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I'll Just Watch: Do The Pro-Social Effects of Coordination Really Generalize to Non-Actors?

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ABSTRACT

Moving in time together has been shown to cultivate pro-social effects in co-actors, such as cooperation and helping. But less is known about who these effects apply to – whether they are restricted only to co-actors, or whether they generalize to those not involved in the coordination. One difference between past work finding generalized vs. restricted effects is whether these ‘outsiders’ were present for the coordination or not. The present study explores whether the pro-social effects of coordination are seen towards observers as well as co-actors, and whether the absence or presence of observers during the coordination is a determining factor. Results show that greater cooperation following coordination is only seen amongst co-actors, regardless of whether the observers were present during the task or not. Findings are discussed in the context of the literature and alternative explanations for research showing generalized effects are suggested.

Keywords: Coordination, Entrainment, Synchrony, Cooperation, Pro-sociality, Group Processes

I'll Just Watch: Do The Pro-Social Effects of Coordination Really Generalize to Non-Actors?

We all rhythmically coordinate our movements with each other, both consciously and unconsciously. We sing, dance, play music and walk in coordinated ways (McNeil 1997). Our postures, gazes, and speech patterns can all spontaneously synchronize with those around us (Allsop, Vaitkus, Marie & Miles, 2017). Coordinated Rhythmic Movement (CRM) – as defined by Cross, Wilson and Golonka (2016) describes a special class of coordination that involves aligning one's physical movements with another person's, in time to a common rhythm (such as two concert-goers dancing in time to music). The experiment presented here focuses exclusively on in-phase synchrony, a special class of coordination with the highest degree of alignment amongst co-actors. In-phase synchrony is where two or more people coordinate their movements so they are moving in the same way and at the same time; for example two people walking in step, where each individual puts their left leg forward at the same time. Previous work shows that in-phase coordination can foster pro-social effects, such as greater rapport (Hove & Risen, 2009), cooperation (Wiltermuth & Heath, 2009), and helping (Reddish, Bulbulia & Fischer, 2014) amongst those who take part.

However, there is mixed evidence on whether these pro-social effects of coordination are generalized. They may be restricted to those actually involved in the coordination, or they may generalize to those not involved. There are two directions in which generalization can be investigated. The first is generalization from a participant involved in a coordination episode towards a person who was not involved. We call this *inside-out* generalization. The other form of generalization is from a person who observes the coordination episode towards a person who engages in it. We call this *outside-in* generalization.

Reddish et al. (2014) support the existence of inside-out generalization, but with limited or no outside-in generalization. In study 2 of Reddish et al., participants were put into groups of six and then split in half, with half of them performing a CRM task and the other half completing a jigsaw puzzle. Participants who had performed the CRM task shared more resources in an economic game with the group that had completed the puzzle task. The group

that had completed the puzzle task shared fewer resources with the group that had performed the CRM task. In study 1, participants in groups of three or four either, performed or observed a CRM task. They were then asked by a confederate if they would be willing to give up their time to participate in another study for that confederate. Participants who had engaged in the CRM task donated more time than participants who had only observed. Crucially, this was the case regardless of whether the confederate was engaged in the movement task or only introduced to the experimental cohort after the CRM task. Study 1 suggests that outside-in generalization is either attenuated with respect to inside-out generalization, or is altogether absent, while both studies suggest inside-out generalization occurs.

However, other work suggests that the pro-social effects of CRM are restricted to those involved in the coordination. For example, Kokal, Engel, Kirschner and Keysers (2011) had participants 'drum along with'¹ two experimenters, one synchronously and the other asynchronously. Following the drumming task, one of the experimenters dropped some pencils, and the number of pencils the participant picked up was taken as a measure of pro-sociality. They found that participants displayed greater pro-sociality only towards people they believed they had moved in synchrony with, compared with people they believed they had moved asynchronously with. Similarly, Cirell, Einarson and Trainor (2014) found that young infants only displayed greater pro-sociality (measured by a directed helping task) towards other infants they had been bounced in time with, and not towards infants who had just observed the movement task. Finally, Tarr, Launay, Cohen and Dunbar (2015) had participants perform a CRM task in which they moved in time to music in groups of three. Participants only reported greater social bonding towards individuals they had moved in time with, rather than towards people more generally. These studies do not find any evidence of inside-out generalization.

¹ Participants were actually drumming in time with a computerized algorithm, but were unaware of this.

One (perhaps crucial) difference between studies that do and do not find evidence for inside-out generalization is whether non-actors were present during the CRM task or not. In both of the Reddish et al. (2014) experiments that supported inside-out generalization, greater pro-sociality was shown towards people who were not present for the coordination. However, in the Cirelli et al. (2014) and Kokal et al. (2011) studies, which do not support inside-out generalization, non-actors were present for but did not participate in the coordination task. In Tarr, et al. (2015), the non-actors were neither absent nor present but hypothetical people whom participants did not interact with at all during the experiment (their wider cohort of classmates). Being present for the CRM task but not engaging in it may serve as a marker that the observer is somehow separate or different from those coordinating. The same level of differentiation and distinction may not occur if non-actors are absent while the coordination is taking place. Therefore, the primary aim of this study was to examine whether the prosocial effects of CRM are restricted to co-actors, or whether they are more generalized, and whether the presence or absence of the non-actor during the coordination episode is a determining factor.

A secondary aim of this research was to explore the cognitive mechanisms by which CRM might affect cooperation. It is worth noting that a range of neuro-hormonal mechanisms have also been proposed to explain this, such as increases in caudate activity, oxytocin, and endorphin release (Kokal et al., 2011; Lang, Bahna, Shaver, Reddish & Xygalatas, 2017; Tarr, Launay, Cohen & Dunbar, 2014). While assessing the impact of these factors was outside the reach of this work, a clear understanding of the mechanisms involved in CRM's relationship to pro-sociality will certainly involve neuro-hormonal factors as well.

Self-other overlap, often assessed using the Inclusion of Other in Self (IOS) measure (Aron, Aron & Smollan, 1992), refers to the degree of blurring between oneself and another person. Hove (2008) initially suggested that it was involved in the pro-social effects of CRM. However, several studies using the IOS have not found any evidence supporting the mediating role of self-other overlap in CRM's effects on cooperation (Cross et al., 2016; Fessler & Holbrook, 2014; Lang et al., 2017; Reddish et al., 2013; Tarr, et al., 2016). The only studies

finding effects on self-other overlap are those that combine it with measures of group cohesion (i.e. Tarr et al., 2015).

Group cohesion refers to the strength of the bonds between individual group members or towards the group as a whole, and is typically measured via self-report measures of liking, closeness, connectedness and similarity (i.e. Wiltermuth & Heath, 2009). Using such measures, researchers have provided evidence that cohesion may partially mediate some of the social effects of CRM (Reddish, 2012; Valdesolo & Desteno, 2011; Wiltermuth, 2012; Wiltermuth & Heath, 2009). However, other work has failed to replicate these findings. Coordination showed no effect on cohesion despite positive changes in cooperation (Cross et al., 2016; Fessler & Holbrook, 2014, 2016; Lang et al., 2017; Reddish et al., 2013), and while imagined coordination showed an effect on cohesion, this did not translate to greater cooperation (Cross, Atherton, Wilson & Golonka, 2017). It is therefore unclear whether and how group cohesion is involved in mediating the effect of coordination on cooperation.

Recently it has been suggested that CRM might increase pro-sociality by signaling a common group membership amongst those involved (e.g. Good, Choma & Russo, 2017; Tuncgenc & Cohen, 2016 Cross, Turgeon & Atherton 2019), leading to other group effects such as increased cooperation with in-group members. In support of this idea, Good, et al., (2017) found that after coordination people are more likely to view themselves and their co-actors as a common group than as a collection of individuals. Cross et al. (2017) found that this also applies to imagined coordination.

The weight of evidence so far does not suggest that the pro-social effects of coordination are directly mediated by group cohesion or self-other overlap, and some evidence suggests that people's self-construal may play a role (Cross, et al., 2017; Good, et al., 2017). Because of this, the current study took measures of all of these potential cognitive mediators, with the goal of testing a comprehensive list of proposed mediators. We hypothesized that engaging in the coordinated version of the CRM task would promote more cooperation in an economic game (compared with an uncoordinated version of the task), and that this cooperation would be shown towards co-actors but not observers. Measuring these

cognitive mediators also allowed us to assess for any potential role they might play in how coordination affects cooperation.

METHOD

Participants

Ninety-six students at Leeds Beckett University volunteered to participate (60 females, $M_{age}=19.6yr$, $SD_{age}=2.53$). Sample size was determined in the design phase, based on the sample sizes used by Reddish et al. (2014). Power analysis confirmed that this sample was adequately powered (>80%) for both Chi-Square and t-tests using effect size estimates of 0.05 (the smaller of the two effect sizes reported for generalized pro-sociality findings in Reddish et al.). The Leeds Beckett University Psychology Ethics Review Board approved the experiment, and the experiment was carried out in accordance with the relevant guidelines. All participants gave full informed consent.

Design and Procedure

People participated in groups of four, and a session lasted approximately 45 minutes. All participants in an experimental session were from different year and/or subject cohorts, and confirmed that they did not know each other before the study. They were told the study was investigating how people performed tasks while they were being observed. A between subjects design was employed. The primary experimental factor (Movement Type) had two levels: Coordinated (where people performed or observed the coordinated version of the task) and Uncoordinated (where people performed or observed the uncoordinated version of the task). Participants were put into one of three categories: Movers (who perform the task), Present Observers (POs, who are in the same room as the Movers and observe the task), and Absent Observers (AOs, who observe the task via livestream from a separate room). The movement task was set up so that the two co-actors sat side by side, facing a screen that displayed a Point Light Display (PLD) of the joysticks' movement trajectory. The PO sat in the corner of the room so that they could see both the co-actors and the PLD. Directly above the

PO a video camera live streamed the same view to the AO, who sat in an adjacent lab for the movement task (see Figure 1 online for a diagram of the lab set up).

[INSERT FIGURE 1 HERE, online only]

Participants were brought into the lab to meet each other. No names were given and no other interaction was allowed, in order to try and achieve a level of homogeneity across testing sessions. We first checked that participants did not already know each other. Participants were each given an A4-sized color marker (red, blue, yellow or green) and were told to refer to the other participants by the color of their marker throughout the experiment. Participants were then sat around a large square table, as depicted in Figure 1, so as to uniformly maximize the distance between each participant and allow them a degree of privacy when completing measures. Participants were asked to keep responses hidden from the other participants by covering them with a blank piece of paper while recording them. Once measures were filled in the participants were instructed to turn them face down, to ensure responses were kept confidential. Participants first completed the baseline measures of affect, trust, overlap and cohesion, and were told this was a check for any differences in first impressions or mood across groups. After completing these measures, participants took part in or observed the movement task (the PO from inside the room, the AO via livestream from an adjacent room; see figure 1 for an illustration). Immediately following the movement task the AO was brought back to the lab, and all participants sat back in their original places. The Movers recorded their perceived success, difficulty, and enjoyment of the task on 5-point Likert scale. Perceived coordination between the Movers was recorded by the observers, also using a 5-point Likert scale. All participants were then asked to fill in a second copy of the affect, trust, overlap and cohesion measures (in order to check for changes from baseline). They then completed the self-construal measure. Finally, participants took part in an economic game that was used as the dependent measure of cooperation. Participants were then debriefed, and paid in line with their actual economic game performance (see below). All measures can be found online in the supplementary materials.

Tasks and Measures

Movement: In the Coordinated task, participants moved joysticks (Genius MaxFighter F-17 joysticks with force feedback disabled, sampling at 60Hz) in-phase (same direction at the same time) with one another. They were told to move at a frequency of 0.75Hz. Participants viewed a PLD to coordinate their movements. The PLD consisted of a laptop screen positioned in front of them, displaying two white dots on a black background. The dots were 40x40 pixels, and separated by a visual angle of $.14^\circ$, one above the other, positioned in the center of the screen.

In the Uncoordinated task, participants made uncoordinated movements at different frequencies. One participant always moved at 0.6 Hz and the other always moved at 0.9 Hz, (0.15Hz less or more than 0.75Hz respectively). One participant began by moving the joystick vertically and the other in clockwise circles. Participants switched movements every trial (so for trial 1 blue would move in circles and green vertically, then for trial 2 blue would move vertically and green in circles, and so on). These movements are comparable to those required in the Coordinated task, with the exception that they are not coordinated, making it a useful control condition. We had participants perform different movements as well as different speeds to inhibit a pull towards in-phase coordination, as this is known to be a strong attractor state (Kelso, 1995).

Participants first saw two 15-second demonstrations of two dots moving at the desired relative phase and frequency (Coordinated) or the required frequencies (Uncoordinated). Both dots were displayed on a single screen, one above the other. Participants were told to move in the same way and at the same time as their dot for as much of the trial as possible. After each demo participants had 30 seconds of practice time to acquaint themselves with the required movements and pace. Following this, all participants completed six 60-second trials of their required movement. Each trial was preceded by a four-second version of the demonstration pacing them to the required relative phase and frequency of movements. This experiment was run with Matlab using Psychtoolbox (Brainard, 1997; Kleiner et al., 2007; Pelli, 1997) on a MacBook Pro using a custom toolbox (Wilson, Collins & Bingham, 2005a, b).

Social Measures

The social psychological measures consisted of a positive affect measure, a trust measure, a cohesion scale, a self-other overlap scale, a self-construal questionnaire and an economic game.

Positive affect: Positive affect was measured by asking participants how happy they felt in that moment. Participants responded using a 185mm continuum line (anchored by Not At All – Very Much So), in order to make it more likely to detect changes after manipulation. Positive affect was assessed as a precautionary check that any effects found were not just due to changes in mood.

Trust: The trust measure asked participants how much they trusted each participant and the cohort as a whole (asked separately for each participant and the experimental cohort as a whole). Items were responded to on the same continuum scale described above. This was done in order to investigate whether cooperation patterns in the economic game were explained by changes in trust.

Group cohesion scale: The group cohesion items were taken from a similar study (Cross et al., 2017), and measured how similar and how close people felt to each other, how much they liked each other, and how much they wanted to see each other again. The participants answered all the items for each individual and for the experimental cohort as a whole. Items were responded to on the same continuum scale described above.

Self-other overlap: Overlap was measured using an adapted version of the Inclusion of Other in Self measure (IOS). The response format was changed to a 170mm continuum line where one anchor signified no overlap and the other anchor signified a large overlap. This was done in order to make the scale more sensitive to repeated measures. Participants rated the amount of overlap they felt with each of the other participants on a separate line. Finally, they rated how much overlap they felt with the cohort as a whole. This was done using the same format as above, except that the cohort was displayed by a larger circle and the 'self' circle placed inside the cohort circle at the far end (in line with recommendations made by Reddish, 2012).

Self-construal: Self-construal was measured by a 45-item Aspects of Identity Questionnaire that measures the relative importance individuals place on four personality characteristics when construing their self-definitions (Cheek, 1989). This measure was chosen instead of asking participants to rate how strongly they felt part of the group because it gives a richer understanding of their current self-construal, yet is more easily quantifiable than the twenty statements test. This measure was taken from the measurement instrument database for the social sciences (<http://www.midss.org/sites/default/files/aiq.pdf>). Participants rated items on a scale from 1 (not important to my sense of who I am) to 5 (extremely important to my sense of who I am).

- Personal identity (10 items, covering personal values and moral standards).
- Social identity (7 items, covering popularity with other people).
- Collective identity (8 items, covering areas such as being a part of many generations of a family).
- Relational identity (10 items, covering relationships with the people the participant feels close to).
- A further 10 items are classified as special items (such as the importance of owned possessions).

Economic game: The stag hunt game was used to measure cooperation between the participants. This game is a form of Prisoner's Dilemma and has been used in a similar context by Reddish et al. (2013). We adapted the version used by Reddish et al. to fit the current design with one minor change: the payout amounts were changed from dollars to comparable pound amounts. Participants had to choose between X (a guaranteed payout of £2.00 no matter what) or Y (a payout of £3.00 if the other person chooses Y, otherwise £0). Y is the cooperative choice, as it provides a higher payout but only if both players choose it. Participants played three rounds of the game sequentially (one with each other member of their cohort). All three rounds of the game were presented to the participant on a single page, and they responded on that page in whichever order they wished without conferring with the

other participants. In order to encourage meaningful choices participants were paid in line with their game performance.

RESULTS

Because we were primarily interested in whether those who had coordinated displayed more pro-sociality towards their fellow participants (co-actor and observers), we first examined the data for Movers.

Movers Results

We examined positive affect change scores (after – before) and task difficulty, success and enjoyment measures to see whether these varied across conditions, using a series of independent samples Mann-Whitney U tests and t-tests (distributions of all data were found to deviate significantly from normality, in each case $p < .05$ except for the mood measure where $p > .05$). There were no significant effects for any variable, so we concluded that mood ($t(46) = 0.06$, $p = .96$, $d = 0.02$), task enjoyment ($U = 284.5$, $Z = 0.08$, $p = .94$, $r = 0.01$), task difficulty ($U = 210$, $Z = 1.7$, $p = .09$, $r = 0.24$) and perceived success ($U = 296.5$, $Z = 0.196$, $p = .85$, $r = 0.03$) did not differ between conditions and were therefore unlikely to have contributed to the effects described below. All descriptive statistics for these measures can be found in Table 1.

[INSERT TABLE 1 HERE]

Coordination

We then checked that participants were coordinating appropriately during the CRM task. All movement trials except for the practice rounds were analyzed. We filtered the time series of each dot's position using a low-pass Butterworth filter with a cut-off frequency of 10Hz, and then differentiated this to yield velocity. We then computed a time series of the relative phase between the two dots as the difference between the arctangent of each dot's velocity over position at each sample. Coordination was measured using mean vector length (MVL), which is the circular statistics measure of data dispersion (Batschelet, 1981; see Wilson et al., 2005a, b for more detail). MVL is the normalized length of the resultant vector (obtained by summing the relative phase vectors from each time step) and measures

coordination stability. MVL ranges from 0 (indicating minimum stability, a uniform circular distribution) to 1 (indicating maximum stability, no variability). It effectively summarizes how consistent the relative phasing (or coordination) was between the movements.

The MVL scores of those in the Coordinated condition ($p=.064$) did not deviate from normality while the scores of those in the Uncoordinated condition differed significantly from normality ($p=.001$). Therefore, we performed an independent samples Mann-Whitney U test. A significant effect of Movement Type on coordination scores was found ($U=576$, $Z=5.95$, $p<.001$, $r=0.85$). Those in the Coordinated condition ($M=0.791$, $Mdn =0.791$, $SD=0.057$) coordinated significantly more consistently than those in the Uncoordinated condition ($M=0.04$, $Mdn =0.035$, $SD=0.024$). This confirmed that our movement manipulation had created the required contexts in which we can now interpret the following results.

Cooperation

We explored whether there was an association between the type of movement task a person performed and whether participants chose to cooperate with each member of their experimental cohort, using a series of Chi-Square tests. We first looked at whether cooperation between co-actors was affected by the type of movement task a person engaged in. A Pearson's Chi-Square found a significant association ($X^2 (1)=4.27$, $p=.04$); 54% of participants in the Coordination condition chose the cooperative option compared with 25% in the Control condition (see Figure 2). Odds ratios indicated that the odds of a participant cooperating with their co-actor after coordination were 3.93 times higher than after making uncoordinated movements. Replicating past results, moving together with another person increased the likelihood of cooperation with that person.

We then looked to see if there was any evidence of cooperation towards the Present Observer (PO) or Absent Observer (AO). We explored whether there was an association between the type of movement task participants engaged in and their cooperation with the observers. Because we predicted we would not see greater cooperation towards observers following coordination, we also ran Bayesian analysis alongside classical frequentist analysis here. One advantage of the Bayesian approach is that, using the Bayes factor (BF01), we can

make an inference about the likelihood of observing the data we did given the null hypothesis (Dienes & McLatchie 2017; Jaroz & Wiley 2014). A Bayes factor of approximately 3 and above is generally considered good evidence for a given hypothesis (Kass & Raftery 1995). Bayesian analysis was conducted using the independent multinomial sampling method (Jamil et al. 2016) and default priors using the JASP software (JASP Team, 2018).

Two separate Chi-Square tests and their Bayesian counterparts were run: one with cooperation scores towards the PO ($X^2(1)=0.097$, $p=.76$; $Bf_{01}=2.97$) and one towards the AO ($X^2(1)=0.01$, $p=.99$; $Bf_{01}=3.32$). Neither test revealed any association and Bayesian analysis provides some support for the null hypothesis, suggesting that CRM does not affect pro-sociality towards those not involved in the coordination. To summarize: participants who coordinated cooperated more with their co-actors than participants who had performed the uncoordinated version of the task, but this did not generalize to either of the observers. This suggests there was no inside-out generalization of cooperation following the CRM task. Figure 2 shows the percentage of Movers who chose to cooperate with their fellow co-actor and each of the observers.

[INSERT FIGURE 2 HERE]

Potential Mediators

To check that the cohesion items were all tapping the same construct, scale analysis was undertaken using the four baseline cohesion items they had answered about the group as a whole (as all participants answered these questions, and had had identical experiences in the study at this point). The four cohesion items were subject to Principal Components Analysis and the sampling adequacy for the analysis was verified ($KMO=0.799$, $p<.001$). Only one factor was extracted, with an Eigen value of 2.871, and it explained 71.77% of the variance. The scree plot also suggested retaining one factor, as did analysis of the component matrix (see Table 2 for the factor loadings). Cronbach's alpha confirmed the reliability of averaging these four scores into a single cohesion index ($\alpha=0.854$). Cohesion change items were then generated separately towards each co-actor and the group as a whole

by subtracting each of the before scores from after scores for each item, and then averaging the four co-actor cohesion change scores and the four cohort cohesion change scores separately.

[INSERT TABLE 2 HERE]

To check whether Cohesion, Overlap or Trust might be mediating the relationship between coordination and cooperation amongst co-actors, we performed mediation analysis using model 4 of the process toolbox (in line with recommendations by Field, 2013 and Hayes, 2013). Condition was included as the IV, Cooperation as the DV, and Cohesion, Trust and Overlap change scores (after – before) towards the co-actor and towards the group as a whole were included as mediators. There were no significant indirect effects through any of the potential mediators ($p > .1$ in each case). See Table 3 for the specific inferential statistics. In summary, none of the candidate mediators explained the pattern of variation in cooperation between Movers as a function of movement task. Descriptive statistics for each of the candidate mediators can be found in Table 4.

[INSERT TABLE 3 HERE]

[INSERT TABLE 4 HERE]

Self-construal items failed the initial checks that they mapped on to their relevant constructs. We subjected the 35 self-construal items covered in the 4 categories to PCA and verified the sampling adequacy with a Kaiser-Meyer-Olkin measure ($KMO=0.674$, $p < .001$). Ten factors were extracted with Eigen values above 1, which only explained 18.06% of the variance. The scree plot suggested retaining only three factors. Cronbach's alpha indicated that items were more reliable as a single scale ($\alpha=0.801$) than the separate components: personal ($\alpha=0.72$), relational, ($\alpha=0.803$), social ($\alpha=0.61$) and collective ($\alpha=0.75$). Therefore we did not combine these items to form the four subscales for use in mediation analyses as planned. Instead, we conducted exploratory, post hoc analysis on this measure. All items were entered into a MANOVA as DV's with movement condition as an IV. Using Pillai's trace, there was an effect of movement type on self-construal ($V=0.999$, $F(2,45)=31.844$, $p=.031$). Four of the items significantly differed between conditions at the $p < .05$ level (national pride, sports

team fan, being a unique person and gender) and one (race/ethnicity) differed at the $p < .01$ level. Specifically, participants rated all these items as being less important to their sense of who they were after Coordination (compared with Uncoordinated movements). All inferential and descriptive statistics for these items are given in Table 5.

[INSERT TABLE 5 HERE]

Observer Results

The following results are for those who just observed the movement task. We first examined whether the perceived coordination individuals observed varied across conditions, using a series of independent samples Mann-Whitney U tests (distributions were found to deviate significantly from normality, in both cases $p < .001$). Those who observed the coordinated condition did report significantly more perceived coordination between Movers than those who observed the uncoordinated condition ($U = 446.0$, $Z = 3.71$, $p < .001$, $r = 0.53$). This confirmed that observers had accurately observed the relevant movement types, so we were able to proceed with interpreting the results.

We explored whether there was an association between the type of movement task a person observed (Coordinated or Uncoordinated), and whether participants chose to cooperate with either their fellow observer or either of the two Movers using a series of Chi-Square tests. Three separate Pearson's Chi-Square tests were run for each observer type (AO, PO): one with cooperation scores relating to the other observer and two with cooperation scores relating to each of the two Movers. None of the six tests revealed an association between the type of movement task a participant observed and the proportion of participants who chose to cooperate with a given target (see Table 6 for individual test statistics). Figure 3 shows the cooperation percentages for each type of observer towards their fellow observer and an averaged cooperation percentage towards Movers (for context, the significantly higher cooperation seen amongst co-actors was above 50%). Therefore these results do not provide any evidence for outside-in generalization.

[INSERT TABLE 6 HERE]

[INSERT FIGURE 3 HERE]

DISCUSSION

The current experiment found greater cooperation after coordinated movements compared with uncoordinated movements. Importantly, increases in cooperation were restricted to co-actors, and did not generalize out towards either type of observer. We therefore found no evidence of inside-out generalization. Cooperation also did not significantly differ after either task for observers, and thus we also found no evidence of outside-in generalization. Given these results, why might other research have found evidence of generalization?

It may be the case that generalization of effects depends upon group size. The present study had pairs of participants engage in a CRM task, but research by Reddish et al. (2014) showing generalized effects had used larger groups of between three and six. It may be the case that effects only generalize when the coordinating group is larger than two. However, Tarr et al. (2015) also failed to show generalization of effects using groups of three. So although it is possible, we do not consider group size to be a likely explanation. Another potential explanation is that generalization depends upon the particular CRM task used. It could be argued that the foot tapping and limb movement tasks used by Reddish et al. (2014) were more engaging than moving a joystick, and that this was sufficient to foster generalized pro-sociality. However, Cirelli et al. (2014), Kokal et al. (2011) and Tarr et al. (2015) used CRM tasks that are arguably as engaging as foot tapping (bouncing to music, drumming and body movements), and found no generalization. Therefore we do not consider task engagement to be a likely explanation.

It is worth noting that Lang et al. (2017) suggests that there are two pathways to the pro-social effects of coordination, one of which (an increase in endorphin release) is only thought to occur when tasks are sufficiently strenuous to engage the endogenous opioid system (EOS). It may be the case that our task was insufficiently strenuous to engage the EOS and therefore incapable of leading to generalized effects. However, we also consider this possibility unlikely, since foot tapping is unlikely to be considerably more strenuous than the arm movements used here. It is also worth noting that other social effects of CRM (e.g. greater

obedience in committing destructive acts (see Wiltermuth, 2012) have also been found to be restricted to only those who participate in the coordination.

One reason for the differences in generalized pro-sociality seen in the Reddish et al. (2014) experiments might actually be *decreased* generalized pro-sociality post-control. Experiment 1 of Cross et al. (2017) suggested that certain control tasks might be capable of affecting cooperation in the opposite way to CRM (i.e. reducing cooperation). The control tasks employed in Reddish et al. (sitting and watching other people engage in a joint action task, and completing a jigsaw) may well foster greater feelings of individuation in participants, highlighting their position as 'not a part of the coordinating group', which might lead to decreased pro-sociality in these conditions. This explanation is supported by the fact that no differences in generalized pro-sociality were found between asynchronous and synchronous movement conditions in Reddish et al. (2014). This further highlights the need to employ control tasks that carefully match experimental tasks in every element bar the actual coordination (as suggested by Cross et al., 2016 and Tarr, Slater & Cohen, 2018).

Impression management is another alternative explanation for the findings of Reddish et al. (2014). It has also been shown by Engelmann, Over, Herrman and Tomasello (2013) that people are more likely to share resources more fairly with other if they believe they are being observed by a common group member. Cirelli (2018), Cross et al. (2019), Good et al. (2017) and Tuncgenc and Cohen (2016) suggest that CRM can act as a cue for common group membership. This may explain why only about half of the people who offered to help in study 1 of Reddish et al. (2014) actually followed through with their offer, and why monetary resources were shared out more evenly by those who had previously coordinated in study 2. In fact, participants in the experimental condition did not donate more to the other group than their previously coordinated co-actors, but shared money out roughly equally between groups. Therefore, impression management may offer a more plausible alternative explanation for these findings than generalized pro-sociality.

The current experiment and the majority of work to date (Cirelli et al. 2014; Kokal et al. 2011; Tarr et al. 2015) suggests that the pro-social effects of CRM do not typically generalize

to those outside of the coordination, but are restricted to co-actors. However, this may not always be the case, especially if non-actors and co-actors share specific affiliative links. Cirelli, Wan, and Trainor (2016) suggest that CRM may foster pro-social effects towards affiliates of co-actors. In this study infants saw a skit showing an affiliative or neutral interaction between two people. They were then moved either synchronously or asynchronously with one of those people. After the task, infants who had been moved synchronously displayed more pro-sociality towards people who had been shown to be affiliated to their coordinated co-actor, compared with the non-affiliates. Pro-sociality following coordination may therefore only generalize to affiliates (common group members) of those people whom we coordinate with, which suggests that these effects may be intimately tied to group membership.

If coordination acts a marker to common group membership, this may help explain why more pro-sociality is seen towards people with whom one has previously coordinated (Cirelli 2018; Good et al., 2017) and affiliates of co-actors (Cirelli et al., 2016). Furthermore, Tuncgenc and Cohen (2016) and Good et al. (2017) and Cross et al (2019) have shown that greater pro-sociality after coordination (cf. control) is not found when co-actors already view each other as common group members. In this case, cooperation post-coordination is equal to that seen as a result of minimal group formation, which suggests the pro-social effects of coordination may be related to changes in how co-actors view their identities following coordination. People are more likely to view themselves and their co-actors as a single unit and in common group terms than as separate individuals following coordination (Cross et al., 2017 and Good et al., 2017).

The present study included a measure of self-construal in order to test whether certain kinds of social identities were more prevalent after coordination, and if this mediated the effects of coordination on pro-sociality. Exploratory analysis suggests that there were differences in how people perceive their self-construal, after coordination. Specifically, people rated items relating to race, gender, nationality, sports team affiliations, and being unique as less important aspects of their identity post coordination. This suggests people place less weight on more stable aspects of their identity and also see themselves as less of a unique

individual following coordination. Given that the relationship between other forms of coordination (mimicry) and pro-sociality are thought to be underpinned by changes in self-construal (Ashton et al., 2007), this may prove a fruitful area in which to investigate potential mediators. Future work may wish to explore how changes in self-construal interacts with the other social consequences of CRM. In particular, work could be done using richer measures of self-construal, such as the twenty statements test used by Ashton et al.. This test allows participants to generate items that are pertinent to their own sense of identity, rather than simply rating items selected for them by researchers.

It would also be of interest for future work to explore how the structure of experimental groups affects the relationship between CRM and pro-sociality. To date, little work has explored how the makeup of experimental cohorts affects both the coordination and its pro-social effects, with the work of Tuncgenc and Cohen (2016) and Good et al. (2017) looking at minimal group structures, and Cross et al. (2019) looking at socio-culturally significant groups being exceptions. The gender, ethnicity and other subject factors inherent to the participants that make up our experimental cohorts may affect both if and how people engage in CRM, and the kinds of effects it has. For example, Lumsden, Miles, Richardson, Smith and Macrae (2012) have shown that people with a pro-social orientation coordinate more tightly than those with a pro-self orientation. Other aspects of our participant cohorts may also affect the relationship between CRM and pro-sociality, which is something future work should take into account.

This work showed greater cooperation in an economic game post coordination compared with uncoordinated movements, but only towards those actually involved in the coordination itself (not towards observers). This suggests that the pro-social consequences of entrainment are restricted to those who actually engage in the coordination episode and do not ordinarily generalize to outsiders. It also provides alternative accounts of results that apparently provide evidence for generalized pro-sociality. These findings add weight to the growing body of work that does not find support for a mediating role of group cohesion or self-other overlap in explaining the relationship between CRM and pro-sociality. We have also

provided some exploratory findings suggesting that people may view their self-construal in different ways following coordination, which also adds weight to the growing body of work suggesting that the social effects of CRM may be intimately tied to group membership.

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TABLES

Table 1.

Mean and SDs for task difficulty, enjoyment, success and mood

Condition	Measure	Mean	Mdn	SD
Coordinated	Task difficulty	2.46	2.48	0.72
	Task success	2.42	2.53	0.93
	Task enjoyment	2	2.06	0.93
	Mood	-0.15	.2	1.17
Uncoordinated	Task difficulty	2.86	3.0	1.04
	Task success	2.38	2.47	0.92
	Task enjoyment	2	2.07	1.18
	Mood	-0.12	2.5	2.22

Table 2.

Factor loadings for the cohesion measure

Items	Item Loading
How similar do you feel to the cohort as a whole?	0.841
How close do you feel to the cohort as a whole?	0.801
How much do you like the cohort as a whole?	0.873
How much would you like to see the cohort again?	0.871

Table 3.

Inferential statistics for Mediation analysis

Tests	Inferential statistics
Indirect effect through Co-Actor Cohesion	Z=-0.32, p=.75
Indirect effect through Co-Actor Overlap	Z=-0.62, p=.53
Indirect effect through Co-Actor Trust	Z=-0.15, p=.88
Indirect effect through Cohort Cohesion	Z=-0.01, p=.99
Indirect effect through Cohort Overlap	Z=-1.21, p=.26
Indirect effect through Cohort Trust	Z=-0.83, p=.41

Table 4.

Means and SDs for the cohesion, overlap and trust change scores of Movers

Measures	Coordinated			Uncoordinated		
	Mean	Mdn	SD	Mean	Mdn	SD
Cohesion (Co-Actor)	7.03	8.55	8.19	5.53	4.95	6.4
Cohesion (PO)	2.91	3.95	8.92	4.5	4.7	6.88
Cohesion (AO)	-3.57	-0.45	8.91	-1.11	-1.7	7.57
Cohesion (Cohort)	5.34	8.8	10.45	3.29	3.55	6.83
Overlap (Co-Actor)	2.57	2.55	3.52	1.0	1.65	3.49
Overlap (PO)	0.47	-.05	3.39	0.60	.85	3.37
Overlap (AO)	-1.7	-1.4	2.63	-1.12	-1.35	2.37
Overlap (Cohort)	1.32	.7	3.18	0.25	.05	2.44
Trust (Co-Actor)	2.32	1.7	2.84	1.03	.75	2.09
Trust (PO)	1.18	.9	2.35	1.32	0.9	1.97
Trust (AO)	-0.54	-.15	2.35	-1.15	-0.08	1.7
Trust (Cohort)	1.15	1.35	2.42	1.25	1.25	2.93

Table 5.

Self-construal inferential and descriptive statistics

Items	Coordinated		Uncoordinated		Inferential statistics
	Mean	SD	Mean	SD	
7. My race or ethnic background	1.96	0.86	2.54	1.35	F(1,46)=3.188, p=.081, $\eta^2=0.065$
19. My sex, being a male or a female	2.88	1.23	3.75	1.19	F(1,46)=6.296, p=.016, $\eta^2=0.12$
21. My feeling of being a unique person, being distinct from others	3.08	0.83	3.75	1.19	F(1,46)=5.076, p=.029, $\eta^2=0.099$
29. My feeling of pride in my country, being proud to be a citizen	2.25	1.07	2.96	1.27	F(1,46)=4.364, p=.042, $\eta^2=0.087$
33. Being a sports fan, identifying with a sports team	1.5	0.72	2.21	1.53	F(1,46)=4.199, p=.046, $\eta^2=0.084$

Table 6.

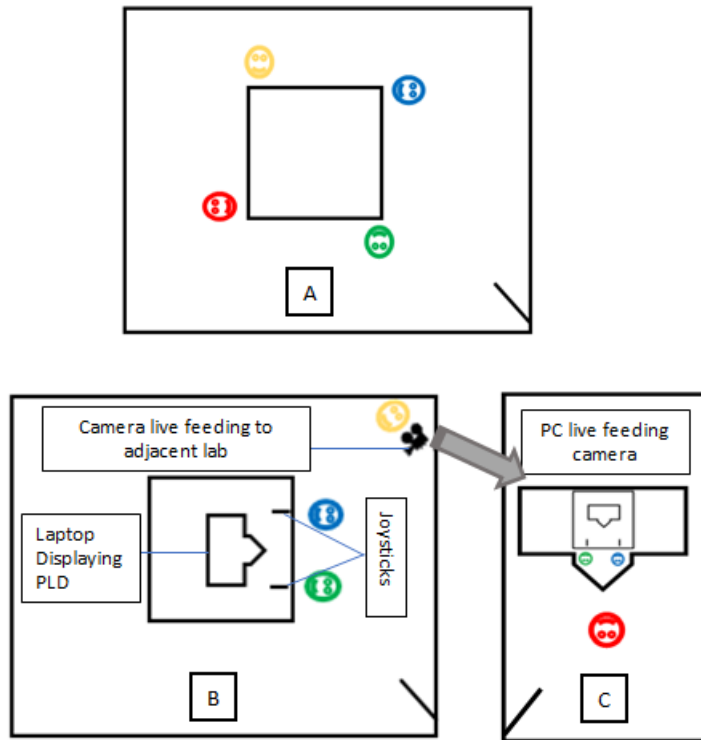
Chi-Square test statistics for Observers cooperation results

Participant	Target	Inferential statistic
AO	PO	$X^2(1)=0.2, p=.65,$
	M 1	$X^2(1)=0.25, p=.62,$
	M 2	$X^2(1)=0.38, p=.54,$
PO	AO	$X^2(1)=0.2, p=.65,$
	M 1	$X^2(1)=0.01, p=.99,$
	M 2	$X^2(1)=0.2, p=.65,$

FIGURES

Figure 1.

Depictions of the set up of the labs



A illustrates the initial lab set up, with all participants sat around a square meeting table, equidistant from each other. Yellow was the PO, Red the AO and Blue and Green the Movers. B illustrates the lab set up for the movement task, with the two Movers sat side by side at the table, with a joystick in front of each person, facing the laptop. The PO was sat in the corner, with a HD video camera directly above them. C illustrates the adjacent lab where the AO was placed, sitting in front of a computer screen that livestreamed from the camera above the PO in the main lab. Following the movement task participants returned to the initial set up for the remainder of the experiment as in A.

Figure 2.

Percentages of Mover cooperation scores towards co-actors and observers (outside-in)

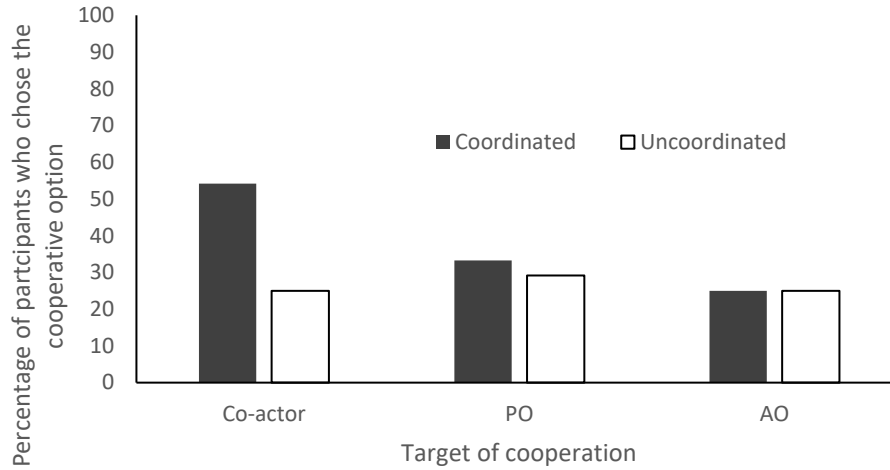


Figure 3.

Percentages of each observer types cooperation scores towards the Movers and the other observer (inside-out).

