

**Does intergroup contact foster solidarity with the disadvantaged? A longitudinal
analysis across seven years**

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Abstract

Contact theory is a well-established paradigm for improving intergroup relations – positive contact between groups promotes *social harmony* by increasing intergroup warmth. A longstanding 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 towards subordinate groups, thus furthering the goal of equality. However, extant 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 contact measures and three solidarity measures ($N = 22,646$). Results showed no within-person change over a one-year period (inconsistent with a causal effect), but significant between-person stability (consistent with third-variable explanations). This reinforces doubts about contact as strategy for promoting equality.

Does intergroup contact foster solidarity with the disadvantaged? A longitudinal analysis across seven years

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 this latter issue of (in)equality (Dixon et al., 2005; Reicher, 1986; Reimer & Sengupta, 2021; Saguy et al., 2008). Indeed, early research suggested that despite fostering greater warmth towards 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 groups, the more they support policy proposals and protest movements favouring those groups (Dixon et al., 2007; Kamberi et al., 2017; Selvanathan et al., 2018). In the most comprehensive analysis to date, Hässler et al. (2020) sampled 8114 individuals from advantaged groups across 69 countries and found that contact was associated with increased support for social change favouring the disadvantaged. This kind of *political solidarity* shown by advantaged groups is crucial for the disadvantaged to achieve greater equality (Subašić 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, relative 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 current article, we explain how the challenge of testing causal effects in contact research can be addressed by applying the latest developments in longitudinal modelling to largescale observational data. We then apply the suggested models (Hamaker et al., 2015) to nationally representative data across seven waves 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 collective action; a necessary condition for any conclusion that contact fosters intergroup equality by increasing the political solidarity expressed by advantaged groups towards 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 – i.e., a process of increasing intimacy and eventual friendship with outgroup members over time (MacInnis & Page-Gould, 2015; see also Yarkoni, 2019).

As a result, the overwhelming majority of contact research has used observational data – specifically, self-reported levels of contact and intergroup attitudes (Christ & Wagner,

2013). One way of making somewhat stronger causal inferences from this type of data is to assess contact and intergroup attitudes over time. Longitudinal analyses, such as the cross-lagged panel model (see Fig. 1), can then be used to test whether higher levels of outgroup contact at a given timepoint predict improved attitudes at a subsequent timepoint. This helps establish a necessary condition for causal interference, *temporal precedence*; a cause must precede its hypothesized effect.

Cross-lagged panel modelling accounts for several 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 timepoint (by modelling the time-specific intercepts for each variable). Second, they adjust for the fact that the level of a variable at a given timepoint is dependent on its value at a preceding timepoint (by modelling auto-regressive effects; parameters $\beta_{x1}^{(x)}$ and $\beta_{y1}^{(y)}$ in Fig. 1). Third, they adjust for unobserved variables that might simultaneously cause both predictor and outcome at a given timepoint (by modelling the time-specific residual covariances between the predictor and outcome; parameters $u_{t-n}^{(y)}$ in Fig 1). Given these advantages, and in the absence of generalisable experimental paradigms, researchers have increasingly employed cross-lagged panel models to test the effects of intergroup contact (Dhont et al., 2012; Meleady et al., 2020; Swart et al., 2011).

However, only one longitudinal study to date has examined whether contact with the disadvantaged predicts *increased political solidarity* among advantaged groups over time. Using cross-lagged panel modelling, Reimer et al. (2017) showed that the more contact heterosexual participants had with non-heterosexuals, the more willing they were to engage in collective action on behalf of the LGBTQ+ community three months later. Although this finding aligns with the correlational research on solidarity effects, it provides limited incremental evidence for a casual effect of contact.

This is because cross-lagged panel models do not account at least one other confound that can produce spurious effects in longitudinal designs: *between-person stability* (Rogosa, 1980; Hamaker et al., 2015). Between-person stability refers to the degree to which time-invariant levels of one variable correlate with time-invariant levels of another – e.g., people with higher levels of contact across all timepoints also showing higher levels of outgroup political solidarity across all timepoints and vice versa (parameter $\psi_{\eta}^{(xy)}$ in Fig. 1). This between-person correlation can be considered a confound when estimating causal effects because factors modelled 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 Fig. 1). This is the degree to which someone who had higher than expected levels of the predictor at one timepoint, relative to their average levels, shows higher than expected levels of the outcome at a subsequent timepoint (Hamaker et al., 2015). Thus, modelling within-person change allows for more accurate estimates of hypothesized causal relationships than extant correlational or cross-lagged effects. Hamaker et al. (2015) demonstrated this improved accuracy empirically, by developing a *random-intercept cross-lagged panel model* to disentangle between-person stability and within-person change.

The present study

Here, we use a random-intercept cross-lagged panel model to examine whether the association between contact and outgroup political solidarity observed in extant research results from (a) stable individual differences in contact correlating with stable individual differences in solidarity across the population (*between-person stability*), (b) people with higher than expected contact at one timepoint expressing higher than expected solidarity at a later timepoint (*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 at 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 overall sample size (for full details on the procedure see Sibley, 2019). 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.

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 socio-economic indicators (The Social Report, 2010). 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 – 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 marches and public demonstrations

supporting the rights of Māori” (1, strongly oppose – 7, strongly support; Osborne & Sibley, 2013).

Cultural Policies. A second measure includes four items ($\alpha = .77$) assessing support for cultural policies benefitting Māori (1, strongly oppose – 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 ($\alpha = .83$) assessing support for reparative policies benefitting Māori (1, strongly oppose – 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 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).

Results

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 timepoints of the NZAVS – i.e., 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 22,646 New Zealand European participants ($M_{\text{age}} = 51.36$, $SD = 13.74$; 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. 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.

Modelling Strategy. Hamaker et al. (2015) showed that the standard longitudinal approach used in extant research (i.e., cross-lagged panel modelling; CLPM), produces biased estimates of change over time because it does not separate time-invariant, between-person stability from longitudinal, within-person change. To solve this problem, Hamaker et al. (2015) developed a new modelling strategy termed “random-intercept cross lagged panel modelling” (RI-CLPM). We applied this new technique to disentangle (a) the between-person stability in the correlation between contact and solidarity from (b) the within-person change in political solidarity predicted by prior levels of contact.

Following the RI-CLPM procedure laid out by Hamaker et al. (2015), each observed variable was modelled as a function of two latent variables. One latent variable was the time-invariant random intercept, which was modelled 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 variance. 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 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 to equality the coefficients of Time 1 to Time 2 variables, Time 2 to Time 3 variables, Time 3 to Time 4 variables etc. 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).

Model Fit. The motivation for conducting an RI-CLPM is to improve upon the standard longitudinal approach (i.e., the cross-lagged panel model; CLPM). To test this improvement formally, we compared the fit of our model with a traditional CLPM. Results showed that the RI-CLPM of contact and the three indices of solidarity ($\chi^2_{(310)} = 3,981.869$; CFI = .989; RMSEA = .023, 95% CI [.022, .024]; sRMR = .041) fit the data well, and significantly better than the CLPM ($\chi^2_{(320)} = 41,193.913$; CFI = .878; RMSEA = .075, 95% CI [.074, .076]; sRMR = .126; $\Delta\chi^2_{(10)} = 36,212.044$; $p < .001$). This establishes that modelling the between- and within-person levels separately is indeed necessary.

Parameter Estimates.

Between-person correlations. We found that the random intercept for contact was positively correlated with the random intercepts of support for collective action ($r = .24, se = .01, p < .001; 99\% CI [.22, .26]$), cultural policies ($r = .18, se = .01, p < .001; 99\% CI [.16, .20]$), and reparative policies $r = .29, se = .01, p < .001; 99\% CI [.27, .30]$. 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 ($r = .59, se = .01, p < .001; 99\% CI [.57, .61]$) and reparative policies ($r = .45, se = .01, p < .001; 99\% CI [.43, .47]$). Finally, the random intercept for cultural policies was positively correlated with the random intercept for reparative policies ($r = .60, se = .01, p < .001; 99\% CI [.59, .62]$).

Within-person coefficients. Crucially, the within-person effects of contact on support for collective action, cultural policies and reparative policies were non-significant (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 one year later. This finding is inconsistent with a causal effect of contact on solidarity because it does not establish that changes in contact precede changes in solidarity.

Innovations. Our RI-CLPM model also estimated associations between the time-specific latent variables within each time point. Hamaker et al. (2015) refer to these within-wave, within-person correlations as "innovations". Innovations 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 "innovation" 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 innovations are cross-sectional associations, raises the same problems with inferring causality that are inherent in any cross-sectional estimate. Thus, significant innovations would, at most, indicate that short-term causal effects cannot be entirely ruled out. In Model 1, however, we found no significant contact-solidarity innovations (see supplementary online 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) – i.e., 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 ($M_{\text{age}} = 51.71$, $SD = 14.46$; 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.

Modelling Strategy. We used the same modelling strategy as in Model 1 – i.e., a random-intercept cross-lagged panel model (Hamaker et al., 2015).

Model Fit. Again, we compared the fit of our model with a traditional CLPM and found that modelling the between- and within-person levels separately was indeed necessary.

The RI-CLPM of contact and the three indices of solidarity ($\chi^2_{(134)} = 1,112.160$; CFI = .995; RMSEA = .021, 95% CI [.020, .022]; sRMR = .066) fit the data well, and significantly better than the CLPM ($\chi^2_{(144)} = 14,022.125$; CFI = .931; RMSEA = .075, 95% CI [.074, .076]; sRMR = .126 $\Delta\chi^2_{(10)} = 12,909.965$; $p < .001$).

Parameter Estimates. We replicated the same pattern of between- and within-person effects as in Model 1.

Between-person correlations. The random intercept for contact was positively correlated with the random intercepts of support for collective action ($r = .36$, $se = .01$, $p < .001$; 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 [.28, .33]). The random intercept for collective action was also positively correlated with the random intercepts for cultural policies ($r = .69$, $se = .01$, $p < .001$; 99% CI [.67, .70]) and reparative policies ($r = .84$, $se < .01$, $p < .001$; 99% CI [.83, .85]). 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 non-significant (see Table 2 for all within-person effects in the model).

Innovations. In contrast to Model 1, Model 2 did produce some significant innovations (see supplementary online materials, Tables S8-S12). However, roughly the same number of innovations were non-significant (seven) as were significant (eight), making it difficult to draw any substantive conclusions about a causal evidence for the contact-solidarity relationship). Moreover, the most consistent innovations 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, Barlow, & Sibley, 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 observational data (MacInnis & Page-Gould, 2015; Christ & Wagner, 2013). 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 for temporal precedence in which changes in the causal variable precede changes in outcome variable. Based on this principle, we used seven waves of nationally representative data 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, those with higher than expected contact in a particular year *did not* show higher than expected solidarity one year later. Thus, we found no evidence, at least in New Zealand, that contact with subordinate groups increases support for policies or protest favouring 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.

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. Nonetheless, 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 – due to our sample size and the 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 construct validity (NZAVS, 2019). Moreover, New Zealand is socio-politically 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 dataset, but in a domain with an extremely high prior expectation of an effect. Specifically, consistent with the decades of multi-method research on prejudice reduction, there was some evidence that contact increased White New Zealander’s warmth towards 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 minorities¹.

Overall, our results underscore the finding that people who are politically supportive of outgroups are also often those who befriend outgroup members, whereas the unsupportive on average have fewer outgroup friends (e.g., Hässler et al., 2020). They call to question, however, theorising put forth by many (including members of this team) that it is contact causally affecting such support (Reimer et al., 2017; Tropp & Barlow, 2018). There is need for more longitudinal contact research across different contexts that can separate between- and within-person effects to provide stronger evidence of causality.

¹ Finding within-person associations, however, is not *sufficient* for inferring causal relationships since time-varying confounders can lead to spurious within-person associations (Rohrer & Murayama, 2021). Still, it relaxes many other assumptions for causal inference (e.g., no time-invariant confounders) that plague extant evidence for the relationship between intergroup contact and solidarity in advantaged groups.

Conclusion

The current study reinforces longstanding doubts about whether contact can promote social change towards 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 characterise much of the industrialised world, fostering ever stronger bonds between groups may offer limited value for achieving social change.

Author contributions: N. K. Sengupta developed the study concept. All authors contributed to the study design. Data collection was performed by C. G. Sibley and Sengupta. Sengupta and N. K. Reimer performed the analyses simultaneously and independently to ensure accuracy of the reported estimates. F. K Barlow provided critical feedback on the interpretation of the findings. Sengupta drafted the manuscript, and Reimer, Barlow and Sibley provided critical revisions. All authors approved the final version of the manuscript for submission.

Open practices statement: Neither of the two analyses reported in the current study were preregistered. Anonymized data are available upon request to qualified researchers. All questionnaire materials technical information about the sample, and analysis code, are available at <https://www.psych.auckland.ac.nz/en/about/new-zealand-attitudes-and-values-study.html>

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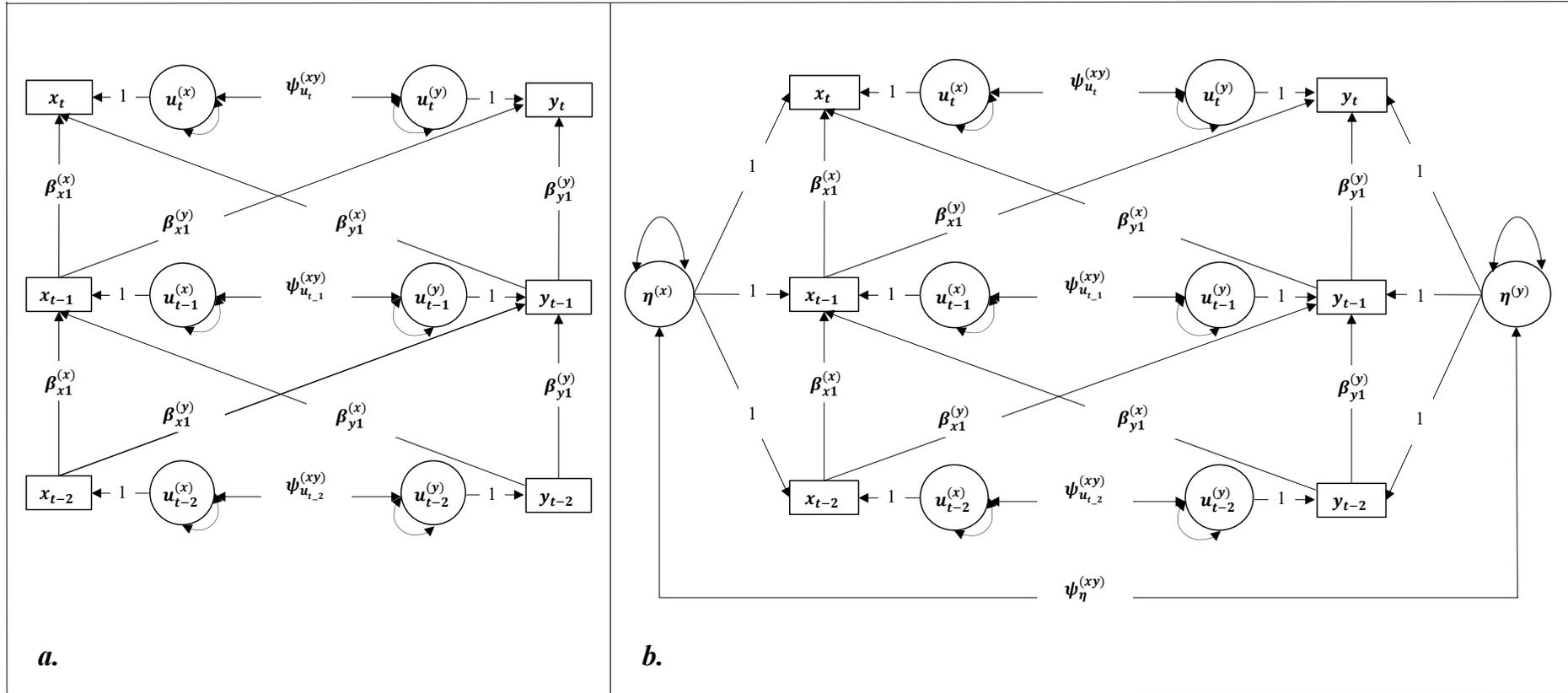


Figure 1. Path diagram showing parameters estimated in a cross-lagged panel model (a) and a random-intercept cross-lagged panel model (b). Note. x_{t-n} is the predictor at time $t-n$, y_{t-n} is the outcome at time $t-n$, $\beta_{x1}^{(x)}$ is the autoregressive effect of the predictor on itself, $\beta_{y1}^{(y)}$ is the autoregressive effect of the outcome on itself, $u_{t-n}^{(x)}$ is the residual variance of the predictor at time $t-n$, $u_{t-n}^{(y)}$ is the residual variance of the outcome at time $t-n$, $\psi_{u_{t-n}}^{(xy)}$ is the residual covariance of the predictor and the outcome at time $t-n$, $\beta_{x1}^{(y)}$ is the cross-lagged effect of the predictor on the outcome, and $\beta_{y1}^{(x)}$ 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, $\psi_{\eta}^{(xy)}$ 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, $\beta_{x1}^{(y)}$, represents within-person change (i.e., a possible causal effect).

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	<i>b</i>	<i>se</i>	<i>z</i>	99% <i>CI</i>	
					<i>low</i>	<i>high</i>
Cultural Policy	Intergroup Contact	<.01	<.01	1.24	<.01	<.01
	Cultural Policy	.16*	.01	26.91	.14	.17
	Redistributive Policy	.02*	<.01	3.32	<.01	.03
	Collective Action	.01*	<.01	4.37	.01	.02
Reparative Policy	Intergroup Contact	<.01	<.01	.81	<.01	<.01
	Cultural Policy	.03*	.01	5.65	.02	.05
	Reparative Policy	.19*	.01	31.12	.18	.21
	Collective Action	.03*	<.01	7.71	.02	.04
Collective Action	Intergroup Contact	<.01	<.01	.14	<.01	<.01
	Cultural Policy	.06*	.01	7.24	.04	.08
	Reparative Policy	.09*	.01	10.31	.06	.11
	Collective Action	.09*	.01	16.31	.08	.11
Intergroup Contact	Intergroup Contact	-.01	<.01	-1.80	-.01	<.01
	Cultural Policy	-.12	.15	-.76	-.51	.28
	Reparative Policy	-.32	.15	-2.13	-.71	.07
	Collective Action	.21	.10	2.23	-.03	.45

Note. * $p < .01$. Focal relationships shown in bold.

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.

Time t	Time t-1	<i>b</i>	<i>se</i>	<i>z</i>	99% <i>CI</i>	
					<i>low</i>	<i>high</i>
Cultural Policy	Intergroup Contact	.01	<.01	1.56	-.01	.02
	Cultural Policy	.13*	.01	14.08	.11	.15
	Reparative Policy	<.01	.01	-.20	-.02	.02
	Collective Action	.02*	.01	3.97	.01	.03
Reparative Policy	Intergroup Contact	.01	.01	1.09	-.01	.02
	Cultural Policy	.03*	.01	3.36	.01	.05
	Reparative Policy	.15*	.01	15.41	.12	.17
	Collective Action	.02*	.01	4.08	.01	.03
Collective Action	Intergroup Contact	.01	.01	.91	-.01	.02
	Cultural Policy	.07*	.01	5.41	.04	.10
	Reparative Policy	.06*	.01	4.41	.02	.09
	Collective Action	.10*	.01	11.47	.08	.12
Intergroup Contact	Intergroup Contact	.09*	.01	10.29	.06	.11
	Cultural Policy	<.01	.01	.34	-.03	.04
	Reparative Policy	.01	.01	.74	-.02	.04
	Collective Action	-.01	.01	-.92	-.03	.01

Note. * $p < .01$. Focal relationships shown in bold.