negative) is in accordance with the inconclusive state of research in this respect. Most studies reveal that direct contact reduces intergroup anxiety (e.g., Shook & Fazio, 2008; Swart, Hewstone, Christ, & Voci, 2010), whereas some other studies indicate that direct contact can also increase intergroup anxiety (e.g., Littleford et al., 2005; Shelton, 2003). Recent advances have begun to explain this inconsistency with the additional consideration of pre-existing intergroup relations and time. That is, individuals high in intergroup anxiety respond with stronger stress reactions in intergroup situations which, however, decrease in subsequent positive intergroup interactions (Page-Gould, Mendoza-Denton, & Tropp, 2008) and, in

particular, in the case of continuous positive intergroup contact over time (MacInnis & Page-Gould, 2015). Given that Studies 3 and 4 include participants from a more conflictual and segregated setting (Northern Ireland), compared to Study 5 (Germany), supported by – relative to the scale – lower levels of direct contact (see Table 1), it is possible that contextual differences account for the inconsistent effects of direct contact on intergroup anxiety across studies.

Indirect contact, however, reduces intergroup anxiety from a more comfortable distance, which is less likely to trigger negative feelings, expectations, and beliefs compared to direct face-to-face interactions with the outgroup, in particular for individuals with unfavorable intergroup relations and attitudes. Moreover, it is plausible that the anxiety-reducing effect of indirect contact also comes with more positive (side) effects, because previous research has found that intergroup anxiety mediates positive contact effects across a wide range of outcome variables (e.g., Gomez et al., 2011; Paolini et al., 2004; Turner et al., 2008), and it is likely to foster other mechanisms underlying indirect contact effects (i.e., inclusion of the other in the self, positive ingroup norms, and positive outgroup norms; Wright et al., 1997).

In this respect, one of the novel contributions of this research is the revealed evidence of the mediating role of intergroup anxiety for the longitudinal effect from indirect contact to future direct contact. Knowing that an ingroup friend has outgroup contact, or interacting with outgroup members electronically, can reduce intergroup anxiety and increase the likelihood of future direct contact with outgroup members. It may thereby help to break the vicious cycle of self-segregation and homophilous networks, biased perceptions, narrow-mindedness, stereotype-confirmation, and contact avoidance.

Limitations and Future Research Directions

Notwithstanding the consistent replication of effects across five longitudinal studies, we acknowledge some limitations of this research, which signal directions for future research. First, we were only able to test and reveal partial support for one possible mediator for the longitudinal effect of indirect contact on future direct contact, intergroup anxiety. Based on results across different measurements of this mediator, our findings suggest that the traditional intergroup anxiety scale by Stephan and Stephan (1985) seems to produce more variance needed for empirical analyses than milder assessments of this construct. However, although solo status situations are, especially for minority groups, not uncommon (e.g., being the only Black student in a school class of White students, or being the only female in a boardroom of men), this scale could potentially restrict findings to high intergroup threat situations. Therefore, future studies should further examine optimal ways of measuring this key construct, which would allow researchers to replicate the present findings using more generic measures of intergroup anxiety. Moreover, the role of other postulated mediators, such as the inclusion of the other in the self (Aron, Aron, Tudor, & Nelson, 1991), the promotion of favorable norms for outgroup contact (Jetten, Spears, & Manstead, 1996), or the improvement of outgroup knowledge (White & Abu-Rayya, 2012), remains unclear. It is worth noting, however, that intergroup anxiety is the only mediator that is theorized to underlie both extended contact as well as electronic contact effects and, therefore, seems to be of particular relevance compared to other mediators that were proposed either for extended contact or for electronic contact. Nonetheless, the role of other mediators from indirect contact to direct contact should be subject to refinement and progress in future research.

A second limitation refers to the fact that we considered empirically only two forms of indirect contact, namely extended contact (Studies 1 to 5) and electronic contact (Study 3), while the role of other forms of indirect contact (e.g., vicarious or parasocial contact) remains

untested. In line with our theoretical reasoning, we expect that our revealed findings would be similar for different forms of indirect contact which, however, still needs to be confirmed in future research.

A third limitation of this paper concerns the under-examined minority perspective. Our datasets included either no minority group members (Study 1), an insufficiently large minority group sample (Study 5), or focused on a setting without a classic majority-minority schism (Studies 3 and 4). Only Study 2 allowed us to systematically compare the majority and minority groups across different countries, which replicated the hypothesized effects but also revealed some interesting group differences (as discussed above). However, even in Study 2 this analysis is limited to the extent that we had to pool different minority groups into one overarching minority group, which was necessary due to the applied network approach (i.e., the number of members from the particular minority groups would have become too small in the class-specific networks to conduct meaningful analyses). More research that considers the perspective of the minority group, ideally by differentiating between different groups with traditional survey data or within larger social networks, will provide valuable and urgently needed insights at a time of increasingly diverse societies.

A final limitation of the present paper is that we provide no experimental test of our primary hypothesis by manipulating the amount of indirect contact to study its impact on future direct contact in order to test the causality of this link. A challenge of future studies that aim to experimentally replicate this effect will be to capture the longitudinal process over a long-term perspective, as realized in our paper by using a one-year (Studies 2 to 4) or two-year lag (Studies 1 and 5), respectively. This is important because it needs time for contact to have an effect in order to reduce intergroup anxiety and, thereafter, to positively affect intergroup relations (Pettigrew, 1998).

To conclude, indirect contact does encourage individuals to take up direct contact, while a reduction in intergroup anxiety mediates this effect in most models. This consistent pattern of findings confirms something that seems to us to have been implicit in its original formulation (e.g., Wright et al., 1997): indirect contact is a stepping stone to direct, face-to-face contact. Hence indirect contact should be increasingly incorporated into interventions aimed at improving intergroup relations. Especially in (post-)conflictual settings where it is likely that individuals have on average higher levels of intergroup anxiety, a two-pronged contact-based approach that additionally uses the anxiety-reducing effect of indirect contact has unrealized potential in interventions to reduce intergroup conflict.

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Footnotes

¹One other item related to the current analysis assessed intergroup contact at work ("How often do you have personal contact with foreigners at work?" rated from 1 = *never* to 4 = *often*), which was not considered because of the more obligatory nature of this type of contact. Items unrelated to the current analysis assessed intergroup attitudes, the importance of intergroup contact, social dominance orientation, and right wing authoritarianism. The data are due to be made publically available this year (see: goo.gl/AM4Ef3). Related papers based on this dataset are Christ et al. (2010, Study 1) and Christ et al. (2014, Study 2a). Both studies differ from the present paper because they analyzed contact effects on a different outcome (i.e., intergroup attitudes).

²For direct contact in the neighborhood (EC_{w1} to DC_{w2} [$b = .28, p < .001$], DC_{w1} to EC_{w2} [$b = .07, p = .004$]); for pleasant outgroup encounters (EC_{w1} to DC_{w2} [$b = .17, p < .001$], DC_{w1} to EC_{w2} [$b = .07, p = .045$]). For the complete model see supplementary online materials.

³Other items related to the current analysis assessed intergroup contact in schools ("How often do you spend time during breaks at school with students from an [OUTGROUP] background?" rated from 1 = *never* to 5 = *every day*) and intergroup contact in the neighborhood ("How often do you spend time in your neighborhood with people from an Asian or Asian British background?" rated from 1 = *never* to 5 = *every day*), which were not considered because they were only assessed at wave 1. Items unrelated to the current analysis include migration-specific characteristics and core dimensions of integration. A complete overview with detailed information of all measures as well as papers that are unrelated to the present research can be found online (see: www.cils4.eu). Related papers based on this dataset are Schmid et al. (2017, Study 5) and Wölfer et al. (2016, Study 1). Both studies differ from

the present paper because they analyzed cross-sectional contact effects on a different outcome (i.e., intergroup attitudes).

⁴Given the restricted network nomination procedure (that asked participants to nominate their five best friends) as well as the relatively small network boundary (i.e., school class), a considerable proportion of the sample has no ingroup members among their five best friends in class (22%), which makes it difficult to estimate their extended contact score. In the main analysis, we nonetheless decided to consider those participants with a '0' as their extended contact score, but this conservative coding procedure masks the hypothesized positive association between extended and direct contact when not controlling for the number of ingroup friends. This arises, because participants with no ingroup friends, and consequently the lowest possible extended contact score, are likely to have more outgroup friends instead. However, when using only participants with at least one ingroup friend ($N = 5,142$), our results remain stable using the same cross-lagged model, even when controlling for the number of ingroup friends (EC_{w1} to DC_{w2} [$b = .26, p < .001$], DC_{w1} to EC_{w2} [$b = .10, p < .001$]) or without controlling for the number of ingroup friends (EC_{w1} to DC_{w2} [$b = .27, p < .001$], DC_{w1} to EC_{w2} [$b = .09, p < .001$]).

⁵This dataset includes no other items related to the present analyses. Items unrelated to the current analysis assessed social identity, negative contact, empathy, norms, pro-diversity beliefs, deprovincialization, intergroup attitudes, intergroup trust, and intergroup stereotypes. Related papers based on this dataset are Hughes et al. (2013, 2015). Both studies differ from the present paper because they analyzed cross-sectional mean differences in intergroup relations between students from different religious communities (Hughes et al., 2013) or socio-economic backgrounds (Hughes et al., 2015).

⁶For direct contact in school (EC_{w1} to DC_{w2} [$b = .10, p < .001$], $EleC_{w1}$ to DC_{w2} [$b = .07, p = .004$]), DC_{w1} to EC_{w2} [$b = .16, p < .001$], DC_{w1} to $EleC_{w2}$ [$b = .27, p < .001$], EC_{w1} to

IGA_{w2} [$b = -.02, p = .499$], EleC_{w1} to IGA_{w2} [$b = -.01, p = .662$], IGA_{w1} to DC_{w2} [$b = -.02, p = .218$]); for direct contact outside of school (EC_{w1} to DC_{w2} [$b = .13, p < .001$], EleC_{w1} to DC_{w2} [$b = .13, p < .001$]; DC_{w1} to EC_{w2} [$b = .15, p < .001$], DC_{w1} to EleC_{w2} [$b = .21, p < .001$]; EC_{w1} to IGA_{w2} [$b = -.01, p = .675$], EleC_{w1} to IGA_{w2} [$b = .01, p = .714$], IGA_{w1} to DC_{w2} [$b = -.06, p = .004$]). For the complete model see supplementary online materials.

⁷Other items related to the current analysis assessed intergroup contact at work ("About how many of your work colleagues are OUTGROUPERS?" rated from 1 = *none* to 6 = *all*; "Overall, how frequently do you interact with OUTGROUPERS at work (e.g., working together, chatting during breaks)?" rated from 1 = *never* to 5 = *very often*; "Is the contact generally friendly or unfriendly?" rated from 1 = *definitely unfriendly* to 5 = *definitely friendly*), which were not considered because of the more obligatory nature of this type of contact. Items unrelated to the current analysis assessed negative contact, perceived threat, experience of violence, social identification, intergroup trust, intergroup emotions, positive and negative action tendencies, and intergroup attitudes. Related papers based on this dataset are Christ et al. (2010, Study 2), Tausch et al. (2011), Schmid et al. (2014, Study 2), Schmid et al. (2008), and Tausch et al. (2010, Study 4). All studies differ from the present paper because they analyzed either cross-sectional associations (Schmid et al., 2008; Tausch et al., 2011) or longitudinal contact effects on different outcomes (Christ et al., 2010, Study 2: behavioral intentions & attitude certainty; Schmid et al., 2014, Study 2: aggression; Tausch et al., 2010, Study 4: attitude generalization).

⁸When estimating the model including both significant and non-significant effects, we revealed almost identical results, with extended contact at wave 1 predicting intergroup anxiety at wave 2 ($b = -.22, p = .001$), intergroup anxiety at wave 1 predicting direct contact at wave 2 ($b = -.10, p = .038$), and the resulting longitudinal indirect effect of extended contact at wave 1 predicting direct contact at time 2 ($b = 0.022, p = .039$ [one-tailed significance])

with the bootstrap confidence interval (95%, 5000 bootstrap samples) excluding zero (.003 – .056).

⁹For quantity of neighborhood contact (EC_{w1} to IGA_{w2} [$b = -.19, p = .001$], IGA_{w1} to DC_{w2} [$b = -.07, p = .204$]; DC_{w1} to EC_{w2} [$b = .14, p = .045$]; DC_{w1} to IGA_{w2} [$b = -.16, p = .003$]; EC_{w1} via IGA to DC_{w2} [$b = 0.014, p = .117$]); for quality of neighborhood contact (EC_{w1} to IGA_{w2} [$b = -.19, p = .003$], IGA_{w1} to DC_{w2} [$b = -.20, p = .003$]; DC_{w1} to IGA_{w2} [$b = .17, p = .005$]; EC_{w1} via IGA to DC_{w2} [$b = 0.038, p = .043$]). For the complete model see supplementary online materials.

¹⁰Other items related to the current analysis assessed intergroup contact at work ("At work, how often do you engage in conversation with people who are not themselves native Germans or whose parents are not from Germany?" rated from 1 = *never* to 5 = *daily*), which were not considered because of the more obligatory nature of this type of contact. Items unrelated to the current analysis assessed empathy, perceived threat, social identity, social cohesion, social capital, diversity beliefs, social norms, and intergroup trust. Related papers based on this dataset are Christ et al. (2014, Study 2b) and Schmid et al. (2014, Study 1). Both studies differ from the present paper because they analyzed contact effects on different outcomes (Christ et al, 2014, Study 2b: intergroup attitude; Schmid et al., 2014, Study 1: attitude generalization).

¹¹For the number of weak ties (EC_{w1} to IGA_{w2} [$b = -.07, p < .001$], IGA_{w1} to DC_{w2} [$b = -.06, p < .001$]; DC_{w1} to EC_{w2} [$b = .18, p < .001$]; EC_{w1} via IGA to DC_{w2} [$b = 0.004, p = .006$]); for the number of strong ties (EC_{w1} to IGA_{w2} [$b = -.07, p < .001$], IGA_{w1} to DC_{w2} [$b = -.05, p = .136$]; DC_{w1} to EC_{w2} [$b = .15, p < .001$]; EC_{w1} via IGA to DC_{w2} [$b = 0.003, p = .086$]). For the complete model see supplementary online materials.

Tables

Table 1. Descriptives and correlations of relevant variables from Studies 1 to 5

STUDY 1 (<i>N</i> = 560)								
	Descriptives		Zero-order Correlations					
	<i>M</i>	<i>SD</i>	1	2	3	4		
1 Direct Contact - wave 1	1.83	0.68		.57	.65	.43		
2 Extended Contact - wave 1	1.98	0.59			.51	.52		
3 Direct Contact - wave 2	1.88	0.64				.55		
4 Extended Contact - wave 2	2.02	0.56						
STUDY 2 (<i>N</i> = 6,600)								
	Descriptives (Majority)		Descriptives (Minority)		Zero-order Correlations			
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	1	2	3	4
1 Direct Contact - wave 1	1.59	0.94	3.02	1.53		.24	.51	.15
2 Extended Contact - wave 1	1.42	0.89	1.95	1.69	-.02		.18	.34
3 Direct Contact - wave 2	1.57	0.91	3.08	1.38	.52	-.08		.16
4 Extended Contact - wave 2	1.19	0.90	1.62	1.65	-.01	.43	-.05	

STUDY 3 (N = 1,593)										
	Descriptives		Zero-order Correlations							
	<i>M</i>	<i>SD</i>	1	2	3	4	5	6	7	8
1 Direct Contact - wave 1	1.93	1.11		.54	.56	-.15	.59	.40	.49	-.13
2 Extended Contact - wave 1	2.71	1.38			.46	-.14	.41	.45	.37	-.11
3 Electronic Contact - wave 1	1.99	1.34				-.18	.43	.36	.48	-.12
4 Intergroup Anxiety - wave 1	1.68	0.95					-.12	-.13	-.15	.34
5 Direct Contact - wave 2	2.06	1.14						.55	.63	-.17
6 Extended Contact - wave 2	2.88	1.35							.48	-.14
7 Electronic Contact - wave 2	2.15	1.40								-.15
8 Intergroup Anxiety - wave 2	1.63	0.94								
STUDY 4 (N = 404)										
	Descriptives		Zero-order Correlations							
	<i>M</i>	<i>SD</i>	1	2	3	4	5	6		
1 Direct Contact - wave 1	2.37	1.05		.50	-.29	.57	.43	-.29		
2 Extended Contact - wave 1	2.77	1.29			-.26	.44	.51	-.35		

Figures

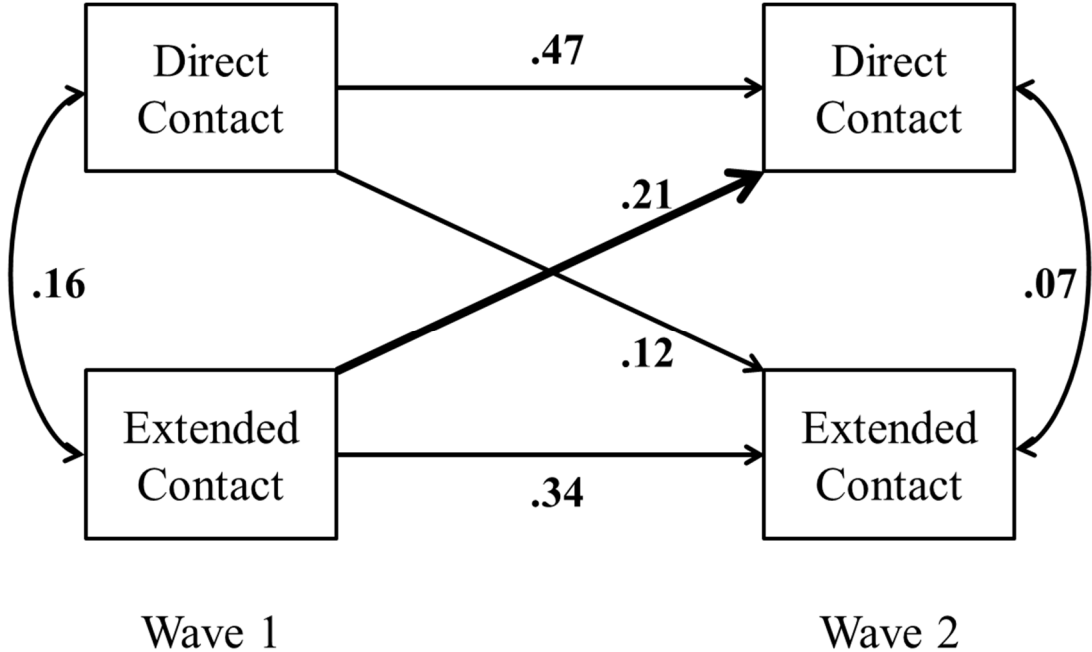


Figure 1. Cross-lagged model controlling for sex, age, education, and contact opportunity (Study 1). Double-headed curved arrows denote covariances between constructs. The relevant link from extended to direct contact is bold-marked.

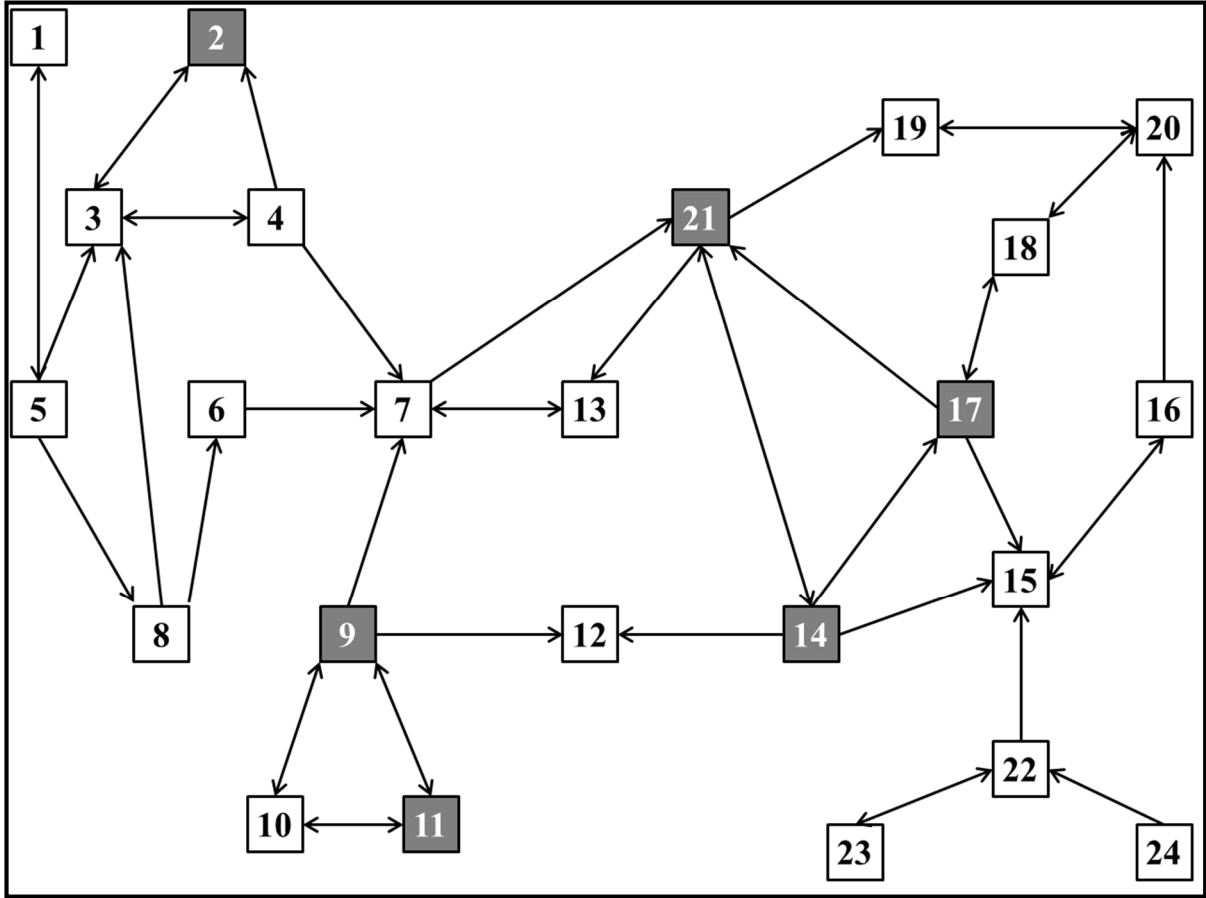


Figure 2. Sample social network that structures students via friendship relationships; colors of boxes indicate majority (white) and minority status (grey), while arrows of ties indicate the nominated friend.

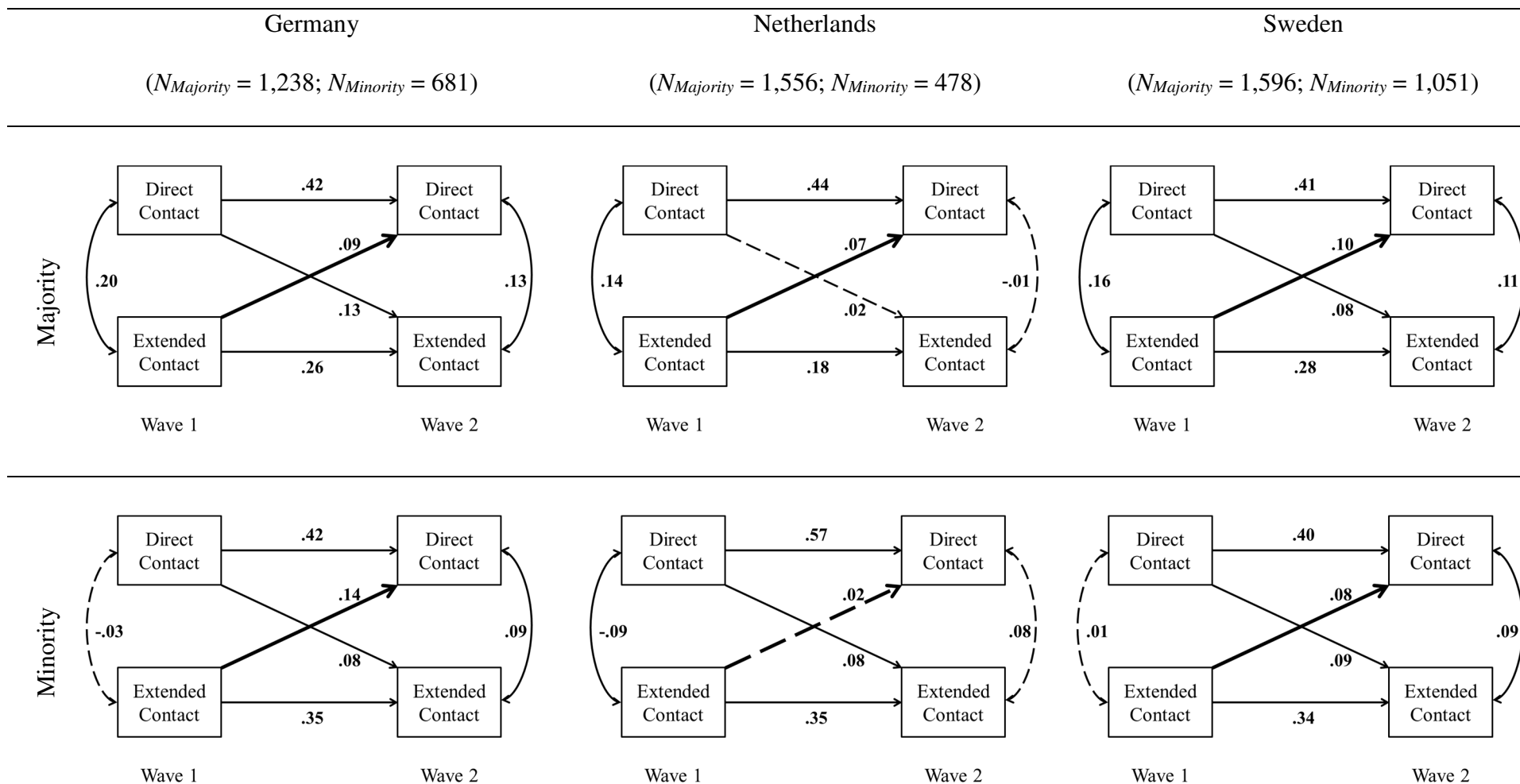


Figure 3. Cross-lagged models controlling for sex, age, and ingroup friends at waves 1 and 2 (Study 2). Double-headed curved arrows denote covariances between constructs. Dashed lines indicate non-significant estimates. The relevant link from extended to direct contact is bold-marked.

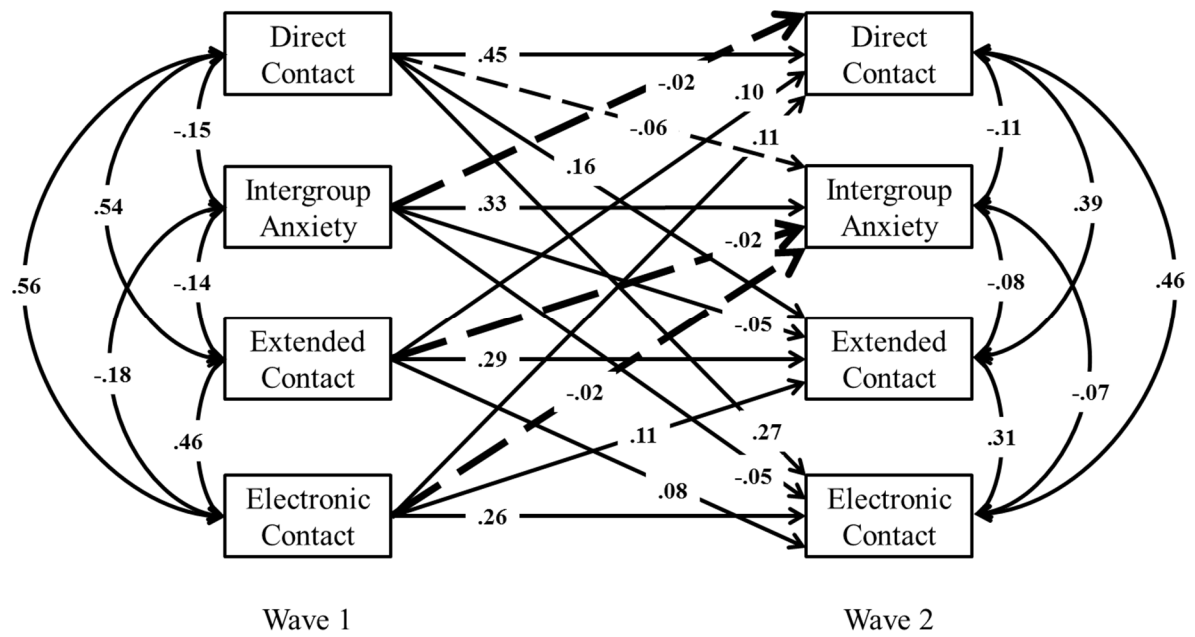


Figure 4. Cross-lagged models controlling for sex, age, religious community, and contact opportunity (Study 3). Double-headed curved arrows denote covariances between constructs. Dashed lines indicate non-significant estimates. The relevant links testing the longitudinal indirect effect of indirect contact to direct contact are bold-marked.

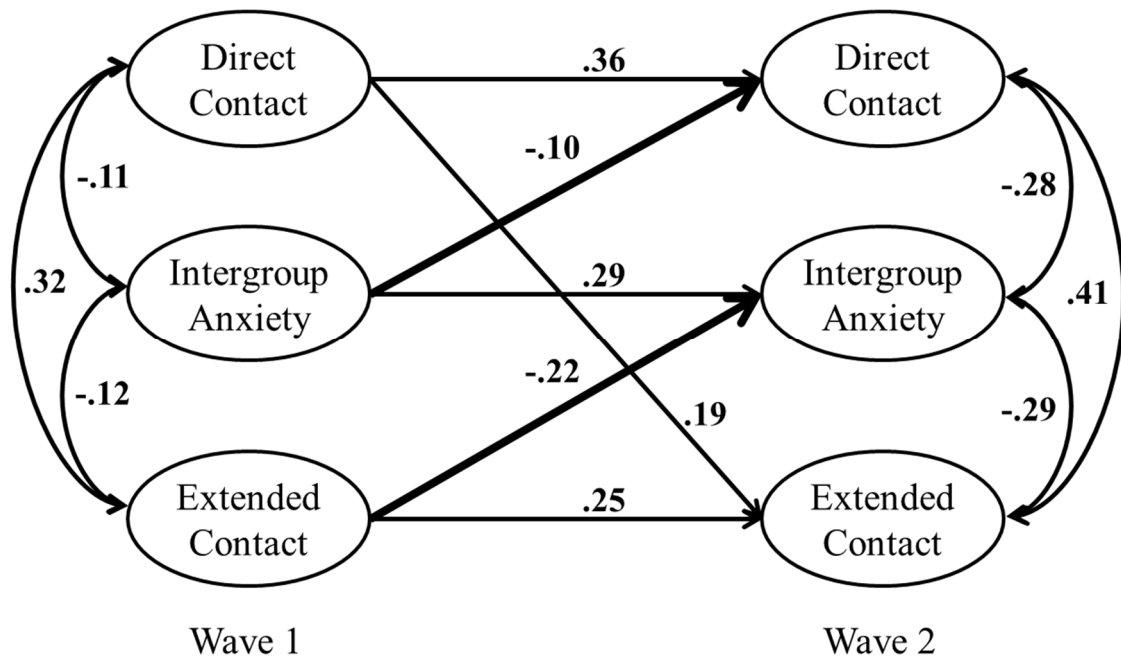


Figure 5. Latent cross-lagged model controlling for sex, age, education, religious community, and contact opportunity (Study 4). Double-headed curved arrows denote covariances between constructs. Only significant paths ($p < .05$) are shown. The relevant links testing the longitudinal indirect effect of extended contact to direct contact are bold-marked.

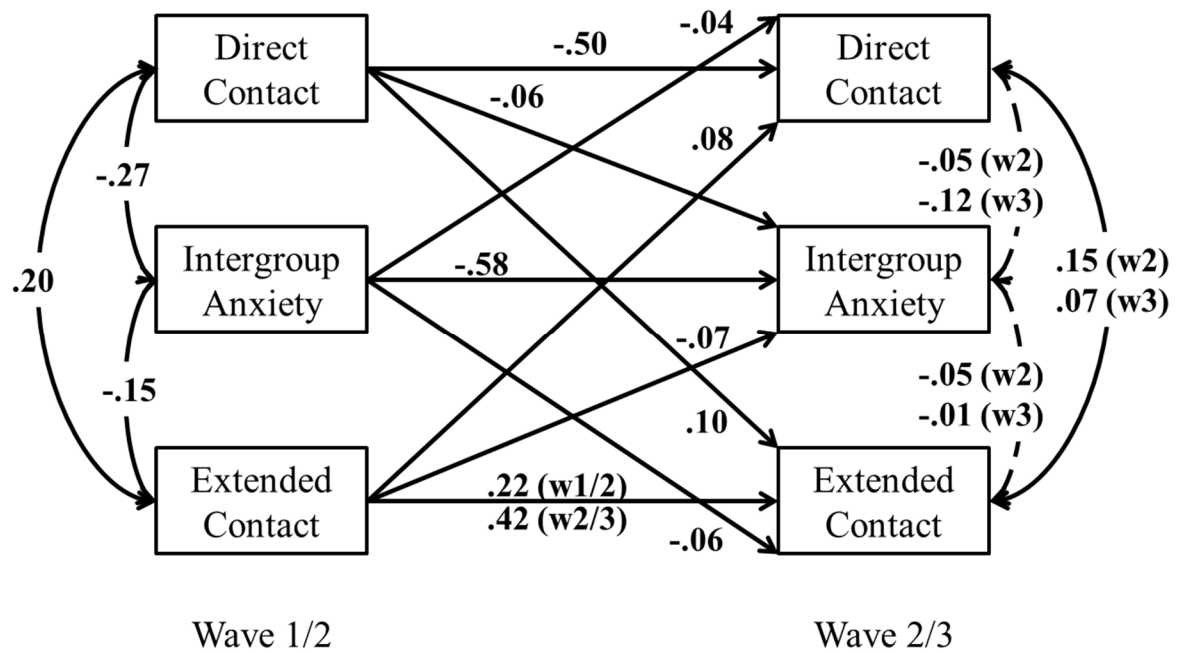


Figure 6. Cross-lagged model controlling for sex, age, education, and contact opportunity (Study 5). Paths estimate the link between constructs at an earlier wave (i.e., wave 1 or 2) and at a subsequent wave (wave 2 or 3). The autocorrelation of EC differs between waves 1 to 2 and 2 to 3, as the autoregressive effect was unconstrained due to lack of stationarity. Covariances of wave 1 are presented on the left side, covariances of waves 2 and 3 on the right side. Dashed lines indicate non-significant estimates (nb: the covariance between direct contact and intergroup anxiety is non-significant at wave 2, but significant at wave 3).