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Abstract

Despite an increasing interest in the drivers of intergroup solidarity, the within-person longitudinal relationships between advantaged group members' engagement for disadvantaged groups and its postulated antecedents remain scarcely tested. In the context of the refugee crisis following Russia's invasion of Ukraine, we conducted a 3-wave longitudinal survey (N_{T1} = 804, N_{T2} = 702, N_{T3} = 624) assessing Poles' (the advantaged group) willingness to act for Ukrainians (the disadvantaged group), together with three hypothesized predictors – moral convictions, intergroup contact, and politicized identity. Employing a random intercept cross-lagged panel model that separates between- from within-person variance, we found that within-person changes in moral convictions and friendship contact directly predicted subsequent action intentions. Contrary to past theorizing, politicized identity emerged as consequence rather than an antecedent of collective action. Superficial intergroup contact indirectly predicted engagement intentions by facilitating cross-group friendship. We discuss the implications of our findings for current models of collective action.

Keywords: collective action for a disadvantaged group, politicized identity, moral convictions, intergroup contact, longitudinal methodology

Recent years witnessed a substantial increase in collective action research (Agostini & van Zomeren, 2021). Next to the collective action of disadvantaged groups to improve their status, more attention has been paid to advantaged group members' engagement for disadvantaged outgroups. Apart from high-quality studies (e.g., Hässler et al., 2020), the literature has been enriched by significant theoretical developments (Radke et al., 2020). Still, some issues remain rather underresearched. One of these are causal associations between collective action for disadvantaged outgroups and its psychological drivers (for exceptions, see Reimer et al., 2017; Sengupta et al., 2022). Out of multiple antecedents of pro-outgroup engagement proposed in the literature (for a review, see Radke et al., 2020), many (e.g., cross-group friendship or moral convictions) do not succumb to experimental manipulation (e.g., MacInnis & Page-Gould, 2015). Moreover, there are reasons to believe that the relationships between collective action for disadvantaged outgroups and its hypothesized determinants are, in fact, reciprocal, and that the antecedents themselves are connected by a complex network of mutual relationships. In these circumstances, longitudinal data in connection with the appropriate statistical apparatus (see Rohrer & Murayama, 2021) seem to be the most optimal way to investigate the antecedents and consequences of pro-outgroup engagement.

In this contribution, we add to the existing literature by examining the longitudinal associations between collective action for a disadvantaged outgroup and its four postulated antecedents – politicized identity, moral convictions, personal intergroup contact, and cross-group friendship (see Radke et al., 2020). We do so in the context of the refugee crisis that arose following Russia's invasion of Ukraine in February 2022. Using 3-wave longitudinal data collected in a nationwide sample of adult Poles (the advantaged group) at the beginning of the war, we estimate the within-person longitudinal associations between collective action for Ukrainians (the disadvantaged group) and its hypothesized determinants. To this aim, we employ the random intercept cross-lagged panel model (RI-CLPM; Hamaker et al., 2015) –

an extension of the traditional cross-lagged panel model (CLPM; e.g., Wu et al., 2013) that distinguishes between-person and within-person variance. Below, we briefly review the literature on the antecedents of collective action for disadvantaged outgroups, introduce the RI-CLPM, and explain the advantages of its use in longitudinal collective action research.

Collective action for disadvantaged outgroups

Many contributions to the collective action literature point to politicized identity (i.e., identification with a social movement; Simon & Klandermans, 2001) as the key antecedent of advantaged group members' engagement for a disadvantaged outgroup. For instance, the extended social identity model of collective action (extended SIMCA; van Zomeren et al., 2018) posits that, when individuals perceive a normative fit between their moral beliefs and the principles of a given social movement, moral convictions facilitate politicized identity development and, in consequence, motivate protest behavior (for evidence, see van Zomeren, Postmes, & Spears, 2012). Recently, Radke and colleagues (2020) proposed that identification with a politicized group aimed at improving the circumstances of a disadvantaged outgroup is likely to reflect a genuine outgroup-focused motivation of advantaged group members.

At the same time, protest behavior was shown to increase identification with social movements (e.g., Becker et al., 2011). Therefore, one may expect that politicized identity simultaneously motivates and originates from pro-outgroup engagement. However, longitudinal research testing this hypothesis is missing. Although Thomas et al. (2021) assessed both identification with the global poverty movement and collective action for a disadvantaged outgroup over multiple occasions, they did not test the over-time associations between these variables. Instead, it was examined whether the *initial* level of politicized identity explained variability in the developmental trajectories of collective action. Our study

aims to fill this gap by examining politicized identity both as an antecedent and a consequence of collective action for a disadvantaged outgroup.

Moral beliefs (i.e., perceptions of what is right and what is wrong; Skitka, 2010) constitute another hypothesized determinant of engagement for disadvantaged outgroups. As moral principles have an absolute character (i.e., are believed to apply to everyone, regardless of group membership; Skitka, 2010; van Zomeren et al., 2018), any behaviors or situations that offend these standards elicit anger (i.e., moral outrage; see Batson et al., 2007) and facilitate action. In the context of asymmetrical power relations, advantaged group members who endorse egalitarian ideals may be motivated to engage for a disadvantaged outgroup to uphold their moral standards (Radke et al., 2020). The explanatory power of moral beliefs was demonstrated in multiple cross-sectional studies, including those embedded in hierarchical group contexts (e.g., van Zomeren, Postmes, & Spears, 2012). For instance, moral convictions (i.e., attitudes on a particular issue that are rooted in one's core values; Skitka, 2010; van Zomeren et al., 2018) predicted greater volunteerism and political activism among Hungarians engaged in supporting refugees during the 2015 crisis (Kende et al., 2017). However, we are not aware of any study that has examined the longitudinal association between moral convictions and collective action for a disadvantaged outgroup.

Finally, advantaged group members' engagement for a disadvantaged outgroup may originate from their positive relations with outgroup members. Numerous studies show that intergroup contact not only improves attitudes toward outgroups (Davies et al., 2011; Pettigrew & Tropp, 2006), but also promotes collective action on their behalf (e.g., Hässler et al., 2020; Reimer et al., 2017; but see Sengupta et al., 2022). The form of intergroup contact that may be especially effective in motivating engagement is cross-group friendship (MacInnis & Hodson, 2019). Specifically, in the course of repeated, intimate interactions with their outgroup friends, advantaged group members are likely to take the perspective of the

disadvantaged outgroup (e.g., Cakal et al., 2021), recognize the inequality that exists between groups (e.g., Lyttle et al., 2017), and empathize with those who occupy lower positions in the social hierarchy (Swart et al., 2011). As all these factors are associated with collective action for the disadvantaged outgroup (e.g., Hässler et al., 2022; Mallett et al., 2008; Saab et al., 2015, Selvanathan et al., 2017), we propose that cross-group friendship would facilitate this type of engagement. Analogous effects are likely to be weaker, or even non-existent, for superficial (i.e., lacking friendship-specific features such as repeated interactions across many situations or mutual self-disclosure; see Davies et al., 2011) forms of intergroup contact. On the other hand, intergroup contact is theorized to improve outgroup-directed attitudes primarily in the early stages of intergroup relationships (MacInnis & Page-Gould, 2015; Page-Gould, 2022). As such, when the coexistence of the advantaged and disadvantaged groups is relatively recent, superficial contact may be sufficient to motivate collective action for a disadvantaged outgroup.

While considering the psychological underpinnings of collective action for a disadvantaged outgroup, one should keep in mind that standing up for intergroup equality may feed back into its antecedents (Becker & Tausch, 2015; van Zomeren, Leach, & Spears, 2012). For instance, equality-based moral beliefs may not only prompt advantaged group members to fight for social change, but also develop in consequence of such engagement. In a similar vein, participating in a demonstration aimed at improving the situation of the disadvantaged outgroup may be a good opportunity to befriend members of this group or strengthen identification with the pro-outgroup social movement.

Moreover, determinants of collective action for the disadvantaged outgroup are likely to be interrelated. For example, moral convictions may define politicized identity and, by doing so, stimulate engagement for social change (van Zomeren et al., 2018; for an alternative view, see Agostini & van Zomeren, 2021). A similar effect may be expected for intergroup

contact – in consequence of positive, close relations with outgroup members, advantaged group members may recognize the injustice of intergroup status differences and develop identification with social movements that seek to flatten intergroup hierarchy (MacInnis & Hodson, 2019). To disentangle these potentially reciprocal relationships, an appropriate statistical framework is needed.

The Dual Nature of Collective Action

Collective action is not a static phenomenon. Instead of maintaining a constant level over time, people's readiness to engage in protest behavior is subject to temporal fluctuations (e.g., Tausch et al., 2013; Thomas et al., 2021). Several theoretical models have been proposed to capture the dynamic aspect of engagement (e.g., Becker & Tausch, 2015; Drury & Reicher, 2009; Louis et al., 2020; van Zomeren, Leach, & Spears, 2012). In these models, changes in collective action are hypothesized to originate from (and translate into) changes in a range of psychological constructs (e.g., social identity, efficacy, or perceived injustice)

On the other hand, one may think of collective action as a relatively stable, trait-like characteristic. It has been acknowledged that protest participation operates in "packages" – rather than fighting for a single cause, people tend to engage in collective action related to multiple (albeit connected) issues (e.g., Andersen & Jennings, 2010; Louis et al., 2016). Moreover, a considerable body of research points to personality as a distal antecedent of collective action (e.g., Besta et al., 2021; Mondak et al., 2010).

Although the dual (i.e., dynamic vs. stable or state-like vs. trait-like) nature of collective action has been recognized in the literature (e.g., Louis et al., 2020), only several studies so far have sought to separate the two aspects empirically. While Thomas et al. (2021) focused on determining individual differences in collective action trajectories, only Sengupta et al. (2022) simultaneously modeled *between*- and *within*-person relationships between intergroup contact and pro-outgroup engagement. The scarcity of research isolating between-

and within-person components of the investigated variables is a serious neglect as the causal relations postulated in psychological models of collective action (e.g., Becker & Tausch, 2015; van Zomeren, Leach, & Spears, 2012) can be conceived of as operating at the within-person level of analysis.

Longitudinal data in different areas of psychology, including the collective action literature (e.g., Thomas et al., 2020), has by default been analyzed with the CLPM.[1] In this framework, the constructs of interest assessed at time T are regressed on themselves (autoregressive effects) and other variables (cross-lagged effects) measured at T-1. As such, a cross-lagged effect in CLPM tests for the prospective effect of a given construct on change in individual differences in another (Orth et al., 2021).

Recently, however, the limitations of the CLPM have been highlighted. The main line of criticism is that the CLPM confounds between-person and within-person variance (e.g., Hamaker et al., 2015, Lucas, 2022; Usami et al., 2019). It is therefore not an appropriate statistical tool to simultaneously account for stability and dynamism in collective action, or to capture the within-person longitudinal associations between engagement and other variables. In addition, as shown by simulation analyses, the CLPM is likely to find spurious cross-lagged effects when there are none and underestimate cross-lagged effects when they exist (Lucas, 2022).

These limitations do not apply to the RI-CLPM. In this model, stable between-person variance in each construct is captured by a separate trait factor (i.e., random intercept factor). The within-person variance, on the other hand, serves as the basis for estimating longitudinal associations between the variables of interest. In contrast to the CLPM, the autoregressive and cross-lagged effects in the RI-CLPM are modeled on the residualized scores and not on

[1] This may be because many longitudinal studies on protest behavior have only two time-points (e.g., Bilali et al., 2020; Górska et al., 2020, 2022), which does not allow using more complex models of longitudinal relationships (Usami et al., 2019).

observed scores. A positive autoregressive effect of construct A thus reveals whether a within-person deviation from the trait level of construct A predicts analogical deviation in the subsequent occasion. Thus, it indicates whether a positive or negative deviation from the trait level of a given construct carries over from one measurement to the next (Hamaker et al., 2015; Usami et al., 2019). Meanwhile, a significant cross-lagged effect of construct A on construct B means that a within-person deviation from the trait level of construct A predicts a within-person deviation from the person's mean for construct B in the following measurement. Therefore, RI-CLPM is an optimal choice when reciprocal relations between constructs are investigated (Usami et al., 2019).

The Current Research

The context for our research was the immediate aftermath of Russia's invasion of Ukraine on February 24th, 2022, which led to around 3.5 million Ukrainians seeking refuge from the war in Poland, (Derewienko, 2022). The refugees received a warm welcome from Polish society, with thousands of private individuals helping them by raising funds and offering free transport and accommodation (Petriczko, 2022). Material help was also dispatched to Ukrainian cities (Brzozowski, 2022).

In this study, we sought to identify the psychological sources and consequences of Poles' collective action for the people of Ukraine (both the refugees and those who remained in their country). To this end, we conducted an online longitudinal study with three measurements separated by two-week intervals. Data collection lasted between March 14th and April 14th, 2022. Data, code, and the full questionnaire are available on the project's OSF website (https://osf.io/e54bk/?view_only=c23e5b247e694148b260ac65d958279b).[2]

Method

[2] Both the study design and the questionnaire (but not the analyses reported in the Results section) were pre-registered (https://osf.io/qb7se/?view_only=a0ef5d8827814000bc4fc0bec7371297). The preregistered analyses concerned the developmental trajectories of collective action (see the Online Supplement).

Sample

Our study involved three (N_{T1} = 804, N_{T2} = 702, N_{T3} = 624) 20-minute measurements. Participants were recruited from an online panel held by an external company (Pollster) and were compensated with credit points that could be exchanged to material rewards. The intended sample size was no less than 500 individuals who participated in all three measurements. This was determined based on funding availability and, given the 80% power, sufficed to detect correlations of $|r| > .12$.[3] After excluding individuals who did not declare Polish nationality, data from 620 individuals (314 men and 306 women aged between 18 and 84, M = 48.87, SD = 16.11) who completed the questionnaire for all measurements were analyzed.[4] The final sample was representative of the Polish adult population in terms of gender and age distribution.[5]

Measures

The measures presented below were embedded in a longer questionnaire that assessed various social-psychological constructs. Unless otherwise noted, all measures used a 1 (*strongly disagree*) to 5 (*strongly agree*) response scale.

Collective action intentions[6]

To assess normative[7] collective action intentions, we asked participants how likely they were to engage in seven behaviors (i.e., participating in a demonstration, donating money to an aid organization, providing material assistance, organizing transport for refugees, providing accommodation to refugees, hanging the Ukrainian flag in the window/on the balcony, adding the Ukrainian flag to their profile picture on social media) in the next two

[3] For the post hoc Monte Carlo power analysis, see the Supplement.
[4] For the full sample (N = 798, after excluding non-Poles) analyses, see the Online Supplement.
[5] For detailed analyses of sample composition, see the Online Supplement.
[6] We also assessed past collective action (see the Online Supplement).
[7] Given that our theorizing concerned normative engagement, the results for nonnormative collective action are presented in the Online Supplement.

weeks to support Ukraine and Ukrainians. The response scale ranged from 1 (*very unlikely*) to 7 (*very likely*).

Personal intergroup contact[8]

The measure of personal intergroup contact consisted of two items: "Do you personally know any Ukrainians?" (1 = *no, I don't*, 2 = *yes, one or two*, 3 = *yes, a few*, 4 = *yes, a lot*, 5 = *yes, many*), and "How often do you talk to Ukrainians?" (1 = *never*, 2 = *once a year*, 3 = *once a month*, 4 = *once a week*, 5 = *everyday*).

Cross-group friendship

To assess cross-group friendship, we employed two items: "Do you have any Ukrainians among your friends?" (1 = *no, I don't*, 2 = *yes, one or two*, 3 = *yes, a few*, 4 = *yes, a lot*, 5 = *yes, many*), and "How often do you talk to Ukrainians who are your friends?" (1 = *never*, 2 = *once a year*, 3 = *once a month*, 4 = *once a week*, 5 = *everyday*).

Politicized identity

To gauge politicized identity, two items were used: "I identify with people who help Ukrainians" and "I identify with the movement to support Ukrainians."

Moral convictions

Moral convictions were measured with two items: "My opinion about helping Ukrainians reflects an important part of who I am" and "My opinion about helping Ukrainians is an important part of my moral norms and values" (van Zomeren, Postmes, & Spears, 2012).

Results

Analytic strategy

Our analyses involved estimating two (i.e., stationary and non-stationary, see below) RI-CLPMs (see Figure 1). Each of these models included five observed variables – collective

[8] The preregistered measure of intergroup contact comprised four items, which encompassed both personal intergroup contact and cross-group friendship questions. Expecting different results for these two forms of intergroup contact, we decided to split the original measure. For the results of analyses that did not distinguish between different types of intergroup contact, see the Online Supplement.

action intentions, politicized identity, moral convictions, personal intergroup contact, and cross-group friendship – assessed across three measurement occasions. To account for between-person stability of the analyzed constructs, a latent random intercept (i.e., participants' average level of the given construct over three measurements) for each construct was created. Factor loadings linking random intercepts with respective observed variables were fixed to 1. The five random intercepts were allowed to correlate. The within-person part of our model consisted of temporal deviations from participants' expected scores (i.e., random intercepts). The 15 (5 variables × 3 measurements) deviation scores were reflected by latent variables linked to the observed variables by factor loadings fixed to 1. As the residual variances of the observed variables were fixed to 0, we decomposed variance in the observed variables into two parts – a between-person component (i.e., the random intercepts), and a within-person component (i.e., latent within-person deviation scores). The within-person autoregressive and cross-lagged effects were created by regressing each deviation score at T3 or T2 on all deviation scores obtained at T2 or T1, respectively.

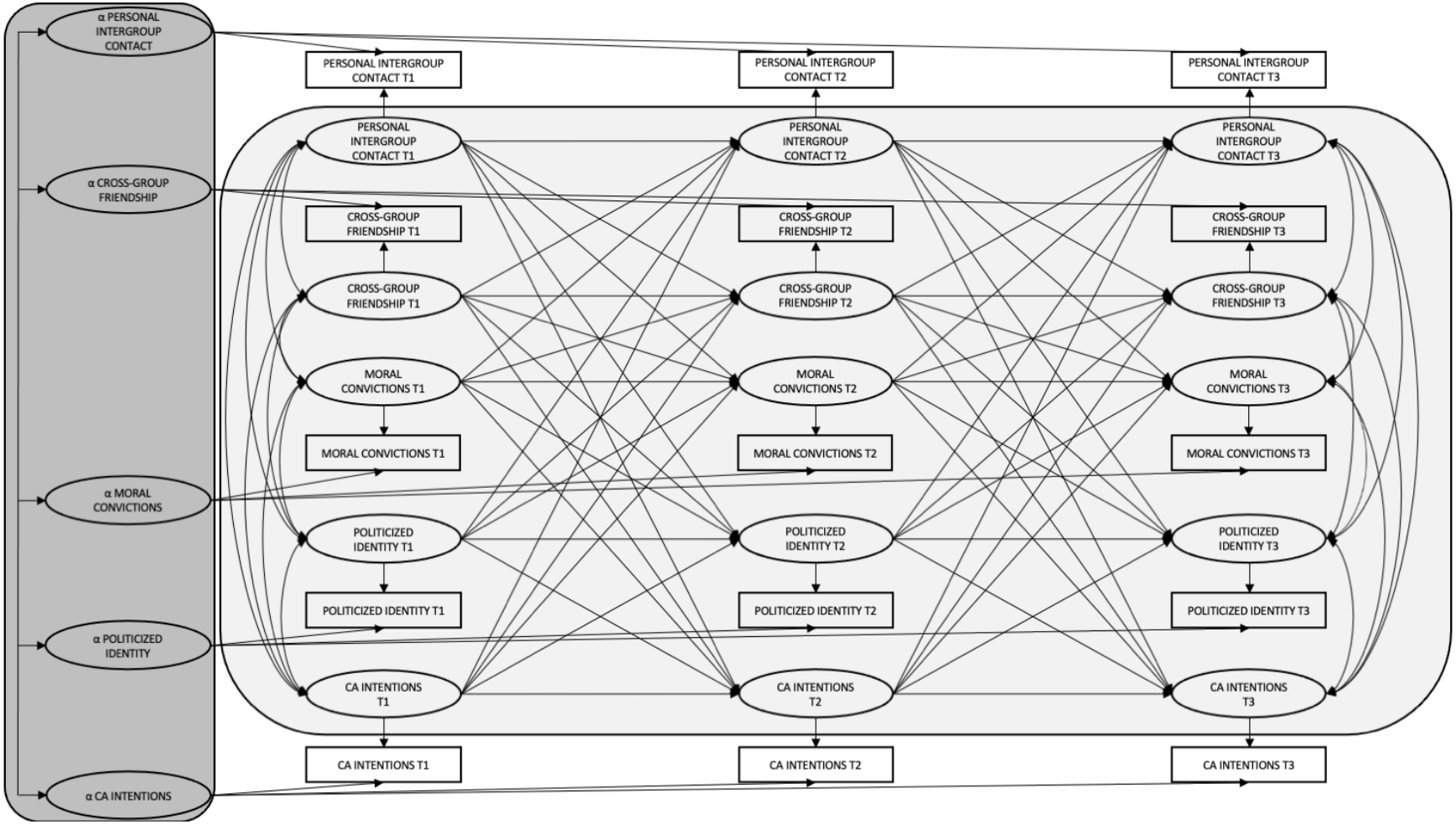

Figure 1. Conceptual random intercept cross-lagged panel model of the associations between collective action for a disadvantaged outgroup and its antecedents.

Note. CA = Collective action. Due to limited space, contemporaneous residual covariances at T2 are not presented.

To test whether our data reflected a stationary process (i.e., where intra-individual change is stable across time), we compared the non-stationary (i.e., unconstrained) model with the stationary solution, which constrained the within-person autoregressive and cross-lagged effects registered between T1 and T2 to be equivalent to the corresponding effects observed between T2 and T3. A better fit of the non-stationary model would suggest that the within-person processes changed over time (Cole & Maxwell, 2003; Hamaker et al., 2015). The results of a better-fitting (or more parsimonious) model were then interpreted. In addition, we estimated 20 indirect effects of T1 variables on T3 variables (for a similar analysis, see Waddell et al., 2021).

All analyses were performed in Mplus 8.0 (Muthén & Muthén, 1998–2017) and employed the ML estimator. The model fit was judged based on conventional criteria (Hu & Bentler, 1999). Model comparisons were performed using the likelihood ratio (LR) test. To account for the violation of multivariate normality assumption [9], the 95% CIs for all estimates were obtained with bias-corrected bootstrapping (10,000 re-samples).[10] There were no missing data.

Random-intercept cross-lagged panel model[11]

Stationarity test

Table 1 presents descriptive statistics, reliabilities, and intercorrelations for the variables of interest. We began our analyses by testing for stationarity. As the non-stationary RI-CLPM did not fit data better than its more parsimonious, stationary counterpart (see Table 2), we interpreted the results of the latter.

Between-person level

[9] Mardia skewness = 3272.09, $p < 0.001$; Mardia kurtosis = 43.04, $p < .001$.
[10] For the results obtained with the MLR estimator, see the Online Supplement.
[11] For the traditional CLPM results, see the Online Supplement.

Among the between-person relationships (see Table 3; Figure 2), only the association between personal acquaintance and moral convictions random intercepts did not reach significance. In line with previous cross-sectional research (e.g., Kende et al., 2017), the remaining associations between the trait factors were positive and significant.

Table 1

Reliabilities, descriptive statistics (means, standard deviations), and intercorrelations for key variables.

	Reliability	***M* (*SD*)**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	**14**
1. CA intentions T1	.84	3.43 (1.44)	–													
2. Personal intergroup contact T1	.87	2.19 (1.10)	**.23**	–												
3. Cross-group friendship T1	.84	1.50 (0.86)	**.32**	**.60**	–											
4. Politicized identity T1	.90	3.98 (1.01)	**.56**	.08	**.15**	–										
5. Moral convictions T1	.87	3.99 (0.89)	**.49**	.04	**.12**	**.72**	–									
6. CA intentions T2	.85	3.21 (1.42)	**.80**	**.19**	**.34**	**.51**	**.46**	–								
7. Personal intergroup contact T2	.86	2.15 (1.08)	**.21**	**.82**	**.53**	**.09**	.06	**.18**	–							
8. Cross-group friendship T2	.85	1.48 (0.84)	**.33**	**.54**	**.75**	**.15**	**.13**	**.35**	**.61**	–						
9. Politicized identity T2	.89	3.87 (1.02)	**.57**	.07	**.18**	**.76**	**.62**	**.58**	**.09**	**.18**	–					
10. Moral convictions T2	.89	3.89 (0.96)	**.49**	.04	**.16**	**.63**	**.66**	**.52**	.08	**.16**	**.73**	–				
11. CA intentions T3	.86	3.06 (1.41)	**.79**	**.15**	**.28**	**.49**	**.42**	**.81**	**.14**	**.33**	**.53**	**.48**	–			
12. Personal intergroup contact T3	.87	2.13 (1.06)	**.22**	**.77**	**.51**	**.11**	.04	**.20**	**.85**	**.57**	**.11**	**.08**	**.18**	–		
13. Cross-group friendship T3	.88	1.47 (0.82)	**.33**	**.47**	**.70**	**.18**	**.14**	**.36**	**.53**	**.78**	**.19**	**.18**	**.33**	**.59**	–	
14. Politicized identity T3	.90	3.85 (1.02)	**.56**	**.10**	**.18**	**.74**	**.59**	**.58**	**.09**	**.19**	**.79**	**.64**	**.57**	**.13**	**.20**	–
15. Moral convictions T3	.89	3.83 (0.98)	**.50**	.06	**.14**	**.63**	**.69**	**.51**	.06	**.17**	**.67**	**.69**	**.51**	**.09**	**.18**	**.77**

Note. $N = 620$. CA = collective action. Significant ($p < .05$) correlation coefficients presented in bold. For two-item measures, Spearman-Brown coefficients are reported to assess reliability.

Table 2

Fit statistics for the non-stationary and stationary RI-CLPM.

	χ^2	*df*	*p*	AIC	BIC	RMSEA [90% CI]	CFI	SRMR	$\Delta\chi^2$	Δdf	Δp
Non-stationary RI-CLPM	20.19	10	.028	19324.93	19878.65	.041 [.013, .066]	.999	.016			
Stationary RI-CLPM	33.63	35	.534	19288.37	19731.34	.000 [.000, .027]	1.000	.021	13.44	25	.971

Note. AIC = Akaike information criterion; BIC = Bayesian information criterion; RMSEA = root mean square error of approximation; CI = confidence interval; CFI = comparative fit index; SRMR = standardized root mean residual.

*** $p < .001$. ** $p < .01$. * $p < .05$.

Table 3

Between-person associations for collective action intentions, personal intergroup contact, cross-group friendship, politicized identity, and moral convictions (stationary model).

Variable 1	**Variable 2**	***B***	***SE***	***p***	**95% *CI***
Collective action intentions	Personal intergroup contact	0.27	0.07	< .001	[0.15, 0.40]
Collective action intentions	Cross-group friendship	0.34	0.06	< .001	[0.24, 0.46]
Collective action intentions	Politicized identity	0.73	0.07	< .001	[0.60, 0.85]
Collective action intentions	Moral convictions	0.59	0.06	< .001	[0.48, 0.70]
Personal intergroup contact	Cross-group friendship	0.40	0.08	< .001	[0.25, 0.52]
Personal intergroup contact	Politicized identity	0.11	0.05	.022	[0.02, 0.20]
Personal intergroup contact	Moral convictions	0.03	0.04	.552	[-0.06, 0.11]
Cross-group friendship	Politicized identity	0.13	0.04	< .001	[0.06, 0.20]
Cross-group friendship	Moral convictions	0.08	0.03	.012	[0.02, 0.15]
Politicized identity	Moral convictions	0.56	0.05	< .001	[0.46, 0.65]

Note. 95% CIs obtained with bootstrapping (10,000 re-samples).

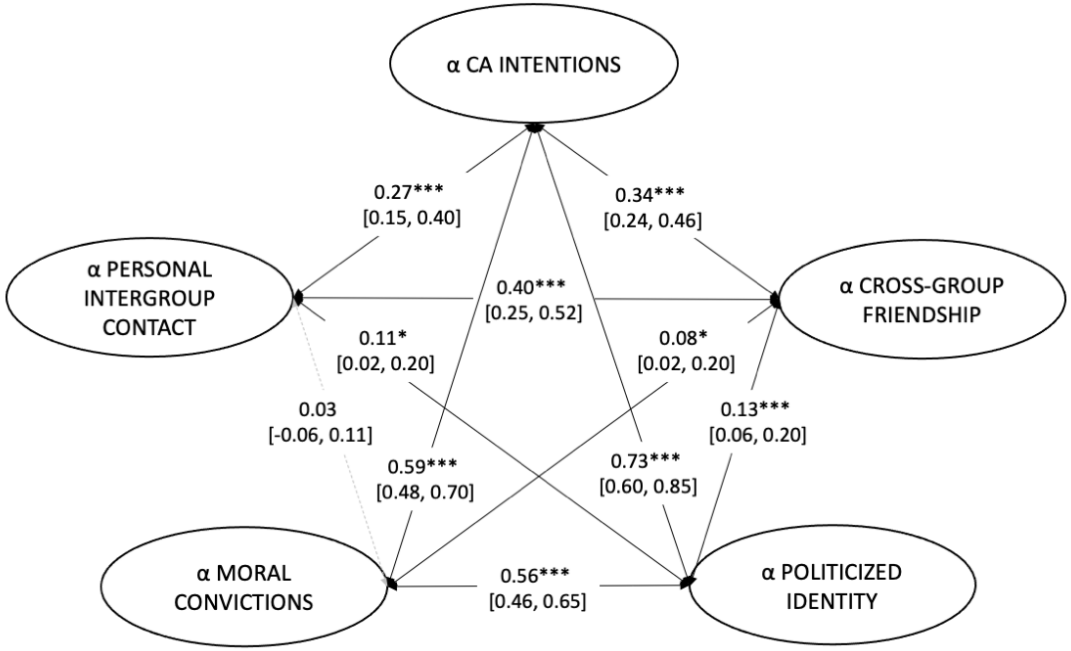

Figure 2. Between-person associations for collective action intentions, personal intergroup contact, cross-group friendship, politicized identity, and moral convictions (stationary random intercept cross-lagged panel model).

Note. CA = collective action. Coefficients are unstandardized. Bootstrap-based (10,000 re-samples) 95% confidence intervals presented in brackets. Grey dashed lines represent nonsignificant effects.

* $p < .05$. ** $p < .01$. *** $p < .001$.

Within-person level

Table 4 and Figure 3 present the within-person components of our model. As evidenced by mostly nonsignificant autoregressive paths, only personal intergroup contact showed a carry-over effect (i.e., the between measurement persistence of a deviation from the individual mean; see Osborne et al., 2021). For the cross-lagged effects, we found that positive deviations from the trait-level of moral convictions were associated with deviations from the mean level of collective action in the following measurement. In other words, when participants' moralization of engagement for the disadvantaged outgroup was greater than usual, they experienced subsequent increase in collective action intentions. In a similar vein, positive deviations from participants' average level of cross-group friendship predicted positive deviations from the trait level of collective action in the next wave. Thus, advantaged group members who reported greater cross-group friendship than usual, were more willing than expected to take collective action for the disadvantaged outgroup in the next measurement. By contrast, deviations from the trait level of collective action did not depend on the preceding deviations from participants' mean levels of politicized identity and personal intergroup contact. At the same time, positive deviations from participants' mean level of collective action were associated with subsequent positive deviations in politicized identity. This suggests that engagement for the disadvantaged outgroup strengthens identification with pro-outgroup social movements.

Regarding the associations between politicized identity, moral convictions, and intergroup contact, we found a mutual positive relationship between moral convictions and cross-group friendship. Moreover, temporal increases in moral convictions were associated with the subsequent positive deviations from the trait level of personal intergroup contact.

Table 4

Within-person autoregressive and cross-lagged effects (stationary model).

Predictor	**Outcome**	***B***	***SE***	***p***	**95% *CI***
Collective action intentions $_{t}$	Collective action intentions $_{t+1}$	0.09	0.12	.484	[-0.14, 0.33]
Personal intergroup contact $_{t}$	Collective action intentions $_{t+1}$	-0.16	0.10	.115	[-0.34, 0.04]
Cross-group friendship $_{t}$	Collective action intentions $_{t+1}$	0.32	0.10	.001	[0.12, 0.52]
Politicized identity $_{t}$	Collective action intentions $_{t+1}$	0.04	0.09	.652	[-0.13, 0.21]
Moral convictions $_{t}$	Collective action intentions $_{t+1}$	0.15	0.08	.048	[0.01, 0.31]
Collective action intentions $_{t}$	Personal intergroup contact $_{t+1}$	-0.01	0.06	.927	[-0.12, 0.11]
Personal intergroup contact $_{t}$	Personal intergroup contact $_{t+1}$	0.36	0.11	.001	[0.16, 0.58]
Cross-group friendship $_{t}$	Personal intergroup contact $_{t+1}$	0.13	0.09	.154	[-0.06, 0.29]
Politicized identity $_{t}$	Personal intergroup contact $_{t+1}$	-0.06	0.07	.355	[-0.19, 0.07]
Moral convictions $_{t}$	Personal intergroup contact $_{t+1}$	0.13	0.06	.020	[0.03, 0.25]
Collective action intentions $_{t}$	Cross-group friendship $_{t+1}$	0.07	0.05	.110	[-0.02, 0.17]
Personal intergroup contact $_{t}$	Cross-group friendship $_{t+1}$	0.16	0.08	.041	[0.01, 0.30]
Cross-group friendship $_{t}$	Cross-group friendship $_{t+1}$	0.18	0.12	.136	[-0.05, 0.42]

Politicized identity $_{t}$	Cross-group friendship $_{t+1}$	-0.04	0.06	.516	[-0.15, 0.07]
Moral convictions $_{t}$	Cross-group friendship $_{t+1}$	0.08	0.04	.059	[0.002, 0.16]
Collective action intentions $_{t}$	Politicized identity $_{t+1}$	0.12	0.06	.041	[0.004, 0.24]
Personal intergroup contact $_{t}$	Politicized identity $_{t+1}$	-0.13	0.08	.086	[-0.29, 0.02]
Cross-group friendship $_{t}$	Politicized identity $_{t+1}$	0.13	0.09	.124	[-0.04, 0.31]
Politicized identity $_{t}$	Politicized identity $_{t+1}$	0.19	0.10	.073	[-0.003, 0.41]
Moral convictions $_{t}$	Politicized identity $_{t+1}$	0.07	0.07	.331	[-0.06, 0.20]
Collective action intentions $_{t}$	Moral convictions $_{t+1}$	0.08	0.06	.173	[-0.04, 0.19]
Personal intergroup contact $_{t}$	Moral convictions $_{t+1}$	-0.03	0.08	.674	[-0.18, 0.12]
Cross-group friendship $_{t}$	Moral convictions $_{t+1}$	0.19	0.09	.030	[0.03, 0.36]
Politicized identity $_{t}$	Moral convictions $_{t+1}$	0.19	0.11	.068	[-0.01, 0.40]
Moral convictions $_{t}$	Moral convictions $_{t+1}$	-0.03	0.10	.778	[-0.23, 0.16]

Note. Unstandardized effects reported. 95% CIs obtained with bootstrapping (10,000 re-samples).

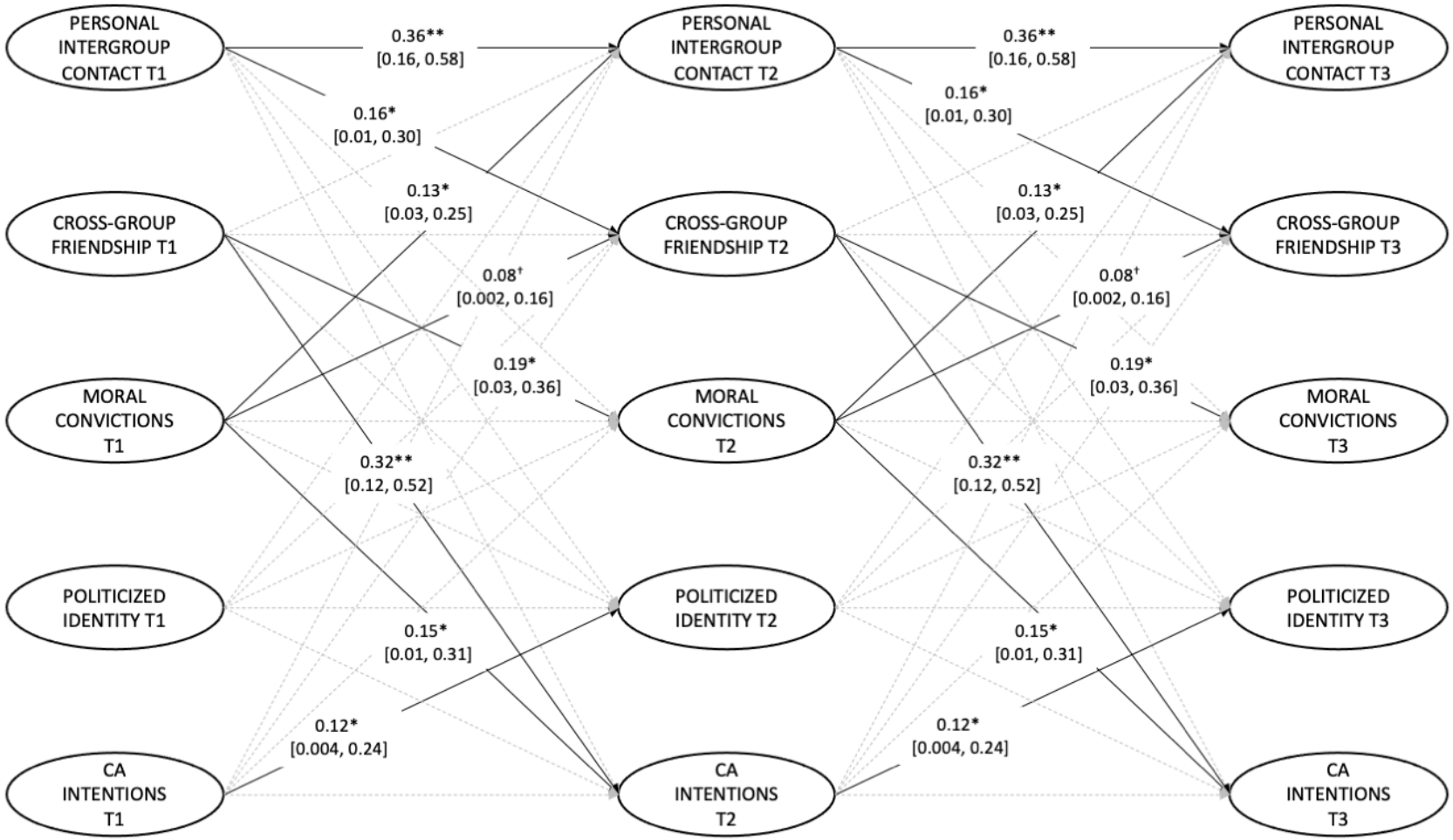
PERSONAL INTERGROUP CONTACT T1
CROSS-GROUP FRIENDSHIP T1
MORAL CONVICTIONS T1
POLITICIZED IDENTITY T1
CA INTENTIONS T1
PERSONAL INTERGROUP CONTACT T2
CROSS-GROUP FRIENDSHIP T2
MORAL CONVICTIONS T2
POLITICIZED IDENTITY T2
CA INTENTIONS T2
PERSONAL INTERGROUP CONTACT T3
CROSS-GROUP FRIENDSHIP T3
MORAL CONVICTIONS T3
POLITICIZED IDENTITY T3
CA INTENTIONS T3
0.36** [0.16, 0.58]
0.16* [0.01, 0.30]
0.13* [0.03, 0.25]
0.08† [0.002, 0.16]
0.19* [0.03, 0.36]
0.32** [0.12, 0.52]
0.15* [0.01, 0.31]
0.12* [0.004, 0.24]
0.36** [0.16, 0.58]
0.16* [0.01, 0.30]
0.13* [0.03, 0.25]
0.08† [0.002, 0.16]
0.19* [0.03, 0.36]
0.32** [0.12, 0.52]
0.15* [0.01, 0.31]
0.12* [0.004, 0.24]

Figure 3. Within-person longitudinal associations for collective action intentions, personal intergroup contact, cross-group friendship, politicized identity, and moral convictions (stationary random intercept cross-lagged panel model).

Note. CA = collective action. Coefficients are unstandardized. Bootstrap-based (10,000 re-samples) 95% confidence intervals presented in brackets. Grey dashed lines represent nonsignificant effects. Due to space constraints, contemporaneous covariances and nonsignificant autoregressive or cross-lagged effects are not presented.

* $p < .05$. ** $p < .01$. *** $p < .001$.

In terms of within-person indirect effects (Table 5), we found a positive indirect effect of moral convictions at T1 on collective action intentions at T3 via cross-group friendship at T2. Likewise, there was a positive indirect effect of cross-group friendship at T1 on collective action at T3 via moral convictions at T2. By exerting a positive effect on collective action at T2, moral convictions at T1 had a positive indirect effect on politicized identity at T3. Finally, there was a positive indirect effect of personal intergroup contact at T1 on collective action at T3 by cross-group friendship at T2.

Table 5

Within-person indirect effects involving collective action intentions (stationary model).

Focal predictor	Mediator	Outcome	*IE*	*SE*	*p*	95% *CI*
Personal intergroup contact $_{t1}$	Cross-group friendship $_{t2}$	Collective action intentions $_{t3}$	0.05	0.03	.072	[0.01, 0.12]
Personal intergroup contact $_{t1}$	Politicized identity $_{t2}$	Collective action intentions $_{t3}$	-0.01	0.01	.683	[-0.05, 0.01]
Personal intergroup contact $_{t1}$	Moral convictions $_{t2}$	Collective action intentions $_{t3}$	-0.01	0.01	.677	[-0.04, 0.01]
Cross-group friendship $_{t1}$	Personal intergroup contact $_{t2}$	Collective action intentions $_{t3}$	-0.02	0.02	.291	[-0.08, 0.004]
Cross-group friendship $_{t1}$	Politicized identity $_{t2}$	Collective action intentions $_{t3}$	0.01	0.01	.688	[-0.01, 0.05]
Cross-group friendship $_{t1}$	Moral convictions $_{t2}$	Collective action intentions $_{t3}$	0.03	0.02	.129	[0.003, 0.09]
Politicized identity $_{t1}$	Personal intergroup contact $_{t2}$	Collective action intentions $_{t3}$	0.01	0.01	.433	[-0.004, 0.06]
Politicized identity $_{t1}$	Cross-group friendship $_{t2}$	Collective action intentions $_{t3}$	-0.01	0.02	.515	[-0.06, 0.02]
Politicized identity $_{t1}$	Moral convictions $_{t2}$	Collective action intentions $_{t3}$	0.03	0.02	.149	[0.001, 0.09]
Moral convictions $_{t1}$	Personal intergroup contact $_{t2}$	Collective action intentions $_{t3}$	-0.02	0.02	.203	[-0.07, 0.001]
Moral convictions $_{t1}$	Cross-group friendship $_{t2}$	Collective action intentions $_{t3}$	0.02	0.02	.111	[0.003, 0.07]
Moral convictions $_{t1}$	Politicized identity $_{t2}$	Collective action intentions $_{t3}$	0.003	0.01	.730	[-0.01, 0.03]
Personal intergroup contact $_{t1}$	Collective action intentions $_{t2}$	Personal intergroup contact $_{t3}$	0.001	0.01	.928	[-0.01, 0.03]

Personal intergroup contact $_{t1}$	Collective action intentions $_{t2}$	Cross-group friendship $_{t3}$	-0.01	0.01	.240	[-0.04, 0.001]
Personal intergroup contact $_{t1}$	Collective action intentions $_{t2}$	Politicized identity $_{t3}$	-0.02	0.01	.172	[-0.06, 0.00]
Personal intergroup contact $_{t1}$	Collective action intentions $_{t2}$	Moral convictions $_{t3}$	-0.01	0.01	.257	[-0.05, 0.001]
Cross-group friendship $_{t1}$	Collective action intentions $_{t2}$	Personal intergroup contact $_{t3}$	-0.002	0.02	.927	[-0.04, 0.03]
Cross-group friendship $_{t1}$	Collective action intentions $_{t2}$	Cross-group friendship $_{t3}$	0.02	0.02	.242	[-0.001, 0.08]
Cross-group friendship $_{t1}$	Collective action intentions $_{t2}$	Politicized identity $_{t3}$	0.04	0.02	.074	[0.004, 0.09]
Cross-group friendship $_{t1}$	Collective action intentions $_{t2}$	Moral convictions $_{t3}$	0.03	0.02	.185	[-0.01, 0.08]
Politicized identity $_{t1}$	Collective action intentions $_{t2}$	Personal intergroup contact $_{t3}$	0.00	0.01	.970	[-0.01, 0.01]
Politicized identity $_{t1}$	Collective action intentions $_{t2}$	Cross-group friendship $_{t3}$	0.003	0.01	.706	[-0.01, 0.03]
Politicized identity $_{t1}$	Collective action intentions $_{t2}$	Politicized identity $_{t3}$	0.01	0.01	.689	[-0.01, 0.04]
Politicized identity $_{t1}$	Collective action intentions $_{t2}$	Moral convictions $_{t3}$	0.003	0.01	.700	[-0.01, 0.03]
Moral convictions $_{t1}$	Collective action intentions $_{t2}$	Personal intergroup contact $_{t3}$	-0.001	0.01	.923	[-0.02, 0.01]
Moral convictions $_{t1}$	Collective action intentions $_{t2}$	Cross-group friendship $_{t3}$	0.01	0.01	.172	[0.00, 0.04]
Moral convictions $_{t1}$	Collective action intentions $_{t2}$	Politicized identity $_{t3}$	0.02	0.01	.145	[0.01, 0.06]
Moral convictions $_{t1}$	Collective action intentions $_{t2}$	Moral convictions $_{t3}$	0.01	0.01	.297	[-0.001, 0.05]
Collective action intentions $_{t1}$	Personal intergroup contact $_{t2}$	Cross-group friendship $_{t3}$	-0.001	0.01	.938	[-0.02, 0.02]

Collective action intentions $_{t1}$	Personal intergroup contact $_{t2}$	Politicized identity $_{t3}$	0.001	0.01	.932	[-0.01, 0.02]
Collective action intentions $_{t1}$	Personal intergroup contact $_{t2}$	Moral convictions $_{t3}$	0.00	0.01	.973	[-0.01, 0.01]
Collective action intentions $_{t1}$	Cross-group friendship $_{t2}$	Personal intergroup contact $_{t3}$	0.01	0.01	.348	[-0.003, 0.04]
Collective action intentions $_{t1}$	Cross-group friendship $_{t2}$	Politicized identity $_{t3}$	0.01	0.01	.316	[-0.002, 0.04]
Collective action intentions $_{t1}$	Cross-group friendship $_{t2}$	Moral convictions $_{t3}$	0.01	0.01	.235	[0.00, 0.05]
Collective action intentions $_{t1}$	Politicized identity $_{t2}$	Personal intergroup contact $_{t3}$	-0.01	0.01	.420	[-0.04, 0.004]
Collective action intentions $_{t1}$	Politicized identity $_{t2}$	Cross-group friendship $_{t3}$	-0.004	0.01	.535	[-0.03, 0.01]
Collective action intentions $_{t1}$	Politicized identity $_{t2}$	Moral convictions $_{t3}$	0.02	0.02	.167	[0.001, 0.08]
Collective action intentions $_{t1}$	Moral convictions $_{t2}$	Personal intergroup contact $_{t3}$	0.01	0.01	.259	[-0.002, 0.04]
Collective action intentions $_{t1}$	Moral convictions $_{t2}$	Cross-group friendship $_{t3}$	0.01	0.01	.262	[-0.001, 0.03]
Collective action intentions $_{t1}$	Moral convictions $_{t2}$	Politicized identity $_{t3}$	0.01	0.01	.439	[-0.002, 0.03]
Collective action intentions $_{t1}$	Personal intergroup contact $_{t2}$	Collective action intentions $_{t3}$	0.001	0.01	.928	[-0.01, 0.03]
Collective action intentions $_{t1}$	Cross-group friendship $_{t2}$	Collective action intentions $_{t3}$	0.02	0.02	.242	[-0.001, 0.08]
Collective action intentions $_{t1}$	Politicized identity $_{t2}$	Collective action intentions $_{t3}$	0.01	0.01	.689	[-0.01, 0.04]
Collective action intentions $_{t1}$	Moral convictions $_{t2}$	Collective action intentions $_{t3}$	0.01	0.01	.297	[-0.001, 0.05]

Note. Unstandardized effects reported. 95% CIs obtained with bootstrapping (10,000 re-samples). For the full list of indirect effects, see the Online Supplement.

Discussion

This research sought to investigate the within-person longitudinal relationships between collective action for a disadvantaged outgroup and its three widely-postulated antecedents – politicized identity, moral convictions, and intergroup contact. Adopting RI-CLPM as a statistical framework, we analyzed data collected at the time of the refugee crisis spurred by Russia's invasion of Ukraine. We found that advantaged group members (i.e., Poles) who reported stronger cross-group friendship and moral convictions than usual exhibited greater positive deviations from their typical level of willingness to engage for a disadvantaged outgroup (i.e., Ukrainians) two weeks later. By contrast, temporal changes in personal intergroup contact and politicized identity did not translate directly to subsequent deviations in participants' collective action intentions. At the same time, however, positive deviations in collective action were associated with positive divergences from the average level of politicized identity (but not with changes in the remaining variables) in the following measurement. For the theory-based antecedents of collective action, we found a reciprocal positive association between cross-group friendship and moral convictions, and a positive prospective effect of moral convictions on personal intergroup contact. These significant cross-lagged effects served as building blocks for several indirect effects.

Our study extends previous research in several ways. First, it provides a longitudinal test for three hypothesized predictors of collective action for the disadvantaged outgroup – moral convictions, intergroup contact, and politicized identity. In accordance with past theorizing (Radke et al., 2020; van Zomeren et al., 2018) and research (van Zomeren, Postmes, & Spears, 2012), moral convictions had a positive prospective effect on engagement intentions. In other words, a temporal increase in viewing engagement for the disadvantaged outgroup as a matter of morality entailed the subsequent positive deviation from participants' mean level of collective action intentions.

Likewise, a within-person increase in cross-group friendship predicted a consecutive increase in willingness to engage for the disadvantaged outgroup (see Hässler et al., 2020; Reimer et al., 2017). This, however, was not the case for personal intergroup contact. These findings corroborate the hypothesis that intergroup contact is particularly likely to motivate engagement for the disadvantaged outgroup when it reaches a critical quality threshold (see MacInnis & Hodson, 2019). The within-person indirect effect of cross-group friendship at T1 on collective action at T3 via moral convictions at T2 points to one of mechanisms behind the positive effect of cross-friendship on engagement for the disadvantaged group. Specifically, having friends from the disadvantaged outgroup seems to prompt advantaged group members to perceive the fight for equality as a moral issue, which translates into higher engagement intentions. At the same time, although we did not find a direct longitudinal effect of personal intergroup contact on collective action, the within-person indirect effect of personal intergroup contact at T1 on collective action at T3 via cross-group friendship at T2 was significant. This suggests that superficial intergroup contact motivated collective action for the disadvantaged outgroup by facilitating cross-group friendship.

By showing a positive longitudinal relationship between cross-group friendship and collective action for a disadvantaged outgroup, the current results diverge from the findings reported by Sengupta et al. (2022). These authors demonstrated that European New Zealanders' (the advantaged group) contact with Māori (the disadvantaged group) had no within-person longitudinal effect on solidarity with the latter. Our findings may depart from those results because of three reasons. First, Ukrainian refugees and New Zealanders of Māori descent experience different types of disadvantage. The inferior status of Ukrainian refugees is incidental – it originates from an external cause, which, once removed, would leave this group no longer disadvantaged. By contrast, Māori face structural disadvantage resulting from years of systemic discrimination. As shown by van Zomeren et al.'s (2008) oft cited meta-

analysis (but not by its recent replication; see Agostini & van Zomeren, 2021), structural disadvantage works as a contextual constraint upon some antecedents of collective action – the explanatory power of injustice and efficacy is significantly smaller for structural than incidental inequalities. As such, the inconsistent results of the two studies may reflect a general difference between collective action taken to tackle structural vs. incidental disadvantage. Second, the two-week interval employed in the current research was much shorter than the one-year lag used by Sengupta et al. (2022). Perhaps, long intervals between subsequent measurements are not optimal to detect longitudinal associations between intergroup contact and pro-outgroup engagement.[12] At the same time, two-week intervals may be insufficient to observe theory-based processes that the current study failed to find (i.e., the over-time effect of politicized identity on collective action). We believe that both intergroup contact and collective action literatures need a study (e.g., a meta-analysis) that systematically checks how the longitudinal effects of interest (e.g., the effect of intergroup contact on pro-outgroup collective action) depend on the length of the interval. Lastly, it is also possible that our findings depart from those of Sengupta et al. (2022) because they capture a different phase in the lifespan of intergroup relationships. In their asymptotic model of intergroup contact, MacInnis and Page-Gould (2015; see also Page-Gould et al., 2022) proposed that the improvement in outgroup-directed attitudes is the strongest in early intergroup interactions (the atomic units of intergroup contact) and diminishes over time. If a similar dynamic applies to pro-outgroup engagement, intergroup contact should fuel collective action to the greater extent at the initial rather than late stages of intergroup relationships. As the percentage of Poles knowing at least one Ukrainian increased after Russia's 2014 invasion of Ukraine (Strzelecki et al., 2022), and further in 2022,[13] we believe that intergroup relationships

[12] Sengupta et al. (2022) allow the possibility that a one-year lag is too long (or too short) and call for more theoretical work on the temporal aspect of intergroup contact effects.
[13] Survey results demonstrate an increase from 20.3% in 2009 (Bilewicz, Bukowski, Cichocka, Wójcik, & Winiewski, 2009) through 33.3% in 2017 (Stefaniak & Winiewski, 2018) to 62.7% in the present research.

registered in the present research were more recent than those examined by Sengupta et al. (2022).

The current results also add to the literature on social identity and collective action. Unexpectedly, temporal within-person increases in politicized identity did not translate into subsequent positive deviations from the trait level of collective action. As the positive effect of social (e.g., politicized) identity on engagement is a common denominator of several influential models of collective action, the present results were not entirely consistent with past theorizing. Most importantly, the nonsignificant prospective effect of politicized identity is at odds with the predictions based on the social identity model of collective action (SIMCA; van Zomeren et al., 2008) and the encapsulated model of social identification and collective action (EMSICA; Thomas et al., 2009, 2012). Moreover, contrary to the extended SIMCA (van Zomeren et al., 2018), deviations from the trait level of politicized identity did not mediate the association between the temporal deviations in moral convictions and engagement intentions. Finally, inconsistent with the dual chamber model of collective action (Agostini & van Zomeren, 2021), social identity *and* morality did not work in parallel to motivate engagement for the disadvantaged outgroup. Instead of predicting the subsequent deviations in engagement, temporal increases in politicized identity were predicted by preceding deviations in collective action. Thus, consistent with prior findings (see Becker et al., 2011; Thomas et al., 2020), when participants' engagement for the disadvantaged outgroup was greater than usual, their identification with the social movement helping the outgroup exceeded its trait level in the following measurement. These results invite two important conclusions. First, in some contexts politicized identity may serve as a consequence rather than the antecedent of collective action. Perhaps this is the case when engagement for the disadvantaged outgroup does not originate from group-level motives (Radke et al., 2020). Second, given the positive association between random intercepts for politicized identity and

collective action in the RI-CLPM, the current results suggest that the established models of collective action may be a more accurate description of between- than within-person processes.

Furthermore, our findings shed light on the associations between the antecedents of collective action for a disadvantaged outgroup. For instance, we found a within-person reciprocal association between moral convictions and cross-group friendship – cross-group friendship had a positive prospective effect on moral convictions and was simultaneously predicted by the preceding deviations in moral convictions. Importantly, moral convictions did not show a reciprocal relationship with personal intergroup contact – while the within-person prospective effect of moral convictions on personal intergroup contact was positive, the opposite effect from personal intergroup contact on moral convictions did not reach significance. As such, in contrast to cross-group friendship, superficial intergroup contact did not entail the moralization of collective action for the disadvantaged outgroup (see MacInnis & Hodson, 2019). Furthermore, we did not find a positive within-person effect of politicized identity on moral convictions – contrary to past theorizing (Radke et al., 2020), politicized identity did not prompt advantaged group members to perceive collective action for a disadvantaged outgroup as a matter of morality.

Next, our study responds to calls for the greater use of longitudinal data in collective action research (e.g., Hässler et al., 2022; Thomas et al., 2021). By employing a general community sample and short, two-week intervals, it complements previous studies that used data collected among the committed supporters of a given cause (e.g., Thomas et al., 2021) or utilized longer (e.g., Górska et al., 2022; Sengupta et al., 2022) time lags between the subsequent measurements.

Finally, the current research adds to collective action literature by using the RI-CLPM (Hamaker et al., 2015) as an analytical framework. While collective action may be

conceptualized both as a dynamic phenomenon and a stable individual difference, this duality has rarely been recognized in empirical research so far (for the exceptions, see Sengupta et al., 2022; Thomas et al., 2021). Instead, past studies investigated temporal changes in engagement using the traditional CLPM (e.g., Thomas et al., 2020; Górska et al., 2020, 2022), which does not separate within- and between-person variance (Hamaker et al., 2015), risking incorrect conclusions about intraindividual processes. By disentangling the within-person processes from stable between-person differences, the RI-CLPM allowed us to capture the dual (i.e., both static and dynamic) character of collective action.

Importantly, the autoregressive and cross-lagged effects in RI-CLPM have a different meaning than those estimated in the traditional CLPM (e.g., Orth et al., 2021). Therefore, divergences between CLPM and RI-CLMP results suggest that different mechanisms may be at work when the temporal changes in collective action are viewed as shifts in the between-person rank-order (CLPM) or within-person fluctuations (RI-CLPM). Novel theories are needed to account for these between-person and within-person processes.

In terms of practical implications, current findings suggest that, besides providing refugees with material assistance, decision-makers should not forget about creating opportunities for positive intergroup contact. As shown by our results, cross-group friendship motivates host society members to help refugees, either directly or by changing their moral convictions. Moreover, positive intergroup contact could mitigate the potential increase of anti-refugee attitudes that may occur amid fears of economic recession (see Glick, 2005).

The present research has some limitations. Although RI-CLPM is the model of choice when one's goal is to estimate reciprocal relations between the variables of interest (Usami et al., 2019), separating between- and within-person variance permits causal inference only after certain assumptions are made (Rohrer & Murayama, 2021). Specifically, one needs to assume that a) all time-varying confounders are included in the model, b) people do not vary in their

change over time (i.e., do not have heterogeneous slopes, and c) there are no contemporaneous causal effects (e.g., the effect of T1 cross-group friendship on T1 politicized identity).

Next, we did not find some well-established longitudinal effects (e.g., the within-person effect of politicized identity on collective action intentions). Perhaps, the null findings may be attributed to the suboptimal measurement of politicized identity and moral convictions. It is also possible that some processes (e.g., developing politicized identity based on one's moral convictions) require more than two weeks to fully unfold or, alternatively, had taken place before data collection was even started. A study using more comprehensive measurement and heterogeneous time intervals would allow to decide which of these explanations is correct.

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