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Past research has shown that intergroup contact can be a promising intervention to improve intergroup relations and that contact-based interventions might be most effective during adolescence. In postconflict Northern Ireland, widespread residential segregation and a largely separate school system limit opportunities for intergroup contact between adolescents from the Catholic and Protestant communities. We evaluated whether a large-scale intervention to facilitate intergroup contact between students attending separate schools (the 'Shared Education' program) improves a range of outcomes relevant for intergroup relations in Northern Ireland. We conducted a 5-wave longitudinal, quasi-experimental study that followed a large sample of school students ($N = 5{,}159$, $M_{age} = 12.4$, age range: 10–14 years; 2,988 girls, 2,044 boys) from 56 predominantly Catholic or Protestant schools from sixth to tenth grade. We compared the developmental trajectories of students who, in 9th (14–15 years) and 10th (15–16 years) grade, shared some classes with students from the other community, as part of the program, to students who did not. We found that participating in shared classes had a medium-size, positive effect on the amount of intergroup contact students had outside of class, and small, positive effects on students' outgroup attitudes, outgroup trust, and intergroup empathy (but not on their intergroup anxiety, future contact intentions, deprovincialization, or multicultural beliefs). Our findings show that a school-based program of shared education can provide a viable and effective intervention to facilitate intergroup contact, improve intergroup relations, and foster social integration among adolescents at a large scale in a postconflict society.

Keywords: intergroup relations, intergroup contact, intervention, adolescence, Northern Ireland

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Following a conflict that lasted more than thirty years, the signing of the 1998 Belfast/Good Friday Agreement and the associated demilitarization made Northern Ireland a more normal society whose citizens enjoy relative peace. Yet, tensions between the Catholic and Protestant communities remain, as do sectarian crime and violence (Police Service of Northern Ireland, 2020). Past research has shown that intergroup contact can be a promising intervention to improve intergroup relations. Adolescents, who were born into the relative peace following the 1998 Agreement, would be well placed to build bridges across the sectarian divide. Furthermore, adolescence is the developmental period in which contact-based interventions might be particularly effective and important for setting adolescents on a developmental trajectory toward positive intergroup relations in early adulthood and beyond (Wölfer et al., 2016). In Northern Ireland, widespread residential segregation and a largely separate school system limit opportunities for intergroup contact between adolescents from the different communities. The 'Shared Education' program is a nation-wide school-based intervention that brings together students from religiously separate schools in shared classes to facilitate intergroup contact. In this paper, we evaluate whether participating in Shared Education improves a range of outcomes relevant for intercommunal relations in postconflict Northern Ireland. In so doing, this study provides a rare test of the viability of large-scale applications of intergroup contact research in real-life settings.

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Intergroup Relations in Postconflict Northern Ireland

Between the late 1960s and 1998, the conflict between the largely Catholic Nationalists, who want Northern Ireland to become part of a united and independent Ireland, and largely Protestant Unionists, who want Northern Ireland to remain part of the United Kingdom, resulted in over 3,600 deaths (Tonge, 2002). Even though major hostilities ended with the 1998 Agreement, recent unrest shows that this peace is relative and fragile ("Northern Ireland's unhappy centenary," 2021). A crucial characteristic of Northern Irish society that helps explain many aspects of the conflict is the extreme degree to which the Protestant and Catholic communities are segregated (Hewstone et al., 2005; Murtagh, 2002). During the conflict, residential segregation increased as a direct result of population movements in response to intimidation, as families moved from religiously mixed areas into safe havens dominated by their coreligionists. As a result, just over half of all wards had 90% or more residents from only one community in 2001; a figure that had decreased to just over one third by 2011 (Nolan, 2013) when our study began. Residents of single-community neighborhoods continue to live largely parallel lives as they tend to avoid areas occupied by the other community in their everyday lives (Dixon et al., 2020).

Segregation limits opportunities for contact between the separate communities. Formulated as an argument for ending legal segregation in the United States, the 'contact hypothesis' (Allport, 1954; Clark, 1953) states that bringing members of different groups together will, under the right conditions, help to break down stereotypes, change attitudes, and improve intergroup relations. Since its initial formulation, thousands of studies have found intergroup contact—and, even more so, cross-group friendship—to be associated with more favorable attitudes toward outgroup members (for meta-analyses, see Davies et al., 2011; Pettigrew & Tropp, 2006). This evidence makes intergroup contact an important tool for resolving intergroup conflict (Al Ramiah & Hewstone, 2013). In Northern Ireland, however, residential segregation, alongside personal experiences during the conflict, prevents many adults from forming relationships with members of the other community.

Intergroup Contact in Adolescence

Adolescents, who were born after the 1998 Agreement, have grown up in a more peaceful society than their parents and grandparents. Theories of social-cognitive development suggest that children are receptive to group processes that can result in them forming prejudices at an early age (Nesdale, 2004; Rutland et al., 2010), a process that is likely accelerated by intractable conflict (Bar-Tal et al., 2017). Studies of the developmental trajectories of intergroup attitudes suggest that prejudices are already formed in early childhood, peak in middle childhood (5–7 years), and decrease somewhat in late childhood (8–10 years; Raabe & Beelmann, 2011). Longitudinal studies show that, while prejudice remains, on average, stable in adolescence (10–19 years), interindividual differences grow stronger in this developmental period (Crocetti et al., 2021). Among those differences, this research found intergroup friendship to be associated with decreasing prejudice.

Adolescence is the developmental period in which intergroup contact might be most effective in fostering positive intergroup attitudes. The 'impressionable years hypothesis' (Krosnick & Alwin, 1989) suggests that attitudes develop at a younger age before they stabilize in adulthood. School years are a crucial phase for shaping intergroup relations in later life. In this phase, adolescents: form an ethnic identity (French et al., 2006); perceive peer relationships, which socialize outgroup attitudes (Bracegirdle et al., in press), as more and more important (Brechwald & Prinstein, 2011); and become more aware of, and receptive to, group norms (Abrams & Rutland, 2008) which increases the potential for intergroup bias (Rutland et al., 2010). Recent longitudinal research followed a large sample of Swedish adolescents and young adults, aged 13 to 26 years, and found that intergroup contact during adolescence is particularly relevant, perhaps even necessary, for developing favorable intergroup attitudes in adulthood (Wölfer et al., 2016; Study 2). Adolescents in postconflict Northern Ireland might therefore be especially well placed to build relational bridges across the sectarian divide and to help resolve intergroup tensions.

Diverse school environments can provide adolescents from segregated neighborhoods with opportunities for intergroup contact that they would not otherwise have (Birtel et al., 2020). In Northern Ireland, however, the largely parallel education system for Catholics and Protestants means that, at the time of our study, only 7% of students attended schools established to give parents the choice to educate their children in an integrated setting comprising students from both communities (Nolan, 2013); most students attended religiously separate primary (elementary) and secondary (middle/high) schools (for a comparison with the American school system, see Table 1).[1] Research has found that, in line with other research on intergroup contact, students at the few integrated schools tend to have more positive intergroup attitudes than students at separate schools (Hughes et al., 2013; McGlynn et al., 2004; Stringer et al., 2009). Support for this separate school system comes from both communities even though, in surveys, the majority of the population claims to support integrated education (Devine & Robinson, 2018). However, some church and community leaders on both sides oppose further growth of the integrated sector (Schiaparelli et al., 2015) which, alongside logistical challenges, means that, in the short term, school integration is not a viable solution for fostering intergroup contact between adolescents from the different communities.

Contact-Based Interventions

Where segregation or conflict precludes intergroup contact in everyday life, researchers and practitioners have sought to improve intergroup relations by facilitating contact between groups in dedicated interventions. Recent meta-analyses have reviewed the evidence for the effectiveness of contact-based interventions in real-life settings. Beelmann and Heinemann (2014) examined

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[1] In Northern Ireland, young people attending schools are referred to as 'pupils' rather than 'students' and attend 'primary' (elementary) and 'secondary' (middle and high) schools (for a comparison with the American system, see Table 1). For the readership of this journal, we use the more universal terminology of (secondary-school) students and refer to U.S. grades in the remainder of this article.

Table 1

Comparison of Northern Irish and U.S. American Secondary-School Grades

| Ages | Northern Ireland | | United States | |
	Grade	School	Grade	School
11–12	Year 8	Secondary School	Sixth	Junior High/Middle School
12–13	Year 9	Secondary School	Seventh	Junior High/Middle School
13–14	Year 10	Secondary School	Eighth	Junior High/Middle School
14–15	Year 11	Secondary School	Ninth/Freshman	High School
15–16	Year 12	Secondary School	10th/Sophomore	High School

interventions to reduce prejudice among children and adolescents, and found that interventions based on direct contact experiences were among the most effective. Lemmer and Wagner (2015) similarly found that contact-based interventions were effective at reducing ethnic prejudice in real-world settings. Paluck et al. (2019) considered only the most rigorously conducted studies of intergroup contact that featured random assignment and delayed outcome measures. Using these criteria, they found 27 experiments to include in their review and reported that 24 of them found positive effects, with an average medium effect size (that is, experiencing contact reduced prejudice, on average, by .39 standard deviations). Few of the studies reviewed in these meta-analyses, however, evaluated interventions with more than a few hundred participants.

Recent studies have evaluated a *large-scale* intervention that promotes intergroup contact and reaches a substantial proportion of the target population. The National Citizen Service, a large-scale contact-based intervention reaching one in six 15- to 17-year-olds in England and Northern Ireland, brings together adolescents from different ethnic and socioeconomic backgrounds for a three- to four-week-long youth engagement program. Research found that participating in the intervention was associated with an increase in intergroup contact 4–6 months later (Laurence, 2019). Evidence for the intervention's effectiveness in improving intergroup relations across various outcomes was, however, mixed and suggested that, if it had an effect on intergroup attitudes, that effect was small (Laurence, 2020; Reimer et al., 2021). One explanation for the program's small and mixed effects might be that it is too short and too far removed from participants' everyday life for cross-group friendships to form that outlast the program. Pettigrew (1998) argued that 'friendship potential' (that is, the extent to which a contact situation provides participants with opportunities to become friends) is an essential condition for cross-group friendships (the most effective form of intergroup contact; Davies et al., 2011) to form, and thereby reduce prejudice. For intergroup contact to prove a viable strategy for policymakers, there is a need for research on interventions that (1) foster in-depth intergroup contact, (2) over an extended period, and (3) reach a substantial proportion of the population.

Shared Education

In 2007, the Shared Education program was established, funded by an international charity, to create opportunities for intergroup contact between children from Catholic and Protestant communities in Northern Ireland. The program brings together children who attend religiously separate schools in regular shared classes with children from the other community. Unlike integrated schooling, Shared Education allows for the retention of separate schools for Catholic and Protestant students, but still connects students from separate schools via shared learning (Gallagher, 2016). Shared Education spans a diverse range of shared activities. At secondary-school level, the focus of our study, some schools collaborate to offer a wider range of academic subjects. Students attend shared classes for subject options not available in their own school. Contact hours for participating students range from a few hours a week within a single year to much more extensive opportunities for engagement across three or more years (Loader & Hughes, 2019). Curriculum-based learning is often supplemented with shared extracurricular opportunities. By June 2019; 716 schools (61% of all schools in Northern Ireland) were involved in Shared Education, accounting for 87,385 students. Of the 716 participating schools, 120 were secondary- schools (Department of Education, 2020). In summary, the Shared Education program is a school-based intervention that provides children and adolescents with opportunities for sustained intergroup contact and reaches a substantial proportion of the target population.

Shared Education, as a school-based program, is a promising contact-based intervention. First, as reviewed earlier, secondary-school years are both a crucial period for shaping intergroup relations in later life and the period in which intergroup contact experiences are particularly important for developing favorable intergroup attitudes. Second, schools are important environments in which to promote intergroup contact (for reviews, see Aboud et al., 2012; Beelmann & Heinemann, 2014; Wölfer et al., 2018), in part because they meet many of the 'optimal' conditions for intergroup contact (Allport, 1954; for a meta-analysis, see Pettigrew & Tropp, 2006). Third, although admission to the program is administered centrally and voluntary at the school level, students do not, with rare exceptions, have a choice in whether to participate. By ruling out selection bias, research can provide a clearer indication of the causal relationship between intergroup contact and prejudice reduction. In addition, limiting students' choice of whether to enter mixed environments means that Shared Education reaches students with fewer prior contact experiences and less favorable initial attitudes who might not have chosen to participate.

Outside of Northern Ireland, the Shared Education model has been adapted for, but not evaluated in, other divided societies, including Israel (Payes, 2013), North Macedonia, Kosovo, and Cyprus. In an initial evaluation in Northern Ireland, Hughes et al. (2012) compared the views of 577 students, between the ages of 12 and 18, from schools that either were, or were not, involved in

the Shared Education program. Students in schools involved in Shared Education reported, on average, higher levels of intergroup contact and of various other outcomes related to positive intergroup relations. Without random assignment or longitudinal data, however, this study could not establish whether these differences were due to the intervention or due to preexisting differences.

Present Research

We conducted a five-wave longitudinal, quasi-experimental study to evaluate whether Shared Education fosters social integration in postconflict Northern Ireland. We followed a large sample of students from 6th (Year 8) to 10th (Year 12) grade, some of whom went to schools that implemented Shared Education in 9th and 10th grade (Years 11 and 12). We sampled students from schools across the country that were either predominantly Catholic or predominantly Protestant. To assess the effectiveness of the intervention, we measured a wide range of outcomes relevant for intergroup relations in Northern Ireland.

Because admission to the program is administered centrally and is voluntary at the school level, it was not possible to use a study design with random assignment. Instead, we evaluated the intervention by using a longitudinal, quasi-experimental design in which participants first completed annual surveys over three years, at which point some were assigned to participate in Shared Education while others were not. We estimated how effective Shared Education was in fostering social integration by comparing students who participated in the program to those who did not (*student-level participation*), and by comparing schools that were involved in Shared Education to those that were not (*school-level involvement*). While the student-level analyses are a more direct test of the effectiveness of the intervention, the school-level analyses are also useful because they allow us to test whether the intervention would affect even students who did not themselves participate in shared classes. For example, based on the idea of 'extended contact' (Wright et al., 1997), it is possible that a student who has an ingroup friend (i.e., from the same religious group) who, in turn, has an outgroup friend (i.e., from the other religious group) would develop more positive attitudes toward the outgroup.

We considered eight outcomes relevant for intercommunal relations in postconflict Northern Ireland, which, based on their content and theoretical focus, fell into four groups. First, we evaluated whether, as intended, Shared Education fosters friendship and other contact experiences with outgroup members outside of school. We hypothesized that students who participated in shared classes would report both having more contact with outgroup members outside of classes and being more willing to engage in intergroup contact in the future. Measuring postintervention intergroup contact is important because it allows us to test whether in-class contact translates to out-of-class relationships—in other words, whether the intervention has 'friendship potential' (Pettigrew, 1998). If so, Shared Education could provide adolescents with the cross-community relationships that growing up with residential segregation and separate schooling would have deprived them off. Measuring willingness to engage in future contact is important because it allows us to test whether Shared Education makes it more likely that participants will continue to seek out

intergroup contact in early adulthood and beyond. Several studies, including some in Northern Ireland (e.g., Tam et al., 2009), have found that intergroup contact is associated with positive action tendencies (e.g., approaching the outgroup), and ratings of willingness for future contact (e.g., Shelton & Richeson, 2005; Wang et al., 2014).

Second, we evaluated whether, in line with most other research on intergroup contact (see Pettigrew & Tropp, 2006), Shared Education leads to more positive thoughts and feelings about outgroup members. We hypothesized that students who participated in shared classes would report both more favorable outgroup attitudes and greater outgroup trust. Trust, a psychological means to overcome uncertain social interactions by making benign assumptions about other people's behavior (Molm et al., 2000), is essential in intergroup behavior because, unlike attitudes, trust implies a willingness to engage in behavior that has potential costs. Several cross-sectional studies (e.g., Tam et al., 2009) and one longitudinal study (Kenworthy et al., 2016) in Northern Ireland indeed confirmed that intergroup contact is associated with greater outgroup trust.

Third, we evaluated whether Shared Education would affect two outcomes that are known to mediate the effects of intergroup contact on outgroup attitudes (for a meta-analysis, see Pettigrew & Tropp, 2008). We hypothesized that students who participated in shared classes would report both *more* intergroup empathy and *less* intergroup anxiety.

Empathy consists of two components, with beneficial effects on intergroup relations (Batson & Ahmad, 2009; Brown & Hewstone, 2005). Empathic concern, the affective component, is the capacity to experience affective reactions to the observed experience of others and involves emotions such as sympathy and compassion (Davis, 1994). Perspective taking, the cognitive component, describes the ability to see a situation from the outgroup's point of view (Aberson & Haag, 2007). Multiple cross-sectional studies (e.g., Cehajic et al., 2008; Hayward et al., 2017), including one study in Northern Ireland (Turner et al., 2013) and one longitudinal study (Swart et al., 2011), have found a positive association between intergroup contact and both affective and cognitive components of empathy.

Intergroup anxiety refers to the feelings of discomfort and nervousness that arise as a result of either experienced or anticipated interactions with an outgroup member (Stephan, 2014; Stephan & Stephan, 1985). In Northern Ireland, Paolini et al. (2004) found that having outgroup friends is associated with lower intergroup anxiety. Anxiety is associated with negativity toward future intergroup encounters (Mendoza-Denton et al., 2002), and avoidance of intergroup interactions (Kenworthy et al., 2016), but having outgroup friends can reduce anxiety and negative expectations of future outgroup contact interactions (Page-Gould et al., 2008). To the extent that Shared Education fosters empathy and reduces anxiety, it has the potential to, first, have an indirect effect on intergroup attitudes and, second, prepare adolescents for future intergroup contact experiences.

Fourth, we evaluated whether Shared Education would affect two outcomes that are closely related and assess whether the effects of the intervention go beyond attitudes, feelings, and future behaviors toward the specific ethno-political outgroup. We hypothesized that students who participated in shared classes would espouse a less narrow focus on the ingroup and a broader

multicultural orientation. 'Deprovincialization' (Pettigrew, 1997; 1998) refers to a distancing from, and reappraisal of, the ingroup and its norms and customs—and is thought to be one of the mechanisms by which contact improves intergroup relations. Multiculturalism and diversity beliefs are concepts that are closely related to deprovincialization (Lolliot, 2013). Multiculturalism involves acknowledging others' customs, rather than imposing those of one's own group, adopting a broader cultural perspective, and respecting other groups' values. Intergroup contact is associated with the development of multicultural attitudes (Verkuyten et al., 2010), and endorsement of such attitudes is, in turn, associated with more favorable outgroup attitudes (Levin et al., 2012; Richeson & Nussbaum, 2004; Verkuyten, 2005). Valuing diversity has also been shown to reduce discrimination against outgroups (e.g., Kauff & Wagner, 2012). Whereas the other outcomes are closely predicted by intergroup contact theory (that is, they are more proximal outcomes), the two broader outcomes are not tied to specific outgroups and are less closely predicted by intergroup contact theory (that is, they are more distal outcomes).

Method

Open Practices Statement

We report how we determined our sample size, all data exclusions, all manipulations, and all measures in the study. We made all data and analysis scripts required to reproduce our analyses, as well as a complete list of all items included in the survey, available online (https://osf.io/wdc5j/). This study was not preregistered. This study has been approved by the Research Ethics Committee of the School of Social Sciences, Education and Social Work, Queen's University Belfast ("The Longitudinal Impact of Cross Community Contact on Social Attitudes: Perceptions of Reconciliation and National Identity in Northern Ireland's Integrated and Segregated Schools").

Participants

We recruited 6,177 students (M_{age} = 12.4, age range: 10–15 years; 3,504 girls, 2,550 boys) from 69 schools from all regions of Northern Ireland (see Figure 1). Of these, 33 schools (n = 3,452) were predominantly Catholic (< 10% Protestant students), 23 schools (n = 1,707) were predominantly Protestant (< 10% Catholic students), and 13 schools (n = 1,018) were mixed (> 10% of both groups). As we were interested in whether the intervention would foster social integration among students in schools with few students from the other community, we included only participants (N = 5,159) from the 56 predominantly Catholic or Protestant schools in our analyses. Of all students, 1,561 (25%) received free school meals, which means that they or their parents received welfare benefits. As 25% of Northern Irish students were entitled to free school meals (Department of Education, 2013), our sample was broadly representative in terms of the students' socioeconomic status.

Procedure

In early 2010, we approached all postprimary schools in Northern Ireland to participate in the study. Of 217 schools, 58 schools agreed to participate. After data collection began in late spring of 2011, we again approached schools that had previously declined to participate or had not responded to earlier recruitment attempts. We thereby bolstered our sample by recruiting another 11 schools in 2012 and 2013. In addition, we encouraged schools who had previously participated to distribute information sheets to students who had not participated in the first wave of the study. In total, our sample included students from 69 schools in Northern Ireland (that is, 32% of all eligible schools).

From 2011 to 2015, we collected data at each participating school in late spring, that is, toward the end of each academic year. In most schools, students had the opportunity to complete our survey using an online survey platform. A few schools had requested pen-and-paper questionnaires that were distributed by teachers and returned to us in sealed envelopes. Some schools gave all students in the relevant year group the opportunity to participate, while other schools restricted participation to only one class of students. In total, our sample included responses from 6,052 students, though not all students participated in every wave of the survey.[2] For example, 3,284 students participated in Year 8. Of those, 2,055 (63%) also participated in Year 9, in addition to another 702 Year 9 students who participated for the first time in that year. Across the five waves, 63% (Year 9), 54% (Year 10), 48% (Year 11), and 40% (Year 12) from previous years returned to participate in the study (see Figure 2). To examine whether dropout was systematic, we ran a logistic regression model to estimate the extent to which the probability of a student not participating in a given year depended on their responses to the eight outcome variables in the preceding year (for details, see online supplemental materials). As we found that the odds of a student not participating were not substantially related to any of the outcome variables (.91 < OR < 1.08), we concluded that dropout was unsystematic with regard to the outcome variables considered in this study.
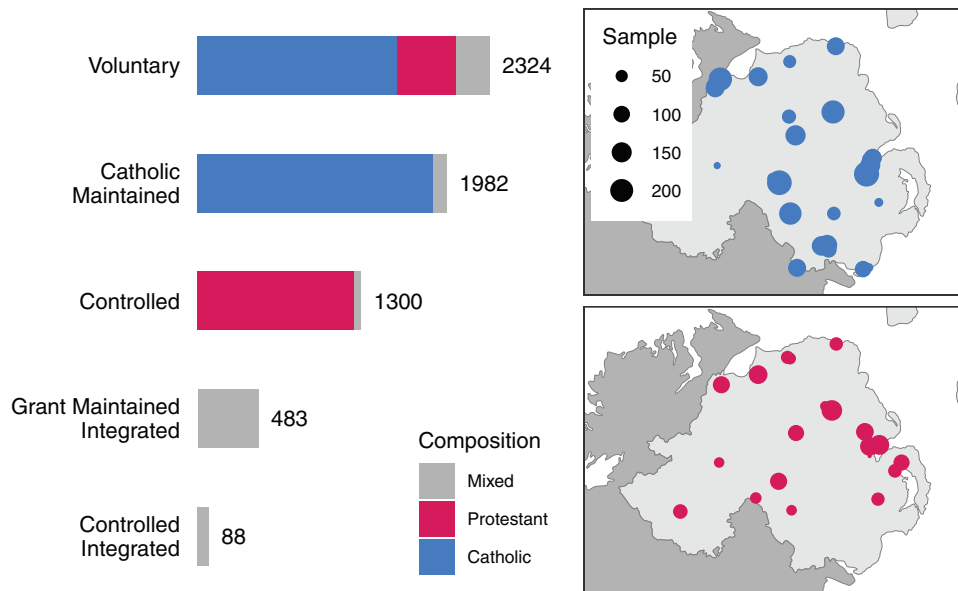
Measures

Table S2 in the online supplemental materials shows correlations, means, and standard deviations for all measures. Below, we report three reliability indices for outcome measures. For multi-item measures, we report McDonald's omega (ω) as a measure of internal consistency reliability (Dunn et al., 2014). For two-item measures, we instead report the Spearman-Brown statistic (ρ) as a measure of internal consistency reliability (Eisinga et al., 2013). For all measures, we also report Spearman's rank correlation coefficient (r) as a measure of test-retest reliability.[3] Items referred to either Catholic or Protestant people as the relevant outgroup when

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[2] This differs from the sample size of 6,177 students reported in the *Sample* section because the latter number includes students who responded only to measures not relevant to the present study.

[3] A conventional cut-off for acceptable internal consistency reliability is >.70, a condition that all measures in our study exceed. What counts as acceptable test–retest reliability depends on the stability of the construct that is being measured. In our study, we should expect test–retest reliabilities to be lower than internal-consistency reliabilities because our study had long inter-survey intervals, took place in a developmental period in which we should expect change over time, and included an intervention designed to effect change. In these circumstances, we consider $r \approx .50$ as evidence for good test–retest reliability.

Figure 1

School Composition, Management Type, and Geographic Distribution of the Students and Schools in Our Sample

Note. "Grant Maintained Integrated" and "Controlled Integrated" schools were established to give parents the choice to educate their children in an integrated setting comprising Catholic and Protestant students. "Voluntary" and "Catholic Maintained" schools tend to be predominantly Catholic while "Controlled" schools are predominantly Protestant. See the online article for the color version of this figure

a pparticipant indicated that they belonged to one of those communities, or to people from "other religious groups" when a pparticipant did not identify with either community.[4]

Predictors

We used responses to several items to determine *student-level participation* in Shared Education. In 9th grade (Year 11), we asked students whether any of their classes were regularly held at a school other than their own and, if so, at which school. In 10th grade (Year 12), we asked students whether they took classes at another school, whether students from another school were in any of their classes at school, and, in both cases, what the other school was called. We used data from the Department of Education to code whether, in each year, students had shared classes with students from another school that was dominated by students from the relevant outgroup. We defined student-level participation as whether students had shared classes with students from the relevant outgroup in *either* of the two years (1 = *yes*) or whether students had not shared classes with students from the relevant outgroup in *both* of the two years (0 = *no*). We defined *school-level involvement* in Shared Education as whether a school had not received funding for Shared Education (1 = *no involvement*; 23 schools, $N = 1,921$), had received funding for Shared Education in the past (2 = *past involvement*; 6 schools, $N = 574$), or did receive funding for Shared Education during the study (3 = *current involvement*; 27 schools, $N = 2,619$). Schools that had received funding in the past might still have been involved in Shared Education at the time of the study because schools might have continued the program without funding.
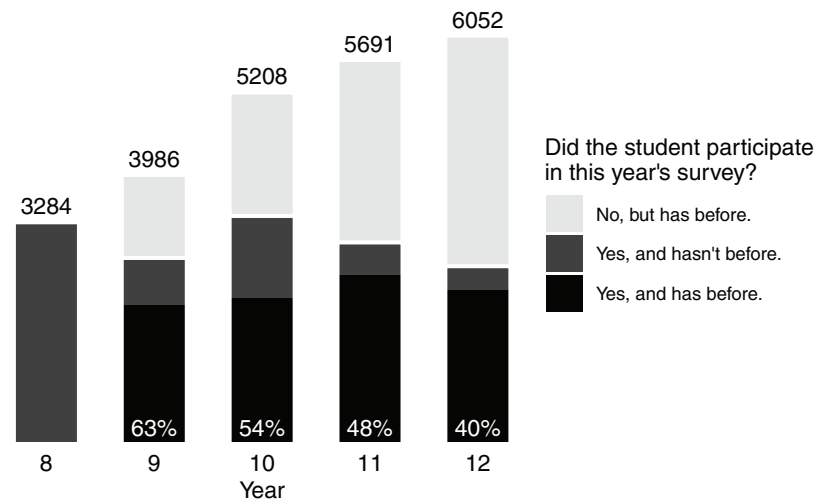
Outcomes

Participants completed a range of items in each annual survey. In this study, we considered all items that (a) measured attitudes and beliefs relevant to intergroup relations and (b) were measured in all five surveys. We considered 26 items that, when the questionnaire was constructed, were intended to measure twelve distinct constructs, each measured by two or three items. We used factor analysis to reduce items to a smaller number of outcome measures that measure distinct constructs (for details, see online supplemental materials). An eight-factor measurement model described participants' responses well, as intended, across all time points (CFI > .92, TLI > .91, RMSEA < .07). All resulting measures showed acceptable internal consistency ($\omega > .76$, $\rho > .75$) and test-retest ($r > .30$) reliability across surveys. All measures were built from items used in previous research, simplified and adapted for use on cross-community relations among schoolchildren in Northern Ireland.

We measured *intergroup contact* with five items: "During break, how often do you spend time with [outgroup] pupils?," "Outside of school, how often do spend your free time with [outgroup]

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[4] A growing number of young people in Northern Ireland identify as belonging to neither the Catholic nor the Protestant community—in numbers which cannot be explained by mixed marriage or a Black or minority ethnic background (ARK, 2015). This suggests that young people are making a conscious decision to move away from the traditional community identities which may define the area in which they are raised and the identity which their family may still hold (Blaylock et al., 2018; Ganiel, 2015).

Figure 2

The Numbers of Students Joining Each Year's Survey, Returning for Each Year's Survey, and Not Returning for Each Year's Survey As Well As the Proportion of Previous Participants Returning for That Year's Survey (As Percentages)

We measured *deprovincialization* with two items, both reverse scored (Lolliot, 2013): "We [ingroup] have customs that others should follow." and "We [ingroup] have better values than other groups" (1 = *strongly disagree*, 5 = *strongly agree*; $\rho > .75$, $r > .36$ for all years). We measured *multicultural-diversity beliefs* with five items (Kauff & Wagner, 2012; Verkuyten et al., 2010): "The mix of people from different groups makes Northern Ireland a better place", "It's fun to have children from all sorts of groups in my school", "All groups should be allowed to maintain their own traditions and culture", "We [ingroup] should have more respect for the culture of all other groups", and "We [ingroup] should do more to learn about the traditions and culture of different groups" (1 = *strongly disagree*, 5 = *strongly agree*; $\omega > .79$, $r > .47$ for all years).

children?," "Outside of school, how often do you send emails or link up on MSN with [outgroup] children?" (1 = *never*, 5 = *very often*), "About how many of your close friends are [outgroup]?" (1 = *none*, 5 = *almost all friends*), and "And in numbers, how many close [outgroup] friends do you have?" (1 = *none*, 5 = *ten or more friends*; $\omega > .86$, $r > .61$ for all years). We measured *willingness to engage in future contact* with two items (Tam et al., 2009): "Would you like to spend more time with [outgroup] children?" and "Would you like to have more [outgroup] friends?" (1 = *not at all*, 5 = *very much*; $\rho > .92$, $r > .52$ for all years).

We measured *outgroup attitudes* with four items (Al Ramiah et al., 2015; Wright et al., 1997): "How would you feel about having [outgroup] neighbours?," "How would you feel about having a [outgroup] boyfriend/girlfriend?" (1 = *very unhappy*, 5 = *very happy*), "How positive or negative do you feel about [outgroup] children?" (1 = *very negative*, 5 = *very positive*), and "How much do you like [outgroup] children?" (1 = *do not like them at all*, 5 = *like them very much*; $\omega > .88$, $r > .58$ for all years). We measured *outgroup trust* with two items (Brehm & Rahn, 1997): "How much do you trust [outgroup] children to be fair to you?" and "How much do you trust [outgroup] children to treat you well?" (1 = *do not trust them at all*, 5 = *trust them very much*; $\rho > .91$, $r > .43$ for all years).

We measured *intergroup empathy* with four items (Davis, 1994; see also Batson et al., 1997) about "getting on with [outgroup] children": "How well do think you understand how [outgroup] children view things?," "Do you find it easy to see things from the point-of-view of [outgroup] children?," "How much do you care about the problems faced by [outgroup] children?," and "Do you feel pity for [outgroup] children when you see them being treated unfairly?" (1 = *not at all*, 5 = *very much*; $\omega > .76$, $r > .47$ for all years). We measured *intergroup anxiety* (based on Stephan & Stephan, 1985) by asking participants to "think of a situation where [they] might meet [outgroup] children" and to respond to two items: "Would you feel nervous toward them?" and "Would you feel uncomfortable around them?" (1 = *not at all*, 5 = *very much*; $\rho > .76$, $r > .30$ for all years).

Analysis Strategy

Our study resulted in a dataset that is longitudinal (observations nested in participants), hierarchical (students nested in schools), has ordinal outcome variables (that is, items with ordered categorical response options), and contains missing responses. To analyze this dataset, we ran a series of multilevel ordinal-regression models in RStan (Stan Development Team, 2019) which estimated to what extent Shared Education affected how participants' responses to the outcome measures changed over time.

Bayesian inference involves choosing a likelihood function and prior distributions. The likelihood function links the observed data to one or more model parameters (e.g., regression coefficients) by expressing how likely the observed data would have been for different values of said model parameters. Prior distributions state how plausible different values of said model parameters are before considering the observed data. Bayesian inference applies Bayes' theorem to update prior distributions in light of the observed data to produce posterior distributions. Posterior distributions state how plausible different values of the model parameters are given the observed data. Our model derived the likelihood of the observed responses from a generalized linear model with a cumulative logit

link function. Our model assigned weakly informative prior distributions to model parameters.[5]

Our study used ordinal items to measure relevant outcomes. Analyzing ordinal data as if they were metric data—as is common practice in psychological research—risks distorting estimates of effect sizes and inflating rates of false-positive and false-negative findings (Liddell & Kruschke, 2018). Instead, we analyzed our dataset using cumulative ordinal regression models which estimated, for each observed response, how likely it was that each response option would be chosen by the participant (for an introduction, see Bürkner & Vuorre, 2019). For example, a model might predict that participants would choose each of five response options with the probabilities $\Pr(y = 1) = .01$, $\Pr(y = 2) = .05$, $\Pr(y = 3) = .49$, $\Pr(y = 4) = .34$, and $\Pr(y = 5) = .11$. From these probabilities, we can derive the estimated mean ($M = 3.48$)[6] which, in turn, allows us to calculate Cohen's d effect size by dividing the difference between two estimated means by the pooled standard deviation.

Our study had a longitudinal quasi-experimental design in which students completed questionnaires annually from 6th to 10th grade (that is, Years 8 to 12) and in which some students participated in Shared Education after 8th grade (that is, after Year 10). We used monotonic effects (Bürkner & Charpentier, 2020) to model change over time. This means that our models estimated the magnitude and direction of the average change that occurs between two time points as well as what proportion of the total change occurs between each of the four pairs of time points. This allowed us, for example, to show that intergroup contact increased more between 8th and 9th grade (Years 10 and 11) than between any of the other time points (see Figure 3A).

In all analyses, we estimated correlated varying (random) effects to account for student-level and school-level differences in initial responses and in trajectories over time. In the student-level analyses, we estimated the magnitude and direction of change over time for students who did not participate in Shared Education and, for each time point, the magnitude and direction of the difference for students who participated in Shared Education. In the school-level analyses, we estimated fixed effects that quantify differences between the three levels of school involvement in Shared Education in initial responses and in the magnitude and direction of change over time. Figure S3 in the online supplemental materials illustrates that our overall analysis strategy allowed us to accurately model students' trajectories.

We report two effect sizes derived from our statistical models. First, we report the adjusted mean difference at the end of the study between students who participated (and schools who were involved) in Shared Education and those who did not. We adjusted the estimated mean difference after students had participated in Shared Education (at 10th grade, Year 12) by subtracting the estimated mean difference before students participated in Shared Education (at 8th grade, Year 10) from it. Second, we report the adjusted mean difference after one year of Shared Education (at 9th grade, Year 11). We transform both mean differences to Cohen's d effect size by dividing the mean difference by the sample standard deviation before the intervention (at 8th grade, Year 10). We represent the uncertainty around all estimates by reporting uncertainty intervals enclosing the 95% most plausible estimates.

Our study resulted in a dataset with missing responses. For multilevel models, missing observations of the (time-variant) outcome variable are unproblematic, if they are missing at random (Schafer & Graham, 2002), and do not need to be imputed. Missing observations of a (time-invariant) predictor variable, however, are problematic because, even if they are missing at random, we need to either impute them or exclude all data from a participant for whom the (time-invariant) predictor variable is missing. As the student-level predictor was measured only in Years 11 and 12 (when some students participated in shared classes), observations of this variable were missing whenever students did not participate in one or both years. To impute missing observations, we built a multilevel logistic regression model that used other observed responses as well as school-level commonalities to predict whether students had participated in Shared Education or not. Using Bayesian inference, we incorporated the imputed probabilities, as well as the uncertainty around these imputed values, into our student-level analysis models (for details, see online supplemental materials).

Results

Student-Level Analyses

We ran eight longitudinal multilevel models, one for each outcome variable, to estimate, for each time point, the magnitude and direction of the differences between students who participated in Shared Education and students who did not. Figure 3A shows the estimated means as a function of time and student-level participation. Figure 3B shows the estimated mean differences (as Cohen's d effect size) in 8th grade (Year 10) between students who later did versus did not participate in shared classes, as well as the estimated mean differences in 9th grade (Year 11) and 10th grade (Year 12) between participating and nonparticipating students, adjusted for preexisting differences in 8th grade (Year 10).
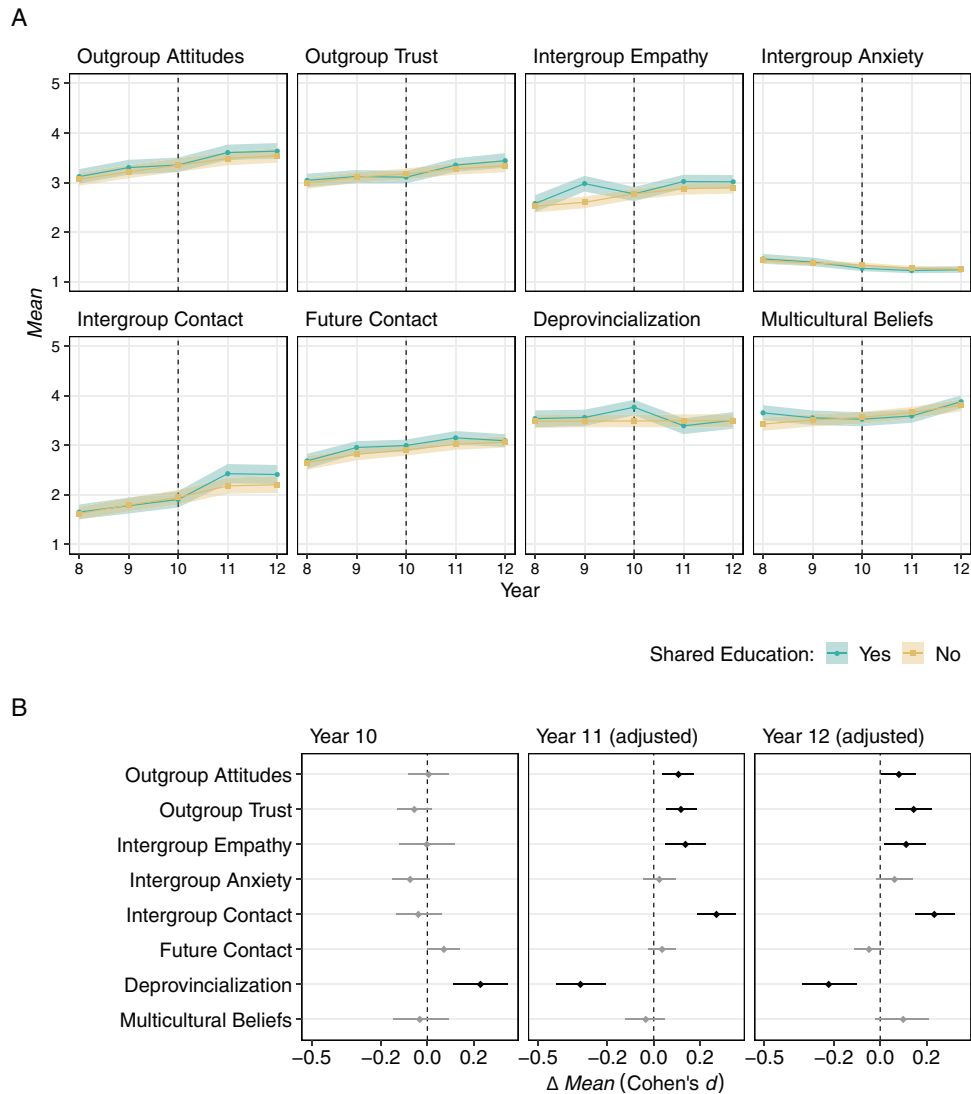
We found that, in Year 10, students who participated in Shared Education did not seem to differ much from students who did not participate. Of all outcomes, we found strong evidence only for preexisting differences in deprovincialization: participating students reported, on average, higher deprovincialization than nonparticipating students ($d_{10} = .23$, [.11, .35]). We thus found evidence for preexisting differences in only one of eight measures. Nonetheless, we adjust for preintervention differences in all our analyses, as described under Analysis Strategy.

We found evidence that, after adjusting for preintervention differences, participating students reported more intergroup contact outside of class ($d_{11} = .27$, [.19, .35]; $d_{12} = .23$, [.15, .32]), more favorable outgroup attitudes ($d_{11} = .11$, [.04, .18]; $d_{12} = .08$, [>.00, .15]), more outgroup trust ($d_{11} = .12$, [.05, .18]; $d_{12} = .14$, [.07, .22]), and more intergroup empathy ($d_{11} = .14$, [.05, .22]; $d_{12} = .11$, [.02, .20]) in 9th and 10th grades (Years 11 and 12) than nonparticipating students. Based on Funder and Ozer's (2019) recommendations for evaluating effect sizes in psychological research, the estimated effect

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[5] Our models used conservative, weakly informative prior distributions for all fixed effects, $\beta \sim \text{Student-}t(3, 0, 1)$ on the log-odds scale, and to model the proportion of the overall change that occurs between each of the five time points, $\zeta \sim \text{Dirichlet}(\{1, 1, 1, 1\})$. We used a Half-Student-t(3, 0, 3) prior for the standard deviation of the varying intercepts across students and Half-Student-t(3, 0, 1) priors for the standard deviations of all other varying effects. Our models used non-informative priors for the thresholds of the cumulative logit link function.

[6] $M = 1\Pr(y = 1) + 2\Pr(y = 2) + 3\Pr(y = 3) + 4\Pr(y = 4) + 5\Pr(y = 5) = 3.48$.

Figure 3

Results From the Student-Level Analyses for Each of the Eight Outcome Measures With Uncertainty Intervals Enclosing the 95% Most Plausible Estimates

Note. (A) Estimated means for each year and for students who did versus did not (go on to) participate in Shared Education in Year 11 and 12. Preintervention differences were estimated in Year 10 (dashed line) before some students participated in Shared Education. (B) Estimated mean differences (as Cohen's *d* effect size) in Year 10 between students who went on to participate and those who did not participate in Shared Education, as well as the estimated mean differences in Year 11 and Year 12, adjusted for preexisting mean differences in Year 10. See the online article for the color version of this figure.

on intergroup contact is of medium size, while the estimated effects on the three outcomes are small. We found little evidence that, after adjusting for preexisting differences, participating students differed from nonparticipating students in terms of willingness to engage in future contact ($d_{11} = .04$, $[-.02, .10]$; $d_{12} = -.05$, $[-.11, .02]$), intergroup anxiety ($d_{11} = .02$, $[-.05, .10]$; $d_{12} = .06$, $[-.02, .14]$), or multicultural beliefs ($d_{11} = -.03$, $[-.12, .05]$; $d_{12} = .10$, $[-.02, .21]$). We also found that, after adjusting for preexisting differences, participating students showed lower deprovincialization than nonparticipating students ($d_{11} = -.32$, $[-.42, -.21]$; $d_{12} = -.22$, $[-.34, -.10]$) after participating in Shared Education. This negative difference, however,

represents participating students returning to the level of deprovincialization they had reported before Year 10. Overall, we thus found strong evidence that participating in Shared Education had a medium, positive effect on intergroup contact ($.23 \leq$ Cohen's $d \leq .27$) and small, positive effects on three of the seven outcome variables ($.08 \leq$ Cohen's $d \leq .14$).
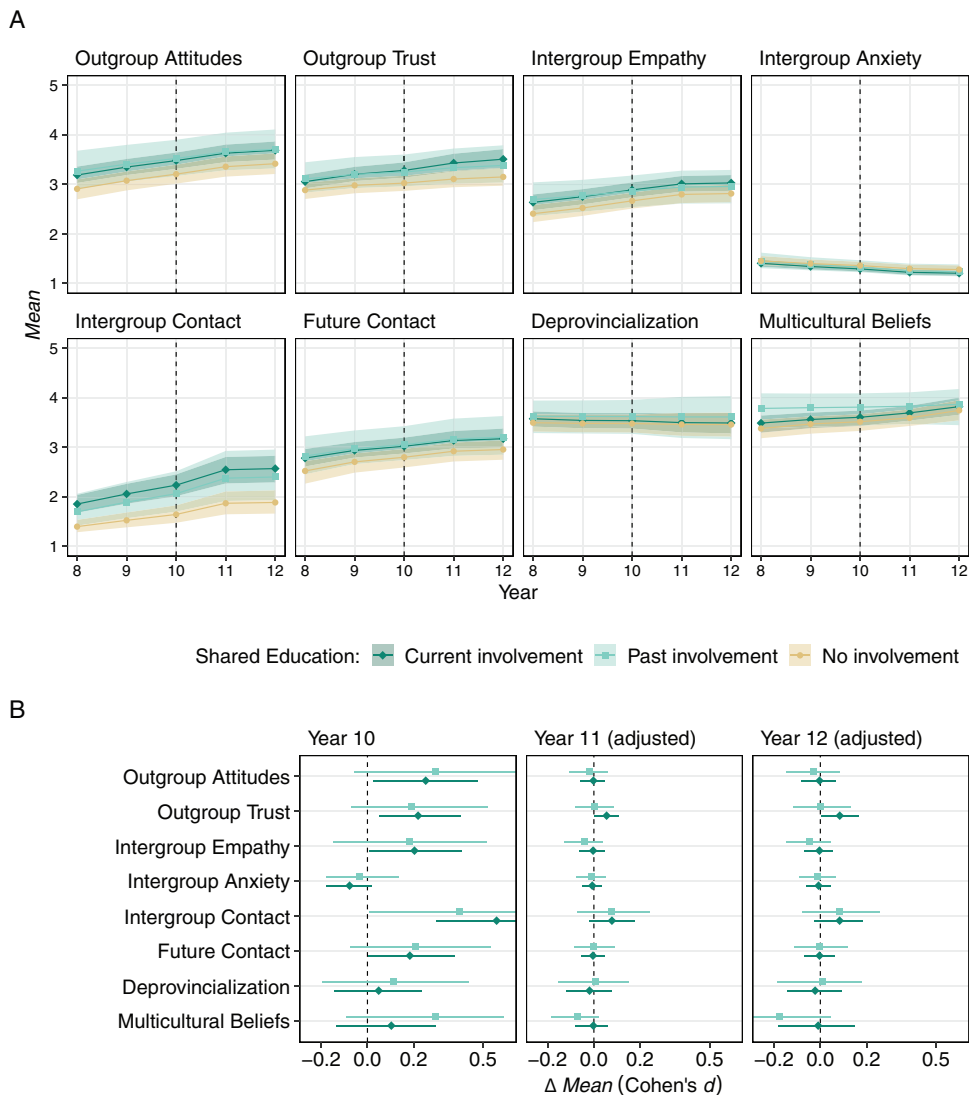
School-Level Analyses

We ran another eight longitudinal multilevel models, one for each measure, to estimate to what extent school-level involvement

in Shared Education affected how students' responses to the measures changed over time. Figure 4A shows the estimated means as a function of time and school-level involvement, showing distinct trajectories for schools that did not receive funding for Shared Education (1 = *no involvement*), had received funding for Shared Education in the past (2 = *past involvement*), or did receive funding for Shared Education during the study (3 = *current involvement*). Figure 4B shows the estimated mean differences (as Cohen's *d* effect size) between, on the one hand, students in schools that were not involved in Shared Education and, on the other hand, students in schools that were involved or had been

involved in the past at 9th and 10th grade (Years 10, 11, and 12). Below, in the text, we do not report effect sizes for schools that had been involved in Shared Education in the past because this sample was small and did not yield precise estimates of preintervention differences (see Figure 4B).

We found that in 8th grade (Year 10), before some students participated in shared classes, students in schools that were involved in Shared Education reported more intergroup contact (d_{10} = .56, [.30, .81]), greater willingness to engage in future contact (d_{10} = .18, [>.00, .38]), more favorable outgroup attitudes (d_{10} = .25, [.03, .48]), more outgroup trust (d_{10} = .22, [.05, .40]), and more

Figure 4

Results From the School-Level Analyses for Each of the Eight Outcome Measures With Uncertainty Intervals Enclosing the 95% Most Plausible Estimates

Note. (A) Estimated means for each year and for each level of school involvement in Shared Education. Preintervention differences were estimated in Year 10 (dashed line) before some students participated in Shared Education. (B) Estimated mean differences (as Cohen's *d* effect size) in Year 10 between, on the one hand, students in schools that were not involved in Shared Education and, on the other hand, students in schools that were involved or had been involved in the past, as well as the estimated mean differences in Year 11 and Year 12, adjusted for preexisting mean differences in Year 10. See the online article for the color version of this figure.

intergroup empathy (d_{10} = .20, [.01, .41]) than students in schools not involved in Shared Education. We found less evidence for pre-intervention differences in intergroup anxiety (d_{10} = −.08, [−.18, .02]), deprovincialization (d_{10} = .05, [−.14, .23]), and multicultural beliefs (d_{10} = .10, [−.13, .29]). Overall, we thus found evidence that schools involved in Shared Education differed from other schools on intergroup contact, and four of the seven outcome variables, before the intervention started. These preexisting differences should, perhaps, be expected as schools volunteered to participate in the intervention and show the necessity of adjusting for these differences. As reported earlier, we did not find much evidence that *students* who participated differed, before the intervention started, from those who did not (which included comparisons made *within* schools).

We found that, after adjusting for preintervention differences, students in schools involved in Shared Education reported somewhat more outgroup trust in Years 11 (9th grade) and 12 (10th grade) than students in schools not involved in Shared Education (d_{11} = .05, [>.00, .11]; d_{12} = .08, [>.00, .16]). We found no evidence that, after adjusting for preintervention differences, school-level involvement in Shared Education affected any of the other outcomes. Overall, we found little evidence that being a student at a school involved in Shared Education was, on its own, enough to improve the relevant outcomes. As the school-level analyses included both students who participated in the program and students who did not, our findings do not contradict the results of the student-level analyses.

Discussion

Past research has found intergroup contact to be a promising intervention to reduce prejudice and has identified adolescence as the developmental period during which contact experiences are most effective for shaping intergroup relations in later life. There is, however, a need for research on interventions that foster sustained intergroup contact among adolescents and do so at a large scale. We evaluated whether the Shared Education program, a nation-wide, school-based intervention that facilitates in-class contact between adolescents from the Catholic and Protestant communities, can foster social integration in postconflict Northern Ireland. In a five-wave longitudinal, quasi-experimental study, we compared the developmental trajectories of a large sample of students, some of whom participated in shared classes with students from the other community who attended a different school. We found that participating in shared classes had a medium, positive effect on the amount of intergroup contact students had outside of class, and small, positive effects on students' outgroup attitudes, outgroup trust, and intergroup empathy (but not on willingness for future contact, intergroup anxiety, deprovincialization, multicultural beliefs). Below, we discuss the implications of our findings for understanding how contact-based interventions in adolescence can shape the developmental trajectories relevant to intergroup relations, and acknowledge some limitations of the present research.

Implications

We found that, as hypothesized, students who shared classes with members of the other community went on to report more

contact, including friendships, with members of the other community outside of class. Forming close friendships is an important developmental task of adolescence (McCormick et al., 2011), the developmental period in which peer relationships exert a growing influence on attitudes, beliefs, and behaviors (Brechwald & Prinstein, 2011). Given how central peer relations are to adolescents' development, promoting cross-group friendships among adolescents might be an important means by which interventions can foster social integration that outlasts the duration of the intervention. It is encouraging that we found the strongest effects for a measure that assessed, albeit self-reported, concrete intergroup behaviors. Contrary to predictions, students in shared classes did not report a greater willingness to engage in future contact. One explanation for these divergent findings might be that gaining friends from the other community had already satisfied participating students' desire for more cross-group friendships. Still, our findings suggest that Shared Education can foster relational bridges between Catholic and Protestant adolescents despite pervasive residential segregation and school separation.

We found that, as hypothesized, students who participated in Shared Education went on to report more favorable outgroup attitudes and greater outgroup trust than students who did not participate. This finding shows that, in line with the 'contact hypothesis' (Allport, 1954), interventions can harness intergroup contact to improve intergroup relations. Paluck et al. (2021) conducted a systematic review and meta-analysis of experimental tests of various approaches to reducing prejudice. While they found promising results for many of these approaches, they also found that larger, more precise studies tended to find smaller, nonsignificant effects while smaller, less precise studies tended to find larger, significant effects. This pattern of results suggests that publication bias affected the available evidence. In contrast, the current study found evidence for the prejudice-reducing effects of a contact-based intervention with a very large sample size.[7] Furthermore, Paluck et al. (2021) found that most recent research on approaches to reducing prejudice relied on 'light touch' interventions lasting less than ten minutes. In contrast, the current study provided a rare test of an extended, recurring contact-based intervention and found evidence for its effectiveness in improving intergroup relations in a postconflict society.

Our findings seemingly contradicted evidence from a recent evaluation of another large-scale contact-based intervention targeting adolescents. The National Citizen Service was not found to be an effective means to improve intergroup attitudes (Reimer et al., 2021). However, while both interventions facilitated intergroup contact among adolescents—the age group thought to be most receptive to the effects of intergroup contact (Wölfer et al., 2016)—the National Citizen Service was limited to 3–4 weeks adolescents spent in a summer program. In contrast, Shared Education provided adolescents with regular opportunities for intergroup contact that were integrated into their day-to-day life. Arguably, these conditions made it more likely that participants formed cross-group friendships that outlasted their participation in the program. Our findings thus support Pettigrew's (1998) argument that contact-

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[7] For comparison, 80% of studies in Paluck et al.'s (2021) review had < 78 participants per condition.

based interventions should be structured to facilitate friendship formation.

We found that, as hypothesized, students who participated in the intervention went on to report more intergroup empathy. Empathizing with outgroup members likely helps adolescents navigate future intergroup interactions. Our findings suggest that, by fostering empathy and trust, participating in shared classes might well prepare adolescents for more positive intergroup interactions in early adulthood and beyond. This effect could be crucial to adolescents' development as it occurs during a sensitive developmental period in which social cognitions and outgroup attitudes form. Contrary to predictions based on cross-sectional and longitudinal evidence (Pettigrew & Tropp, 2008; Swart et al., 2011), students who participated in the intervention did not go on to report less intergroup anxiety than students who did not participate. That said, intergroup anxiety did not seem to be a notable obstacle to social integration in our sample as participants overwhelmingly reported little anxiety about interacting with the other community.

Also contrary to predictions, we found that students who participated in shared classes did not go on to report greater deprovincialization or endorsement of multicultural beliefs than students who did not participate. Instead, we found that students who participated in the intervention showed more deprovincialization *before* participating in the intervention—but also that their levels of deprovincialization returned to baseline *after* participating in the intervention. In any case, our findings suggest that a school-based intervention to facilitate intergroup contact might not improve broader attachments to group values or multicultural orientations. As noted in the introduction, these outcomes are not tied to specific outgroups, are less closely predicted by intergroup contact theory than the other outcomes, and can thus be considered more distal outcomes.

In summary, we found strong evidence that participating in Shared Education had a medium-size, positive effect on intergroup contact ($.23 \leq$ Cohen's $d \leq .27$) and small, positive effects on outgroup attitudes, outgroup trust, and intergroup empathy ($.08 \leq$ Cohen's $d \leq .14$). A comparative interpretation of these effect sizes shows that the estimated effects are smaller than the effects estimated in two meta-analyses (Lemmer & Wagner, 2015; Paluck et al., 2021) both of which found contact-based interventions to reduce prejudice by Cohen's $d = .28$ standard deviations. This comparison is, however, flawed as meta-analytic effect sizes are inflated by publication bias and questionable research practices in the underlying literature (Carter et al., 2019). Simulation is an intuitive approach to interpreting the estimated effect sizes. We simulated posterior predictions for 1,000 students from 10 schools based on estimates from the student-level models. This allowed us to make counterfactual predictions—that is, to compare what would have happened to the same students if they had or had not participated in Shared Education. Based on these simulations, we predict that 82 [39, 125] students would improve in outgroup trust for every 1,000 students participating in Shared Education. This difference is 70 [44, 97] for intergroup contact, 32 [0, 62] for outgroup attitudes, and 35 [5, 64] for intergroup empathy. To put the impact of the intervention into perspective, we predict that almost 10% of students would improve in terms of their outgroup trust—which means that thousands of students across Northern Ireland would have more intercommunal contact and trust if Shared Education were to be rolled out to all separate schools.

Whether a small effect is of practical importance depends, in part, on whether it cumulates (Funder & Ozer, 2019). One way for a small effect to cumulate is across individuals. Even if the effect of Shared Education is small for most participating students, its aggregate effect will still be important for improving intergroup relations in Northern Ireland as long as it reaches enough students. For example, past research has found outgroup trust to be associated with more positive behavioral tendencies, and (negative) outgroup attitudes to be associated with more aggressive and avoidant behavioral tendencies in Northern Ireland (Tam et al., 2009). Even if Shared Education has only a small effect on each student's outgroup trust and attitudes, and even if these outcomes influence each student's behavior on only a few occasions, this might add up to an important cumulative effect across many students and occasions. Another way for a small effect to cumulate is across time. As the intervention occurs during adolescence, the developmental period in which attitudes develop before they stabilize in adulthood (Krosnick & Alwin, 1989), its effects might accumulate across the lifetime of participating adolescents. If, as discussed, Shared Education prepares adolescents for more positive intergroup interactions in early adulthood and beyond, Shared Education might initiate a virtuous cycle by leading to more positive contact experiences in the future. In this way, Shared Education might have a compounding effect on participants' intergroup attitudes and experiences.

Alternatively, a small *average* effect might mean that the effect is not small and uniform, as assumed above, but that it is large and variable. That is, participating in Shared Education might have a large effect on some students but no effect on many others. In this study, we had little control over, or insight into, how Shared Education was implemented across settings. As such, our study speaks to the intervention's effectiveness (how well it works in a real-world setting) rather than its efficacy (how well it works in an ideal setting). It is possible that we would have observed a large effect of Shared Education if its implementation had been more consistent across settings. Increasing implementation quality and fidelity (Carroll et al., 2007) should thus be an important goal for future applications.

In addition to student-level analyses, we ran school-level analyses. First, we found that, before the onset of the intervention, students in schools involved in Shared Education already reported more favorable outcomes than students in schools that were not involved in the program. Such differences are not surprising since schools volunteered to participate in the program. Still, this finding shows the importance of controlling for differences between individuals' and schools' starting points and trajectories, as we did in the present research. Second, we found little evidence that students in schools involved in Shared Education went on to report more favorable outcomes after the onset of the intervention. This suggests that the effects of the intervention were limited to students who themselves participated in shared classes and did not extend to students who merely attended schools with shared classes but who did not themselves participate in them.

Limitations

We conducted a rigorous evaluation of a school-based intervention to promote intergroup contact. We used a five-wave longitudinal, quasi-experimental design to compare the developmental

trajectories of adolescents who, in the fourth and fifth year of the study, participated in shared classes to adolescents who did not. This design allowed us to control for preintervention differences as well as for student- and school-level differences and trajectories. In addition, we recruited a large sample of students and schools which allowed us to estimate the effects of the intervention with great precision. Still, in lieu of random assignment, we need to make several assumptions to draw causal inferences about the effectiveness of the intervention from the estimated effects.

First, we need to assume that, after controlling for preintervention differences, students who participated in shared classes did not differ, from students who did not participate in shared classes, in terms of any relevant confounding variable. In principle, we could have controlled for any unobserved confounders by randomly assigning students to participate in the intervention. In practice, we had no influence over which schools received funding for Shared Education or which classes within a school were selected to be shared with students from the other community. That said, students had little control over whether they would participate in shared classes and, in some schools, had incentives to participate that were independent of intergroup contact (e.g., being able to attend a class not offered in one's school). We also did not find evidence for preintervention differences between participating and nonparticipating students (see Figure 3B). As self-selection bias was thus an unlikely confounder and as we controlled for preintervention differences in outcomes, we can be confident that the estimated differences between participating and nonparticipating students resulted from the intervention.

Second, we need to assume that students who participated in shared classes would, if they had not participated, have followed the same developmental trajectory as students who did not participate in shared classes. Without random assignment, we could not know: (1) whether students who participated differed from students who did not participate in terms of unobserved confounding variables, and (2) whether differences in these variables would have caused students who participated in shared classes to improve more over time even if they had not participated. That said, we found few differences in students' trajectories in the three years (8–10) preceding the two-year period (11–12) in which some of them participated in shared classes (see Figure 3A).

Third, as we did not observe the student-level treatment variable for all participants, we need to assume that the model we used to impute the missing observations was accurate. On the one hand, we used cross-validation to assess the out-of-sample prediction accuracy of the imputation model and found it to make reasonably accurate predictions (72%, $F_1 = .71$). On the other hand, we found that the imputation model made more accurate predictions for students who had not, in fact, participated (83%) than for students who had participated (63%) in the intervention. As such, the imputation model—though very accurate overall—likely underestimated participation relative to nonparticipation. That said, underestimating student-level participation would attenuate effect sizes in the student-level analyses so that, if anything, our study underestimated the effects of participating in Shared Education. In addition, we accounted for the uncertainty around imputed values in the student-level analyses, limiting their influence on our estimates (see online supplemental materials).

Despite its strengths, our study design could not rule out that the estimated effect sizes were, to some extent, influenced by unobserved confounders and by missing observations. Still, we argue that the assumptions we need to make to interpret the estimated effects as causal effects are plausible. In addition, we note that, in contrast to other evaluations (e.g., Mousa, 2020), we did not assess concrete behavior but instead relied on self-report measures. We encourage researchers to replicate our findings in future studies that use random assignment, behavioral outcomes, and consider the quality and fidelity of intervention implementation.

Conclusion

In a large, longitudinal, quasi-experimental study we compared the developmental trajectories of secondary-school students in postconflict Northern Ireland who, as part of the Shared Education program, shared some classes with students from the other ethno-religious community to students who did not. We estimated that participating in shared classes had a medium, positive effect on the amount of intergroup contact students had outside of class, and small, positive effects on students' outgroup attitudes, outgroup trust, and intergroup empathy (but not on four other outcomes). Our findings show that a school-based program of shared education can provide a viable and effective intervention to promote intergroup contact, improve intergroup relations, and foster social integration among adolescents—and that it can do so at a large scale in a postconflict society marked by residential segregation and separate schooling.

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