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Does Intergroup Contact Foster Solidarity With the Disadvantaged? A Longitudinal Analysis Across 7 Years

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Contact theory is a well-established paradigm for improving intergroup relations—positive contact between groups promotes *social harmony* by increasing intergroup warmth. A long-standing critique of this paradigm is that contact does not necessarily promote *social equality*. Recent research has blunted this critique by showing that contact correlates positively with political solidarity expressed by dominant groups toward subordinate groups, thus furthering the goal of equality. However, this research precludes causal inferences because it conflates within-person change (people with higher contact *subsequently* expressing higher solidarity) and between-person stability (people with chronically high contact *simultaneously* expressing chronically high solidarity, and vice versa). We addressed this problem in a highly powered, seven-wave study using two different measures of contact and three different measures of political solidarity ($N = 22{,}646$). Results showed no within-person change over a 1-year period (inconsistent with a causal effect), but significant between-person stability (consistent with third-variable explanations). This reinforces doubts about contact as a strategy for promoting equality.

Public Significance Statement
The 2020 Black Lives Matter protests, the largest protest movement in U.S. history, have drawn attention to persistent social injustices and shown the power of political mobilization to challenge these injustices. While led by Black people, these protests saw the participation of, and solidarity from, many White people. Understanding what motivates the advantaged, who do not stand to directly benefit from these movements, is not only an important question for social scientists but is also practically important for understanding coalition building in diverse societies. By critically evaluating one of the most prominent explanations for political solidarity in psychological science, our research makes an important contribution to this endeavor.

Keywords: intergroup contact, political solidarity, political attitudes, longitudinal analysis

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As societies become more diverse and the potential for intergroup disharmony rises (Putnam, 2007), contact theory offers a powerful paradigm for promoting harmony: Greater frequency and intimacy of contact between groups fosters intergroup warmth (Pettigrew & Tropp, 2006). However, disharmony is not the only problem facing diverse societies. There is also *inequality* in the distribution of resources between groups (Sidanius & Pratto, 1999). Theorists have long questioned the usefulness of the contact paradigm for addressing the problem of inequality (Dixon et al., 2005; Reicher, 1986; Saguy et al., 2008; see also Reimer & Sengupta, 2022). Indeed, early research suggested that despite fostering greater warmth toward disadvantaged groups, contact may *not* make advantaged groups any more supportive of policies aimed at reducing intergroup inequality (Jackman & Crane, 1986).

But over the past decade, a different picture has begun to emerge. Recent research has shown that the more contact advantaged groups have with disadvantaged outgroups, the more they support policy proposals and protest movements favoring those outgroups (Dixon et al., 2007; Kamberi et al., 2017; Selvanathan et al., 2018). In the most comprehensive analysis to date, Hässler et al. (2020) sampled 8,114 individuals from advantaged groups across 69 countries and found that contact was associated with higher support for social change favoring the disadvantaged. This kind of *political solidarity* shown by advantaged groups—recently visible in the Black Lives Matter protests of 2020—is crucial for the disadvantaged to achieve greater equality (Subašic et al., 2008). Thus, a growing body of evidence now suggests that by fostering not just outgroup warmth but also solidarity, contact facilitates both harmony *and* equality (Reimer et al., 2017; Tropp & Barlow, 2018).

However, compared to research on outgroup warmth, research on outgroup political solidarity is still in its infancy. Hässler et al.'s (2020) recent analysis represented a big step forward because it addressed the problems of analytical flexibility and contextual variability in contact research by showing that solidarity effects are robust to variations in measurement and intergroup context. Nonetheless, the study shares a key limitation with much of the literature on solidarity effects: It is correlational and precludes causal inferences. More specifically, it cannot establish whether changes in contact *precede* changes in solidarity.

In the present article, we explain how the challenge of testing causal effects in contact research can be addressed by applying the latest developments in longitudinal modeling to large-scale survey data. We then apply the suggested models (Hamaker et al., 2015) to data from a seven-wave national probability sample in New Zealand, using two different indicators of contact and three different indicators of solidarity ($N = 22{,}646$). Thus, we present the strongest test, to date, of whether contact precedes political solidarity. This is a necessary condition for establishing whether contact fosters intergroup equality by increasing support for protests or policies favoring the disadvantaged.

Causal Inference in Contact Research

Causal inference is a difficult epistemological problem. Randomized-controlled trials are the best solution to this problem, but they are not a panacea (Cartwright & Hardie, 2012). The problem is exacerbated when a hypothetical causal variable is not easily amenable to random assignment into conditions, as is the case for intergroup contact. It is extremely difficult for researchers to manipulate the quantity and quality of people's outgroup interactions. To the limited extent that these interactions can be experimentally induced, it is unclear how well they capture the phenomenon of intergroup contact as conceived in contact theory—that is, a process of increasing intimacy and eventual friendship with outgroup members over time (MacInnis & Page-Gould, 2015; see also Yarkoni, 2022).

As a result, the overwhelming majority of contact research has used survey data—specifically, self-reported levels of contact and intergroup attitudes (Christ & Wagner, 2013). One way of making somewhat stronger causal inferences from these data is to assess contact and intergroup attitudes over time. Longitudinal analyses, such as the cross-lagged panel model (see Figure 1), can then be used to test whether higher levels of outgroup contact at a given time point predict improved attitudes at a subsequent time point. This helps establish a necessary condition for causal interference, *temporal precedence*; a cause must precede its hypothesized effect.

Causal Inference Using Longitudinal Models

Cross-lagged panel modeling (CLPM), the most commonly used method for longitudinal analysis, accounts for several

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confounds that cannot be adjusted for in cross-sectional regression models, yielding stronger tests of temporal precedence (Zyphur et al., 2019). First, these models adjust for shared experiences that affect everyone in the population at a given time point (by modeling the time-specific intercepts for each variable). Second, they adjust for the fact that the level of a variable at a given time point is dependent on its value at a preceding time point (by modeling autoregressive effects; parameters $\beta_{x1}^{(x)}$ and $\beta_{y1}^{(y)}$ in Figure 1). Third, they adjust for unobserved variables that might simultaneously cause both predictor and outcome at a given time point (by modeling the time-specific residual covariances between the predictor and outcome; parameters $u_{t-n}^{(y)}$ in Figure 1). Given these advantages, and in the absence of generalizable experimental paradigms, researchers have increasingly employed CLPM to test the effects of intergroup contact (Dhont et al., 2012; Meleady et al., 2021; Swart et al., 2011).

However, only one longitudinal study to date has examined whether contact with the disadvantaged predicts higher *political solidarity* among advantaged groups over time. Using CLPM, Reimer et al. (2017) showed that the more contact heterosexual participants had with nonheterosexuals, the more willing they were to engage in collective action on behalf of the lesbian, gay, bisexual and transgender (LGBT) community 3 months later. Although this finding aligns with the correlational research on solidarity effects, it provides limited incremental evidence for a causal effect of contact. This is because CLPM does not account for at least one other confound that can produce spurious effects in longitudinal designs: *between-person stability* (Hamaker et al., 2015; Rogosa, 1980).

Between-person stability refers to the degree to which time-invariant levels of one variable correlate with time-invariant levels of another—for example, people with higher levels of contact across all time points also show higher levels of outgroup political solidarity across all time points and vice versa (parameter $\psi_{\eta}^{(xy)}$ in Figure 1b). This between-person correlation can be considered a confound when estimating causal effects because factors modeled as stable over time cannot be part of a temporal sequence in which one factor causes the other (Granger, 1980). When between-person stability is accounted for, the lagged effect of the predictor on the outcome represents *within-person change* (parameter $\beta_{x1}^{(y)}$ in Figure 1b). This is the degree to which someone who had higher than expected levels of the predictor at one time point, relative to their average levels, shows higher than expected levels of the outcome at a subsequent time point (Hamaker et al., 2015). Thus, modeling within-person change allows for more accurate estimates of hypothesized causal relationships than extant correlational or cross-lagged effects.

The Random-Intercept Cross-Lagged Panel Model

Hamaker et al. (2015) demonstrated this improved accuracy empirically by developing a random-intercept cross-lagged panel model (RI-CLPM) that could estimate within-person change while adjusting for between-person stability (see also Berry & Willoughby, 2017; Zyphur et al., 2019). They showed that there are many ways in which confounding between-person stability and within-person change, as in the traditional CLPM approach, could lead to incorrect conclusions about cross-lagged effects. These include (a) indicating a negative relationship between variables when the true relationship is positive, (b) identifying the wrong variable as causally dominant, and (c) indicating reciprocal effects when they do not exist.

In more recent simulations, Lucas (2022) found that CLPM often produces spurious cross-lagged effects and that “the likelihood of finding such spurious effects can reach 100% in many realistic scenarios” (p. 4). Moreover, CLPMs are more likely to underestimate within-person effects when they actually exist (Lucas, 2022). Although there are a few alternative methods for separating between-person stability and within-person change, RI-CLPMs are the most likely to converge (Orth et al., 2021) and do not require intensive longitudinal data (e.g., 30+ assessments). Thus, RI-CLPMs are quickly becoming the method of choice amongst longitudinal researchers interested in within-person change (see Lucas, 2022; Osborne & Little, in press; Osborne & Sibley, 2020).

The Present Study

Here, we use RI-CLPM to examine whether the association between contact and outgroup political solidarity observed in recent research arises from (a) stable individual differences in contact correlating with stable individual differences in solidarity in the population (*between-person stability)*, (b) people

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with higher-than-expected contact at one time point expressing higher-than-expected solidarity at a later time point (*within-person change*), or (c) both. Based on the correlational evidence, we expect to find a positive association between contact and solidarity. However, we leave the question open as to how much of this covariation reflects between-person stability versus within-person change. Between-person stability would indicate that unobserved variables cause individual differences in contact and solidarity to correlate in the population. Within-person change would indicate the temporal precedence of contact relative to political solidarity, consistent with a causal effect. This represents a crucial next step in determining whether recent correlational findings—which contradict prior theorizing that contact is ineffective for promoting solidarity (Jackman & Crane, 1986)—reflect a real phenomenon.

Method

Participants and Sampling Procedure

Data for the current research were drawn from an annual, national, longitudinal study in New Zealand: The New Zealand Attitudes and Values Study (NZAVS). The NZAVS began in 2009 (i.e., Time 1) and is based on a random sample of adults from the electoral roll. Invitations to participate in Time 1 of the NZAVS were sent to 40,500 people randomly selected from the electoral roll, 6,518 (16.6%) of whom returned completed surveys. In late 2010, Time 2 surveys were sent to participants who responded to Time 1. This survey was completed by 4,423 (67.9%) participants from the initial Time 1 sample.

From Time 3 to Time 5 (2011–2013), and again at Time 8 (2016), in addition to sending out surveys to those existing participants, booster sampling was conducted to increase the overall sample size (see Sibley, 2021, for full details on the procedure). Thus, the Time 3 NZAVS (2011) contained responses from 6,844 participants, the Time 4 (2012) NZAVS had 12,182 participants, the Time 5 (2013) NZAVS had 18,264 participants, the Time 6 (2014) NZAVS had 15,822 participants, the Time 7 (2015) NZAVS had 13,944 participants, the Time 8 (2016) NZAVS had 21,936 participants, and the Time 9 (2017) NZAVS had 17,072 participants. The NZAVS received ethics approval from the University of Auckland Human Participants Ethics Committee (Reference Number: UAHPEC22576).

Intergroup Context

Our hypothesis relates to the contact dominant groups have with subordinate groups, and the most politically relevant dimension of intergroup differentiation in New Zealand is ethnicity. Thus, our analyses focus on outgroup contact among the ethnic majority group in New Zealand—New Zealanders of European descent (hereafter, Europeans). The focal outgroup with whom they can feel varying degrees of political solidarity is Māori. Māori are the indigenous peoples of New Zealand, the second-largest ethnic group, and are disadvantaged relative to Europeans on a range of socioeconomic indicators (Ministry of Social Development, 2016). We focus on these groups to the exclusion of other ethnic groups in New Zealand because measures of political solidarity included in the NZAVS (see below) are specific to relations between these two groups.

Questionnaire Measures

Intergroup Contact

The NZAVS includes two indices of positive intergroup contact. One index asks, "Roughly how many hours (if any) have you spent with friends from each of the following groups in the last week?" and lists the four most populous ethnic groups in New Zealand: "NZ Europeans," "Māori," "Pacific Islanders," and "Asians." We used the response that corresponded to hours spent with Māori friends. A second index measures relatively more casual contact and asks: "How frequently do you have positive/good interactions with Māori" (1 = *not frequently at all* to 7 = *very frequently*; from Barlow et al., 2012).

Political Solidarity

The NZAVS includes three measures that, among European participants, correspond to an expression of political solidarity with Māori.

Collective Action

One measure taps collective action intentions and asks participants to rate the degree to which they support "protest

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marches and public demonstrations supporting the rights of Māori” (1 = *strongly oppose* to 7 = *strongly support*; Osborne & Sibley, 2013).

Cultural Policies

A second measure includes four items (α = .77) assessing support for cultural policies benefitting Māori (1 = *strongly oppose* to 7 = *strongly support*; Liu & Sibley, 2006): “Performance of the Haka at international sports events.”; “Waitangi Day as a national celebration of biculturalism”; “Teaching Māori language in New Zealand primary schools”; “Singing the national anthem in Māori and English.”

Reparative Policies

The third measure includes four items (α = .83) assessing support for reparative policies benefitting Māori (1 = *strongly oppose* to 7 = *strongly support*; Liu & Sibley, 2006): “Māori ownership of the seabed and foreshore”; “Reserving places for Māori students to study medicine”; “Rates exemptions on Māori land”; and “Crown (government) ownership of the seabed and foreshore” (reverse-scored).

The key distinction between the two policy groupings is that they are designed to address symbolic and material inequality, respectively. Cultural policies are aimed at greater inclusion of Māori culture in New Zealand’s national identity, whereas reparative policies aim to redress the imbalance in societal resources between Māori and Europeans (partly to compensate for colonial exploitation; Sengupta et al., 2015).

Neither of the two analyses reported in the present study were preregistered.

Results

All analyses were conducted in MPlus Version 8.2 (Muthén & Muthén, 2015; see Supplemental Material Appendix for full syntax).

Model 1

To allow the highest possible power to detect longitudinal effects, our first analysis utilized the contact measure which had been included in the most time points of the NZAVS—that is, hours spent with outgroup friends. Friendship represents a particularly impactful form of contact and therefore has greater potential to shift people’s attitudes than more casual contact (Pettigrew & Tropp, 2006). The friendship measure was included in all waves of the NZAVS from Time 1 (2009) to Time 9 (2017). However, the political solidarity measures were only included from Time 3 (2011) onwards. Therefore, Model 1 used data from the seven consecutive waves of the NZAVS that contained all the items required for our model—Time 3 (2011) to Time 9 (2017). This yielded a total sample size of 22,693 New Zealand European participants (M_{age} = 51.36, *SD* = 13.74; age range: 18–102 years; 63.2% women), who provided partial or complete responses to the variables of interest and who responded to at least two of the seven surveys (see Supplemetal Material Appendix, for more details on the treatment of missing data). No a priori power analysis was conducted. However, due to our extremely large sample size and long study duration (seven waves), we had adequate statistical power to detect even very small longitudinal effects.

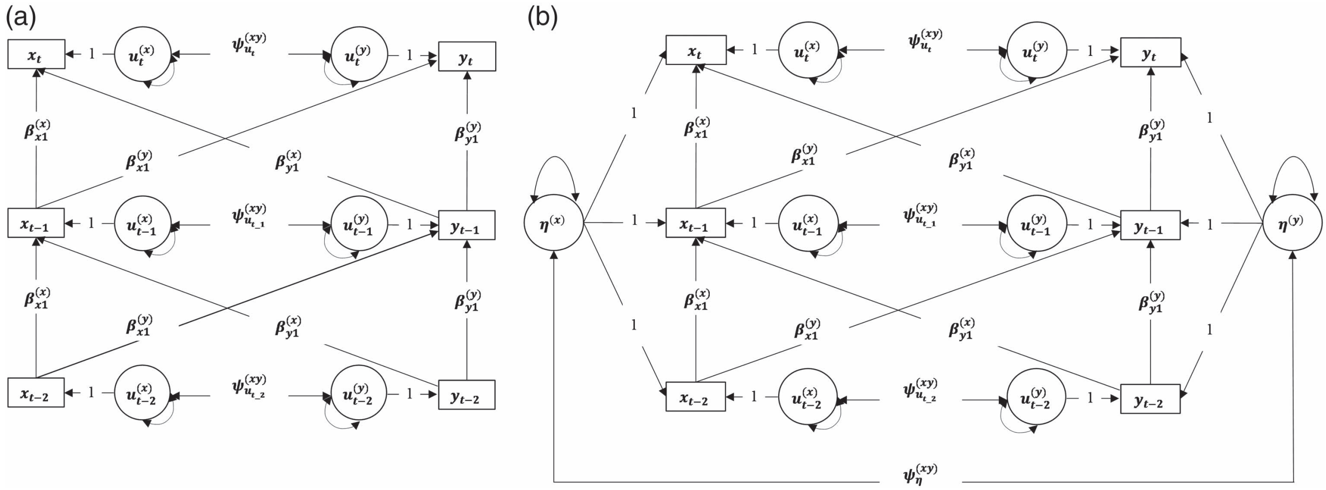
Modeling Strategy

Following the RI-CLPM procedure laid out by Hamaker et al. (2015), each observed variable was modeled as a function of two latent variables. One latent variable was the time-invariant random intercept, which was modeled as loading equally on each congeneric indicator across time (i.e., the random intercept of contact loads equally on contact at Time 3, contact at Time 4, contact at Time 5, etc., and similarly for the political solidarity intercept). The second latent variable loaded only on its corresponding indicator, thus representing its time-specific departure from the average response over time. The error variances of the observed variables were constrained to zero meaning that all variation in the observed scores was explained by the within-person and between-person factor structure.

The random intercepts of contact and solidarity were then correlated with each other to estimate between-person effects. This correlation indicates the degree to which individual differences in contact across all waves are associated with individual differences in solidarity across all waves (i.e., trait-like stability). Having thus accounted for between-person effects, the time-specific latent variables for contact and solidarity were regressed on each other in the same manner as a traditional cross-lagged panel model, yielding autoregressive

Figure 1

Path Diagram Showing Parameters Estimated in (a) a Cross-Lagged Panel Model (CLPM) and (b) a Random-Intercept Cross-Lagged Panel Model (RI-CLPM; Zyphur et al., 2019)



Note. x_{t-n} is the predictor at time $t-n$, y_{t-n} is the outcome at time $t-n$, $\beta^{(x)}_{x1}$ is the autoregressive effect of the predictor on itself, $\beta^{(y)}_{y1}$ is the autoregressive effect of the outcome on itself, $u^{(x)}_{t-n}$ is the residual variance of the predictor at time $t-n$, $u^{(y)}_{t-n}$ is the residual variance of the outcome at time $t-n$, $\psi^{(xy)}_{t-n}$ is the residual covariance of the predictor and the outcome at time $t-n$, $\beta^{(y)}_{x1}$ is the cross-lagged effect of the predictor on the outcome, and $\beta^{(x)}_{y1}$ is the cross-lagged effect of the outcome on the predictor. For the additional parameters in Panel B, $\eta^{(x)}$ is the between-person stability of the predictor, $\eta^{(y)}$ is the between-person stability of the outcome, $\psi^{(xy)}_{\eta}$ is the time-invariant association between the predictor and outcome (i.e., not a causal effect). In this model, the cross-lagged effect of the predictor on the outcome, $\beta^{(y)}_{x1}$ represents within-person change (i.e., a possible causal effect).

and cross-lagged coefficients (i.e., contact at Time 2 regressed on contact and all three indices of political solidarity at Time 1; all three indices of political solidarity at Time 2 regressed, in turn, on contact and the three political solidarity indices at Time 1, etc.). These coefficients represent within-person change over time—the degree to which an individual's deviation from their expected score (i.e., their own mean across waves) at Time 1 on the predictor variable explains their deviation from their expected score at Time 2 on the outcome variable. Finally, we constrained the lagged effects of Time 1 variables on Time 2 variables to be equal to the corresponding lagged effects of Time 2 variables on Time 3 variables, to be equal to the corresponding lagged effects from Time 3 variables to Time 4 variables, and so on. This approach maximizes power and represents an assumption of a stationary process (i.e., a continuous process with an unknown starting point; see McArdle, 2009). This means that the model is agnostic about which particular time point advantaged-group members experienced deviations from their mean levels of contact; what matters is the 1-year-lagged effect of those deviations on solidarity. The full statistical syntax used to specify this model is available in the Supplemental Material Appendix.

Model Fit

Because a traditional CLPM reflects a constrained version of a RI-CLPM (i.e., the CLPM is a model in which the variances and covariances of the random intercepts are constrained to zero), it is possible to empirically evaluate whether the RI-CLPM provides a better fit to these data than a traditional CLPM. Comparing the fit of our model with a traditional CLPM showed that the RI-CLPM fit the data well, $\chi^2_{(320)} = 3677.514$; comparative fit index (CFI) = .983; root-mean-square error of approximation (RMSEA) = .022, 95% CI [.021, .022]; standardized root-mean-square residual (sRMR) = .026, and significantly better than the CLPM, $\chi^2_{(330)} = 22{,}146.331$; CFI = .889; RMSEA = .054, 95% CI [.053, .055]; sRMR = .091; $\Delta\chi^2_{(10)} = 18{,}465.817$; $p < .001$; ΔCFI = .094; ΔRMSEA = .032. This establishes that modeling the between- and within-person levels separately is indeed necessary.

Parameter Estimates

All parameters were estimated using maximum likelihood with robust estimation of standard errors.

Between-Person Correlations. We found that the random intercept for contact was positively correlated with the random intercepts of support for collective action ($r = .21$, $SE = .01$, $p < .001$; 99% CI [.20, .24]), cultural policies ($r = .17$, $SE = .01$, $p < .001$; 99% CI [.16, .19]), and reparative policies ($r = .30$, $SE = .01$, $p < .001$; 99% CI [.27, .32]). This indicates that those who were higher on contact across the seven-wave assessment period were also higher on all three indices of political solidarity across the same period. The random intercept for collective action was also positively correlated with the random intercepts for cultural policies ($r = .60$, $SE = .01$, $p < .001$; 99% CI [.57, .61]) and reparative policies ($r = .86$, $SE \leq .01$, $p < .001$; 99% CI [.43, .47]). Finally, the random intercept for cultural policies was positively correlated with the random intercept for reparative policies ($r = .71$, $SE \leq .01$, $p < .001$; 99% CI [.59, .62]).

Within-Person Coefficients. Crucially, the within-person effects of contact on support for collective action, cultural policies, and reparative policies were nonsignificant (see Table 1, for all within-person effects in the model). This indicated that an individual's deviation from their expected score of contact (i.e., from their own mean of contact across all seven waves) was not associated with their deviation from their expected score on political solidarity 1 year later. This finding is inconsistent with a causal effect of contact on solidarity because it does not show that changes in contact precede changes in solidarity.

Disturbances. Our RI-CLPM also estimated associations between the time-specific latent variables within each time point, called "disturbances." Disturbances represent the degree to which a person's deviation from their expected score on one variable in a given year is correlated with their deviation from their expected score on another variable *in that same year*. For example, a positive contact-solidarity disturbance at Time 5 in our model would mean that people who had higher than expected contact in 2013 also showed higher political solidarity in 2013. This would suggest that there might be a *short-term* within-person effect of contact on solidarity, even in the absence of longer term effects. However, the fact that disturbances are cross-sectional associations raises the same problems with inferring causality that are inherent in any cross-sectional estimate. Thus, significant disturbances would, at most, indicate that short-term causal effects cannot be entirely ruled out. In Model 1, however, we found no significant contact-solidarity disturbances (see online Supplemental Materials, Tables S1–S7). This is consistent with our longitudinal findings, in that no evidence emerged for a causal effect of contact on solidarity.

Model 2

The contact measure used in Model 1 (i.e., hours spent with outgroup friends) is atypical in the literature and has been criticized for, among other things, not accounting for people who have no outgroup friends or no contact with those friends in the week preceding their questionnaire responses (Schmid et al., 2017). Therefore, we also included a different contact measure, which has been widely used in research across many contexts (Schmid et al., 2017)—that is, the subjective frequency of positive interactions with the outgroup. However, this measure was only included from Time 3 (2011) to Time 7 (2015). Therefore, Model 2 used data from the five consecutive waves of the NZAVS that contained all the items required for our model—Time 3 to Time 7. This yielded a total sample size of 17,084 New Zealand European participants ($M_{age} = 51.71$, $SD = 14.46$; age range: 19–100 years; 62.2% women), who provided partial or complete responses to our variables of interest and who responded to at least two of the five annual surveys.

Modeling Strategy

We used the same modeling strategy as in Model 1—that is, an RI-CLPM (Hamaker et al., 2015).

Model Fit

Again, we compared the fit of our model with a traditional CLPM and found that modeling the between- and within-

Table 1
Parameter Estimates From Model 1 for the Within-Person Effects of Every Variable at Time t − 1 With Each Variable at Time t

Time *t*	Time *t* − 1	*b*	*SE*	*z*	*p*	99% CI Low	99% CI High
Cultural policy	**Intergroup contact**	**<.001**	**<.001**	**1.743**	**.081**	**<.001**	**.001**
	Cultural policy	.166*	.008	20.393	<.001	.145	.187
	Redistributive policy	.024*	.006	4.177	<.001	.009	.039
	Collective action	−.022*	.003	−6.368	<.001	−.031	−.013
Reparative policy	**Intergroup contact**	**<.001**	**<.001**	**1.099**	**.272**	**<.001**	**.001**
	Cultural policy	.039*	.006	6.230	<.001	.023	.054
	Reparative policy	.196*	.009	22.011	<.001	.173	.219
	Collective action	−.024*	.004	−6.402	<.001	−.033	−.014
Collective action	**Intergroup contact**	**.001**	**<.001**	**2.322**	**.020**	**<.001**	**.002**
	Cultural policy	−.008	.009	−.865	.387	−.031	.016
	Reparative policy	−.020	.009	−2.245	.025	−.044	.003
	Collective action	.094*	.007	12.715	<.001	.075	.113
Intergroup contact	Intergroup contact	.095*	.024	3.941	<.001	.033	.157
	Cultural policy	−.073	.154	−.472	.637	−.471	.325
	Reparative policy	−.214	.165	−1.295	.195	−.639	.211
	Collective action	.173	.098	1.762	.078	−.080	.426

Note. Focal relationships are shown in bold. *SE* = standard error; CI = confidence interval.
* $p < .01$.

person levels separately was indeed necessary. The RI-CLPM of contact and the three indices of solidarity, $\chi^2_{(140)}$ = 2,171.908; CFI = .987; RMSEA = .029, 95% CI [.028, .030]; sRMR = .028, fit the data well, and significantly better than the CLPM, $\chi^2_{(150)}$ = 14,888.614; CFI = .904; RMSEA = .076, 95% CI [.075, .077]; sRMR = .065; $\Delta\chi^2_{(10)}$ = 12,716.706; $p < .001$; ΔCFI = .083; ΔRMSEA = .047.

Parameter Estimates

Between-Person Correlations. The random intercept for contact was positively correlated with the random intercepts of support for collective action ($r = .37$, $SE = .01$, $p < .001$; 99% CI [.34, .39]), cultural policies ($r = .47$, $SE = .01$, $p < .001$; 99% CI [.45, .49]), and reparative policies ($r = .31$, $SE = .01$, $p < .001$; 99% CI [.29, .33]). The random intercept for collective action was also positively correlated with the random intercepts for cultural policies ($r = .71$, $SE = .01$, $p < .001$; 99% CI [.69, .72]) and reparative policies ($r = .86$, $SE < .01$, $p < .001$; 99% CI [.85, .87]). Finally, the random intercept for cultural policies was positively correlated with the random intercept for reparative policies ($r = .59$, $SE = .01$, $p < .001$; 99% CI [.58, .61]).

Within-Person Coefficients. Once again, the within-person effects of contact on support for collective action, cultural policies, and reparative policies were nonsignificant (see Table 2, for all within-person effects in the model).

Disturbances. In contrast to Model 1, Model 2 did produce some significant disturbances (see online Supplemental Materials, Tables S8–S12). However, roughly the same number of disturbances were nonsignificant (seven) as were significant (eight), making it difficult to draw any substantive conclusions about causal evidence for the contact–solidarity relationship). Moreover, the most consistent disturbances were found for the association between contact and cultural policy. Cultural policies reflect the “softest” form of support for equality, because they do not require difficult structural changes to the distribution of resources between groups (see Sengupta et al., 2012). Thus, to the limited extent our findings allow for the possibility of contact effects, they occur only in the very short-term, and only for policies that promote symbolic (and not material) forms of equality.

Discussion

For both practical and epistemological reasons, contact research relies heavily on survey data (Christ & Wagner, 2013; MacInnis & Page-Gould, 2015). Although these data cannot prove causality, they can provide varying degrees of support for it. Correlations and cross-lagged effects tested in extant research represent extremely weak evidence. Longitudinal within-person effects, after adjusting for between-person stability, represent stronger evidence (Zyphur et al., 2019). Specifically, they provide evidence that changes in the causal variable precede changes in outcome variable. Based on this principle, we used data from a seven-wave national probability sample to address a longstanding question in intergroup relations research: Does contact with the disadvantaged foster political attitudes that promote equality?

Results showed that advantaged-group members who had more outgroup contact across all years also expressed higher political solidarity with subordinate groups across all years (and vice versa). However, this tells us little about the extent to which a change in contact leads to a change in political solidarity—or even the extent to which a person’s level of contact is stable over time (e.g., if contact uniformly increased or decreased in the sample, one’s relative position in the sample would be unchanged despite everyone undergoing a change in

Table 2

Parameter Estimates From Model 2 for the Within-Person Effects of Every Variable at Time t − 1 With Each Variable at Time t

						99% CI	
Time *t*	Time *t* − 1	*b*	*SE*	*z*	*p*	Low	high
Cultural policy	**Intergroup contact**	**.008**	**.005**	**1.575**	**.115**	**−.005**	**.021**
	Cultural policy	.131*	.012	10.804	<.001	.099	.162
	Reparative policy	−.008	.008	−.962	.336	−.030	.014
	Collective action	−.038*	.005	−8.111	<.001	−.049	−.026
Reparative policy	**Intergroup contact**	**.006**	**.005**	**1.243**	**.214**	**−.006**	**.019**
	Cultural policy	.024*	.009	2.745	.006	.001	.046
	Reparative policy	.135*	.012	10.840	<.001	.103	.167
	Collective action	−.044*	.005	−8.799	<.001	−.057	−.031
Collective action	**Intergroup contact**	**.007**	**.008**	**.924**	**.356**	**−.013**	**.028**
	Cultural policy	−.050*	.012	−4.023	<.001	−.082	−.018
	Reparative policy	−.093*	.013	−7.448	<.001	−.126	−.061
	Collective action	.083*	.011	7.394	<.001	.054	.112
Intergroup contact	Intergroup contact	.086*	.010	8.199	<.001	.059	.113
	Cultural policy	.003	.014	.182	.855	−.033	.038
	Reparative policy	.008	.013	.581	.561	−.026	.041
	Collective action	−.015	.008	−1.823	.068	−.036	.006

Note. Focal relationships are shown in bold. *SE* = standard error; CI = confidence interval.
* $p < .01$.

contact). Thus, the between-person correlation does not provide evidence for a causal effect but rather indicates the potential effect of unobserved variables that cause both contact and solidarity to covary in the population across all waves.

The tests of within-person change, which would have been consistent with a causal effect, showed that those with higher-than-expected contact in a particular year *did not* express higher-than-expected solidarity 1 year later. Thus, we found no evidence that contact with subordinate groups increases support for policies or protests favoring those groups. This indicates that prior correlational research may have been capturing the time-invariant association between contact and solidarity (as found in the current work), rather than a psychological process in which contact increases solidarity. Finding within-person associations is not *sufficient* for inferring causal relationships since time-varying confounders can lead to spurious within-person associations (Rohrer & Murayama, 2021). Nonetheless, it relaxes many other assumptions for causal inference (e.g., by removing time-invariant confounders) that limit existing studies on contact and political solidarity.

This is, of course, only one study in one intergroup context and cannot make the claim that contact *does not* foster solidarity with the disadvantaged. But we had an excellent chance to detect within-person contact effects if they did indeed exist. Our study followed participants over 5–7 years, long enough for the hypothesized causal processes to unfold. We had exceptionally high power—as reflected in the very precise estimates for within-person effects[1]—due to our sample size and a large number of repeated measurements. The results also were consistent across multiple indices of contact and solidarity, all of which have been used extensively in prior research and show good criterion-related validity. Moreover, New Zealand is sociopolitically similar to other postcolonial nations where contact research is most often conducted, making it unlikely that unique features of the context prevented longitudinal contact effects from emerging. Indeed, within-person effects of contact have been observed in this very data set, but in a domain with an extremely high prior expectation of an effect. Consistent with the decades of multimethod research on prejudice reduction, there was some evidence that contact increased White New Zealander's warmth toward ethnic minorities over time (Barlow et al., 2019). In the present work, we find that these effects do not extend to supporting the political interests of ethnic minorities.

Overall, our results underscore the finding that people who are politically supportive of outgroups are also often those who spend time with and befriend outgroup members, whereas the unsupportive on average spend less time with outgroups (e.g., Hässler et al., 2020). They call to question, however, theorizing put forth by many (including members of this team) that spending more time with and befriending outgroup members *causes* people to be more politically supportive of them (Reimer et al., 2017; Tropp & Barlow, 2018). There is a need for more longitudinal contact research across different contexts that can separate between- and within-person effects to provide stronger evidence of causality.

There is also a need for more research to contribute longitudinal data to this debate that can overcome the limitations of our single study—specifically, the nature of the measures and the frequency of the repeated measurements. The use of short, self-report measures is unavoidable for a large-scale panel study such as the NZAVS, as this allows for large amounts of information to be gathered about diverse samples over long periods of time. However, shorter scales are less reliable, and self-report measures might not validly capture actual levels of contact and solidarity. Therefore, our findings should be interpreted with the caveat that better measurement of the key constructs may reveal significant within-person effects of contact on solidarity.

Nonetheless, it is worth noting that similar measures are routinely used in the extant literature—for example, single-item, or very short self-report scales for contact are typical in contact studies (e.g., see Schmid et al., 2017). These short scales produce the same cross-sectional association with solidarity as other contact measures (Hässler et al., 2020; see also Table S13). As such, correlational findings based on identical or very similar scales have previously been taken as evidence that contact increases solidarity (e.g., Reimer et al., 2017). The same logic applies when considering the self-report nature of our measures. To the extent that these indices lack validity, this is not just a problem for interpreting the current findings, but for all prior findings that purport to show that contact increases solidarity. Thus, by following established methods, the present study raises the prospect that the "standard practice" in contact research might be generating spurious correlations.

That said, there is reason to remain sanguine about the validity self-report measures of contact and solidarity. This is because they correlate positively with other measures that are putatively more objective. For example, self-reported outgroup contact correlates with levels of outgroup interaction derived from social network data (Wölfer et al., 2017). Similarly, measures of solidarity intentions correlate positively with measures of solidarity behavior (e.g., the proportion of potential payments donated to support minority rights; Reimer et al., 2017).

On the issue of the frequency of repeated measurements, a relevant caveat is that the 1-year lag used in this study was not chosen because of its theoretical relevance to the question of contact effects. For a national panel study aiming to assess

[1] This was true for all but three within-person effects across both models. Specifically, the estimates for the effects of the solidarity measures on the hours of contact variable (Model 1) were not as precise as the others (as reflected in the higher standard errors for these estimates; see Table 1). Nonetheless, the focal (null) effects of contact on solidarity were very precisely estimated.

hundreds of social and political indicators, decisions such as frequency of measurement are driven by broader considerations than the lag most relevant to testing a specific hypothesis. However, even if we had intended to select the optimal timeframe to detect contact effects a priori, we would not have found much guidance in contact theory. The difficulty of obtaining longitudinal data has meant that there is limited consideration in the contact literature of what the expected timeframe of contact effects should be. In the absence of such theorizing, it is possible that the 1-year lag in our study is either too short (implying that contact effects are relatively short lived) or too long (implying that contact is a relatively slow process). Thus, the present study highlights the need for more theoretical work on the temporal dynamics of contact to guide future longitudinal research.

Our findings also highlight a need for contact research to consider more carefully the nature of contact that is most likely to change political opinions (e.g., see Droogendyk et al., 2016). By indicating the infectiveness of contact, our results align with other research showing that political attitudes are highly stable and resistant to change over time (Hatemi & Verhulst, 2015). Despite this, some people clearly do develop a greater willingness to support the political struggles of the disadvantaged because of meaningful interactions with outgroup members. Stories of reformed White Supremacists who renounce their ideology after positive experiences with ethnic minorities (Lipman, 2020), or of ethnic minority activists successfully reforming committed racists over time (Brown, 2017), provide anecdotal evidence of such a process.

Moreover, it remains plausible that certain highly immersive contact experiences—such as moving to a diverse residential university during one's formative years (see Sidanius et al., 2008), or living through a process of rapid desegregation (see Farley & Frey, 1994)—might shift political attitudes. However, understanding these mechanisms of change would require a different conceptual and empirical approach than presently employed in much of the contact literature (i.e., to measure self-reported positive contact and solidarity in the general population). Our findings suggest that, on average across the population, contact is correlated with, but not predictive of, levels of solidarity toward outgroups.

The present study reinforces longstanding doubts about whether contact can promote social change toward equality by fostering allyship with the disadvantaged (Jackman & Crane, 1986). This does not, however, negate the well-researched benefits of contact for reducing prejudice (Pettigrew & Tropp, 2006). In contexts where conflict is rife and harmony is the most pressing goal, contact theory offers a clear and empirically supported strategy for improving intergroup relations. However, in the relatively peaceful (yet unequal) societies that characterize much of the industrialized world, fostering ever stronger bonds between groups may offer limited value for achieving social change.

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