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# Still want to help? Interpersonal coordination's effects on helping behaviour after a 24 hour delay

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ABSTRACT

A fast-growing literature is establishing how moving in time together has pro-social consequences, though no work to date has explored the persistence of these effects over time. Across two studies, people who had previously performed coordinated movements were over three times more likely to give their time to help their co-actor when asked 24 hours later than those who had performed a similar but uncoordinated task. Findings showed that group-level categorisation, but not social affiliation, partially mediated helping behaviour. This provides preliminary evidence that the pro-social effects of coordination are sustainable over a longer period than previously reported, and that the effects of coordination upon pro-social motivation may be more related to changes in group level categorisations than increased social affiliations.

Rhythmically coordinating our movements with people over time is a common feature of everyday life. People regularly dance, sing, chant, play music and even walk together in coordinated ways (McNeil, 1995). These forms of interpersonal coordination have been repeatedly shown to promote a wide range of pro-social behaviours amongst co-actors, including increased liking, rapport, trust, helping, cooperation and even decreasing negative attitudes (Anshel & Kipper, 1988; Atherton, Sebanz, & Cross, 2019; Cross, Wilson, & Golonka, 2016, 2019; Good & Russo, 2016; Hove & Risen, 2009; Kokal, Engel, Kirschner, & Keysers, 2011; Launay, Dean, & Bailes, 2014; Rabinowitch & Knafo-Noam, 2015; Rabinowitch & Meltzoff, 2017; Reddish, Fischer, & Bulbulia, 2013; Reddish, Bulbulia, & Fischer, 2014; Reddish, Tong, Jong, Lanman, & Whitehouse, 2016; Wiltermuth & Heath, 2009).

Why is it coordination has such effects? From an evolutionary perspective, one important insight is that coordination may serve as a reliable indicator of ingroup membership, since our ancestors would have coordinated their decision-making and actions with ingroup members very often, and with outsiders, much less often (Sterelny, 2012; Tomasello, 2012). If so, then there are various reasons why co-ordination with another agent would incline one to behave prosocially towards that agent. Direct (Trivers, 1971) and indirect (Nowak & Sigmund, 1998) reciprocity would both explain why it would be in one's own interests to behave prosocially towards ingroup members, namely because one would be likely to interact with them in the future (direct reciprocity; Trivers, 1971), or to maintain one's reputation within the group (indirect reciprocity; Nowak & Sigmund, 1998). In addition, other evolutionary mechanisms may explain why one might be genuinely interested in others' well-being irrespective of any ulterior motive. For example, the cultural group selection hypothesis (Henrich & Henrich, 2007; Boyd & Richerson, 1992) proposes that groups in which individuals act relatively unselfishly and in the interests of the group would be more likely to survive, creating a context in which selection pressure could favour genes for prosocial motives. Roberts' (2005) 'interdependence hypothesis' could also explain why one might be genuinely interested in the well-being of other group members. This hypothesis holds that humans' tendency to cooperate arose thoughout a period in which our ancestors lived in small groups of individuals whose interests were largely interdependent, and for whom it was therefore not typically beneficial to act selfishly to the detriment of other group members.

These evolutionary considerations motivate the hypothesis that greater pro-sociality following coordination is mediated by group level identification. In other words, pro-sociality is a reflection of changes in how co-actors view themselves in common group terms (Cirelli, 2018; Cross, Atherton, Wilson, & Golonka, 2017; Cross, Turgeon, & Atherton, 2019a, 2019b; Good, Choma, & Russo, 2017; Pearce, Launay, MacCarron, & Dunbar, 2017). And indeed, some recent research supports this. For example, Good et al. (2017) showed that people were

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more likely to rate themselves and their co-actors as a single group than as separate individuals following a coordination task. Similarly, Cross et al. (2017) showed changes in how people rated themselves after imagined coordination. In this study, imagining walking in synchrony with other people was shown to lead to increases in individuals' ratings of themselves in group level as opposed to individual terms, which correlated with other measures of pro-sociality.

The group level identification hypothesis is distinct from, though not incompatible with, a different hypothesis concerning the psychological mechanism mediating between coordination and subsequent prosocial motivation – namely, that coordination fosters pro-sociality through an increase in social affiliation amongst co-actors (Wiltermuth & Heath, 2009). This is typically assessed using self-report measures of how close, similar, and connected co-actors feel to one another. However, evidence for the mediating role of social affiliation in coordination's cultivation of pro-social behaviour is mixed (for a detailed review see Cross, Turgeon, & Atherton, 2019a). While some studies have found evidence for the role of affiliation as a potential mediator (Reddish et al., 2014; Wiltermuth & Heath, 2009; Wiltermuth, 2012), other studies do not support this link (Cross et al., 2016, 2017; Cross, Wilson, & Golonka, 2019; Fessler & Holbrook, 2014, 2016; Lang, Bahna, Shaver, Reddish, & Xygalatas, 2017; Reddish et al., 2013, 2016).

In the present study, we compared the group level identification hypothesis with the social affiliation hypothesis. To this end, we measured participants' affiliation towards the individual co-actor with those whom they had coordinated with, as well as their group level categorisation. This latter measure enabled us to gauge to what extent participants felt like individuals versus group members following a coordination period, in order to see which of these variables was more likely to explain any pattern in post coordination helping.

We also aimed to test a further implication of the background evolutionary theory – namely, that moving together in time should have long-lasting effects upon prosocial motivations towards co-actors. The group-level identification hypothesis motivates the hypothesis that these effects should be persistent over time because people's identification with groups tends to be stable over time, making it worthwhile to invest in group membership even when the rewards are temporally delayed. While some researchers have speculated that the prosocial effects of coordination should persist over time (Brown, 2000; Hagen & Bryant, 2003; Huron, 2001; McNeil, 1995; Merker, 2000), this has not yet been empirically demonstrated. Indeed, in previous research, measures of pro-sociality are typically taken immediately after a coordination period (i.e. Cross et al., 2016; Cross, Turgeon, & Atherton, 2019a, 2019b; Hove & Risen, 2009; Reddish et al., 2013, 2014; Wiltermuth & Heath, 2009; Valdesolo & DeSteno, 2011).

One study came close to addressing this question (Reddish et al., 2014). In this study, participants, after engaging in a movement task, were requested to take part in an unrelated online survey of varying lengths and were emailed the link to take part 24 hours after the coordination had taken place. Results showed that those participants who had moved synchronously with each other were more likely to volunteer for the longer study than those participants who had not moved synchronously with each other. However, the authors did not report whether previously coordinated individuals were more likely to follow through on their offer to help. To address this question, the present study conceptually replicated the helping paradigm used by Reddish et al. Crucially, we investigated whether participants who had previously coordinated were more likely to actually help their co-actor when emailed such a request 24 hours after engaging in the movement task, compared to participants who made similar yet uncoordinated movements with a co-actor.

Study 1 therefore, had two aims, to assess: 1) whether the pro-social effects of coordination persist twenty-four hours after the coordination had taken place and 2) whether affiliation and/or group level categorisation could explain the relationship between pro-sociality and coordination. It was hypothesised that 1) people who coordinated would be more likely to help their co-actor after a temporal delay than those who performed a matched uncoordinated version of the task, and 2) those who had previously coordinated would report feeling more like group members (and less like unique individuals) than those in the uncoordinated condition and 3) this would better mediate helping patterns than measures of social affiliation.

## 1. Study 1

### 1.1. Method

#### 1.1.1. Participants & design

Seventy students and staff at The University of Buckingham volunteered to participate (two participants declined to record age and gender, of the remaining 68 there were 21 males and 47 females, $M_{age}$ = 24.93 yr, $SD_{age}$ = 8.67). The sample was specified in the design phrase based on the sample size used by Reddish et al. (2014) of 26–33 per group. Power analysis using Gpower predicted a sample of 35 participants per condition gave us > 90% power to detect effect sizes of d = 0.7 as seen in Reddish et al. (2014). The study employed an experimental design with a single between-subjects factor, Movement Type, which had two levels: Coordinated (i.e. in-phase or 0°), or Uncoordinated (Control). Participants were tested individually and there were equal numbers of participants per condition. Participants were assigned to conditions quasi-randomly using a pre-defined order set before testing commenced. Participants were informed at the outset via an information sheet that the study was measuring how personality is related to how we perform movement tasks. This experiment was approved by the School of Science and Medicine Ethics Committee of Buckingham University. Participants were compensated with a small monetary payment (£3) in line with UK minimum wage for 20 min work, which is the recommended payment for study participants (Dickert & Grady, 1999).

#### 1.1.2. Manipulation, measures & procedure

Participants were brought in to the lab and introduced to the experimenter, who was a visiting researcher from another country and was unknown to the participants. They first took part in the movement task. The participant and the experimenter each moved a joystick (Genius MaxFighter F-17 with force feedback disabled) while sitting side by side. In each condition, the participant moved the joystick horizontally at 0.75 Hz. In the Coordinated condition, the experimenter moved their joystick at the same speed using a Point Light Display (PLD) to coordinate their movements so as to maintain 0° relative motion (i.e. in-phase). This PLD consisted of two white feedback dots displayed on a black background by a single laptop screen positioned approximately 1 m in front of them. In the Uncoordinated condition, the participant moved in exactly the same way, but the experimenter alternated their speed between 0.6 Hz and 0.9 Hz (0.75 Hz ± 0.15 Hz). The experimenter had been previously trained to move at the various required speeds to ensure they were not falling in-phase with the participant in the Uncoordinated condition.

Participants in all conditions first saw two 15 s demonstrations of dots moving at the desired phase and frequency. Participants were asked to move in the same way and at the same speed as their respective dot in the demo. The participant was always replicating the movements of the bottom dot, and the experimenter the top dot. In the experimental condition, both dots moved at 0.75 Hz (at 0° relative to each other). In the control condition, the bottom dot (representing the participant) still moved at 0.75 Hz throughout all trials, while the speed of the top dot (representing the experimenter) alternated from 0.6 Hz (slower) to 0.9 Hz (faster). Instructions were closely matched between conditions. Participants were instructed that their task was to replicate the movements of the bottom dot. Meanwhile, the experimenter would be making similar movements replicating the top dot. They were told that their task was always to replicate the direction and speed of the

bottom dots movement, no matter whether the other dot was the same or not.

Instructions were presented to the participant in text. Verbal interaction with the experimenter was kept to a minimum. The experimenter answered questions verbatim from a carefully worded script to ensure consistency between groups. After the initial PLD demonstrations, participants had 30s practice time to acquaint themselves with the required movements. Following this brief initial practice, participants completed six 60s trials. Each trial was preceded by a four-second version of the PLD demonstration pacing them to the required phase and frequency of movements. This experiment was run on a MacBook Pro with a custom Matlab toolbox (Wilson, Collins, & Bingham, 2005). Since in-phase coordination is known to be a strong attractor state (Kelso, 1995), in the control condition the PLD was only visible for the first 15 s of the trial before it disappeared. This was done in order to ensure participants did not end up falling into an in-phase movement pattern with the experimenter in the uncoordinated condition.

Following the movement task, participants completed self-report measures of social affiliation and group categorisation. The social affiliation scale measured how close, similar and connected the participant felt to the experimenter. This was assessed in 3 separate questions (how close do you feel to the experimenter, how similar do you feel to the experimenter, how connected do you feel to the experimenter), all responded to using a 185 mm continuum, anchored from 'Not at all' to 'Very much so.' Participants were told that there were multiple experimenters running the study, and these questions were necessary to confirm whether there were any differences between individual experimenters.

The group categorisation measure consisted of three questions assessing how participants construed themselves in individual vs group terms. It was scored on a 185 mm continuum, and contained the following questions: How much do you see yourself as (an individual – a group member)? To what extent do you think of yourself as a unique individual (not at all – very much so)? (this item was reverse scored) To what extent do you qualify as a group member (not at all – very much so)?

The group categorisation measure is identical to that used by Cross et al. (2017), originally adapted from Hutter, Wood and Turner (2013), and it is typically termed as a measure of deindividuation. However, the term deindividuation often has a different connotation than that of the construct measured in this scale. Deindividuation typically refers to a decreased state of self-awareness in which an individual pays greater attention to the collective environment than to their own internalized standard of conduct (Reicher, Spears, & Postmes, 1995). This decreased sense of self can result in antinormative "lynch mob" types of behaviour (Festinger, Pepitone, & Newcomb, 1952; Zimbardo, 1969), particularly when such behaviour is reflective of what is normative for the group rather than the individual. While research shows that deindividuation is more common in groups and varies as a function of group size (see Postmes & Spears, 1998 for a review), some argue that group immersion is merely a catalyst for the state of decreased self-awareness which defines deindividuation (Diener, 1979). Others disagree, and instead propose that rather than decreasing self-awareness, deindividuation represents a shift from an individual to a group identity (Reicher, 1984), which is more in line with the measure used here. As there is disagreement about the nature of this construct, this scale will be referred to as a measure of group level categorisation, as we are interested in how people categorise themselves in individual vs group terms.

Before leaving participants were asked if they would be willing to complete a further unrelated study for the experimenter in their own time. They were told that this was not paid but would only take about 30 minutes and could be done online. The wording of this request was read verbatim from a prepared script and standardized across sessions. Participants were asked if they would leave a contact email address and were then emailed a tracked Qualtrics invitation to participate the next day, 24 hours after they had participated. This link directed participants to a pilot study for a different experiment which involved various self-report measures and a common measure of theory of mind and took around 30–45 minutes to complete. This link stayed active for one week, after which participants' email addresses and the tracked invitation were destroyed.

### 1.2. Results

All data was first checked for parametric assumptions, using Shapiro-Wilkes tests, where checks failed, non-parametric tests were used.

We first checked that participants were performing the movement task appropriately. All movement trials except for the practice round were analysed. A low-pass Butterworth filter with a cut-off frequency of 10 Hz filtered each dot's position time series. A 60 Hz time series of the relative phase between the two dots was computed as the difference between the arctangent of each dot's velocity over position at each sample. Mean vector length (MVL) is the circular equivalent of the standard deviation (see Wilson et al. (2005) for more details). It is the normalised length of the resultant vector obtained by summing the relative phase vectors from each time step and measures coordination stability. It effectively summarises how consistent the relative phasing (the coordination) between the movements were. MVL ranges from 0 (indicating minimum stability, a uniform circular distribution) to 1 (indicating maximum stability, no variability). Those who were instructed to move in a coordinated way (M = 0.818, SD = 0.082, Mdn = 0.837, range: 0.660–0.962) did achieve more consistent coordination than those in the control condition (M = 0.149, SD = 0.068, Mdn = 0.13, range: 0.043–0.283) U = 1225.0, p < .001, $r$ = 0.860. This confirmed that our movement manipulation had created the desired contexts in which we can now interpret the following results.

Whether participants completed the additional survey when sent the link 24 hours later was used as a measure of delayed helping. A greater proportion of people in the coordinated condition completed the online survey than those in the uncoordinated condition $\chi 2$ (1, n = 70) = 5.51, p = .019, V = 0.28. Based on the odds ratio, the odds of those in the coordinated condition revealed them to be 3.625 times more likely than those in the uncoordinated condition to follow through with their offer to help (see Fig. 1 for percentages of people who helped in either condition).

Affiliation composite scores were computed by taking the average of the three affiliation items Cronbach's alpha confirmed the scale was suitably robust to be combined into a composite score (a = 0.890). Those in the coordinated condition reported feeling more affiliated to the experimenter than those in the uncoordinated condition, U = 780.0, p = .049, r = 0.24. Descriptive statics can be found in Table 1.

The unique item of the solidarity scale was first reversed scored to

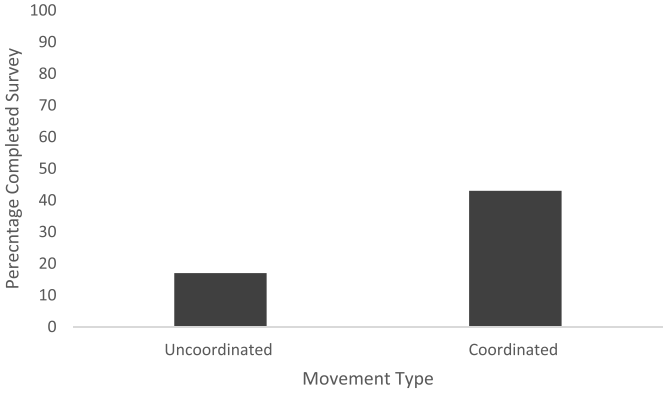


**Fig. 1.** The percentages of participants who followed through with their offer to help.

**Table 1**
Descriptive statistics for the affiliation composite measure and self-categorisation items.

| | | Unique item | Group-member item | Comparison item | Affiliation Total |
|---|---|---|---|---|---|
| Uncoordinated | Mean | 130.8 | 112.23 | 61.09 | 65.58 |
| | SD | 42.73 | 40.99 | 43.9 | 37.68 |
| | SE | 7.22 | 6.93 | 7.42 | 6.37 |
| | Median | 135.5 | 116.67 | 52.0 | 59.67 |
| | Range | 1–185 | 22–178 | 0–154 | 9–136.33 |
| Coordinated | Mean | 128.86 | 126.12 | 97.34 | 85.33 |
| | SD | 44.03 | 40.61 | 54.1 | 42.33 |
| | SE | 7.44 | 6.86 | 9.15 | 7.15 |
| | Median | 138.0 | 125.0 | 94.67 | 97.33 |
| | Range | 5–185 | 41–185 | 2–185 | 2–164.33 |

be in line with the other items. The Cronbach's alpha for combining all 3 items was only 0.462, the Cronbach's alpha rose to 0.613, if the unique item was deleted, although this is still not generally considered acceptable (Nunally & Bernstein, 1994). Therefore we did not combine these items as originally planned and instead analysed individual questions separately. Those in the coordinated condition rated themselves as more like a group member than a unique individual in the comparison question than those in the uncoordinated condition $U = 854.5.0$, $p = .005$, $r = 0.34$. While participants in the coordinated condition did give higher ratings for the question assessing the degree to which they considered themselves a group member than those in the uncoordinated condition, this difference was not significant $U = 725.5$, $p = .184$, $r = 0.19$. Finally, while participants in the coordinated condition also gave marginally lower ratings for the question assessing the degree to which they felt like a unique individual compared to those in the control condition, this difference was also not significant $U = 597.0$, $p = .855$, $r = 0.03$. Table 1 gives the descriptive statistics for each of those questions by condition.

There was a positive correlation between the items measuring the degree to which participants rated themselves as either an individual or a group member and whether they completed the survey ($rs = 0.270$, $p = .024$) and how affiliated they felt towards the experimenter ($rs = 0.290$, $p = .015$). However, there did not appear to be a relationship between affiliation and whether somebody completed the survey ($rs = 0.046$, $p = .704$). The odds ratio shows that participants were 3.625 times more likely to complete the additional survey if they had participated in the coordinated condition than in the uncoordinated condition. Controlling for affiliation did not substantially decrease the odds ratio, in fact, the odds of someone completing the additional survey if they had participated in the coordinated condition rose slightly to 3.764 when affiliation was controlled for. Controlling for the aforementioned self-categorisation question, however, reduced the odds ratio by around a quarter. Specifically, the odds of someone completing the additional survey if they had participated in the coordinated condition when group level categorisation was controlled for, was reduced to 2.7. This suggests that affiliation is not likely to be mediating the relationship between coordination and helping behaviour, while group level categorisation may be partially mediating this relationship.

### *1.3. Discussion*

The pro-social effects of interpersonal coordination are well established (i.e. Anshel & Kipper, 1988; Hove & Risen, 2009; Wiltermuth & Heath, 2009), and this work now provides the first evidence that lab-based coordination tasks are capable of affecting pro-sociality some time after the actual coordination has taken place. Study 1's results showed that people were over three and a half times more likely to help someone they had coordinated with 24 hours prior than if they had previously performed a similar but uncoordinated movement task with that person.

Results showed that greater group-level categorisation was not only present post coordination but may also partially mediate coordination's effects on helping. The way in which participants classified themselves in individual vs group terms was shown to positively correlate with levels of helping, and controlling for this substantially reduced coordination's effect on cooperation. This was not the case for social affiliation. While greater social affiliation was seen post coordination, this did not seem to interact with coordination's relationship with helping behaviour.

Every attempt was made to minimize the effects of experimenter expectancy and social desirability in this work. Due to the nature of this study, the experimenter could not be blinded to condition, though they were blind to the research predictions. Experimenter interaction with the participant was kept to an absolute minimum. All instructions for the tasks were identical for every participant and presented in text. Where necessary the experimenter worked from carefully worded scripts and was trained to standardise their behaviour. Therefore, we do not believe that experimenter expectancy and social desirability are likely explanations for our findings. However, the alternative explanation that coordination led the experimenter to act more pro-socially towards the participant, particularly when requesting participation in the future study, cannot be ruled out. To address this, we designed Study 2 to replicate these findings and to remove this potential confounds by ensuring that the experimenter would not interact with the participant following the movement task. To this end, in study 2, participants went to a separate lab after the movement task, and were instructed to fill in the self-report measures online, while alone. A final question was embedded into the end of this survey asking them to indicate whether they would be willing to participate in future research for the experimenter.

## 2. Study 2 methods

Study 2 replicates the methods of Study 1, while controlling for the potential confound of experimenter effects. A wide range of coordination tasks have been shown to foster pro-sociality amongst those who take part (walking, drumming, dancing, tapping, for a review see, Cross, Turgeon, & Atherton, 2019a). A tapping task like that used by Hove and Risen (2009) was used in lieu of the joystick task from Study 1, in order to demonstrate that the greater delayed helping follow's interpersonal coordination in general rather than being specific to the exact type of coordination task previously used. Study 2 was tested in a new location, and used a different experimenter, a research assistant at the new institution, who was completely blind to all experimental aims and hypotheses.

Sixty students and staff members at The University of Wolverhampton volunteered to participate (24 Males, 36 females, $M_{age} = 28.2$ yr, $SD_{age} = 9.6$). Power analysis using Gpower predicted a sample of 30 participants per condition gave us > 80% power to detect effect sizes of $v = 0.28$ based on the findings of Study 1. Participants were informed at the outset via an information sheet that the study was

measuring how personality is related to how we perform movement tasks. This experiment was approved by the University of Wolverhampton Ethics Committee. Instead of cash, participants were compensated with course credits and entered into a raffle to win Amazon gift vouchers.

Participants were brought in to the lab and introduced to the experimenter, who was a research assistant blind to all experimental aims and hypothesis and unknown to the participants. They first took part in the tapping task using a custom Psychopy script. The participant and the experimenter tapped on a keyboard key (Q or P), whenever they heard a beat, while sitting side by side. In each condition, right handed participants sat on the right and tapped the P key with their index finger of the right hand, the experimenter on the left always tapping the Q key with their index finger (and vice-versa for left handed participants).

The participants always heard a steady metronome of 60BPM in both conditions, and were instructed to tap their key when they heard a beat. In the coordinated conditions the experimenter heard and tapped to the same beat as the participant, in the uncoordinated condition the experimenter heard and tapped to an irregular beat, which contained the same number of overall stimuli, but presented in an irregular and unpredictable fashion designed to not co-occur with the participant's beats. See Fig. 2 for a schematic of the auditory stimuli. A white cross appeared on the relevant side of the screen whenever a key press was registered. Participants in all conditions first had two 30 s practice trials to familiarise themselves with the task. Participants were asked to tap their respective key in time with the beat they heard. Meanwhile, the experimenter would be tapping to their beat. Instructions were presented to the participant in text. Verbal interaction with the experimenter was kept to an absolute minimum. After the initial practice there were three 60 s trials.

Following the movement task, the experimenter showed the participant to another room where they completed self-report measures of social affiliation and group level categorisation. Due to issues with scale validity with the group categorisation measure from study 1, only the comparison question asking how much do you see yourself as a unique individual (0) or a group member (100) was used here. This was supplemented with a further question assessing how much participants identified as being a part of the same group as the experimenter, rated on the same scale. Affiliation was measured using the same three questions from study 1. They completed these measures on Qualtrics, alone in a separate lab, and at the end of the survey were presented with a final screen asking if they would be willing to compete future online studies for the experimenter, which they answered by ticking the relevant (yes or no) box presented on screen. After completion participants were debriefed on screen and thanked for their time. Twenty four hours after participating, participations were emailed a tracked Qualtrics invitation to participate in a further study which involved various self-report measures and a common measure of theory of mind which took around 30–45 min to complete. This link stayed active for one week, after which the log of participants email addresses and the tracked invitation were destroyed.

### 2.1. Results

We first checked that participants were performing the movement task appropriately. In line with Hove and Risen (2009) we calculated the average proportion of taps occurring within 100 ms of each other for each pair across the three trials. Those in the coordinated condition had a greater proportion of taps occurring within 100 ms (M = 0.826, SD = 0.123, Mdn = 0.856, range: 0.51–0.98) than those in the uncoordinated condition (M = 0.189, SD = 0.035, Mdn = 0.191, range: 0.11–0.26), U = 900.0, p < .001, r = 0.86, which confirmed that our movement manipulation had created the desired contexts in which we can now interpret the following results.

Whether participants completed the additional survey when sent the link 24 hours later was again used as a measure of delayed helping. A greater proportion of people in the coordinated condition completed the online survey than those in the uncoordinated condition (see Fig. 3 for percentages of people who helped in either condition). X2 (1, n = 60) = 4.02, p = .045, V = 0.259. Based on the odds ratio, the odds of those in the coordinated condition's revealed them to be around 3.33 times more likely than those in the uncoordinated condition to follow through with their offer to help.

Affiliation composite scores were computed by taking the average of the 3 affiliation items Cronbach's alpha again confirmed the validity of combining these into a composite score (a = 0.894). Those in the coordinated condition did report feeling slightly more affiliated with the experimenter than those in the uncoordinated condition, but this difference was not significant (U = 473.5, p = .728, r = 0.044). Those in

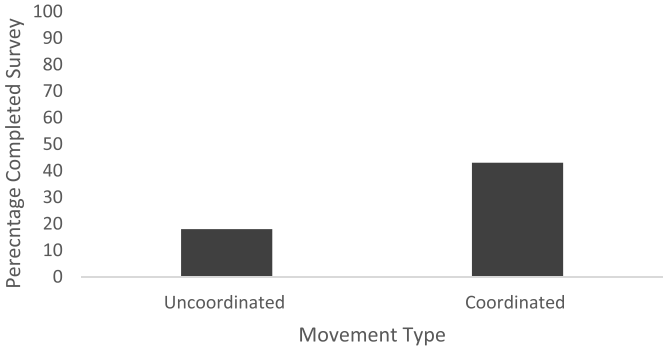


**Fig. 3.** The percentages of participants who followed through with their offer to help for study 2.

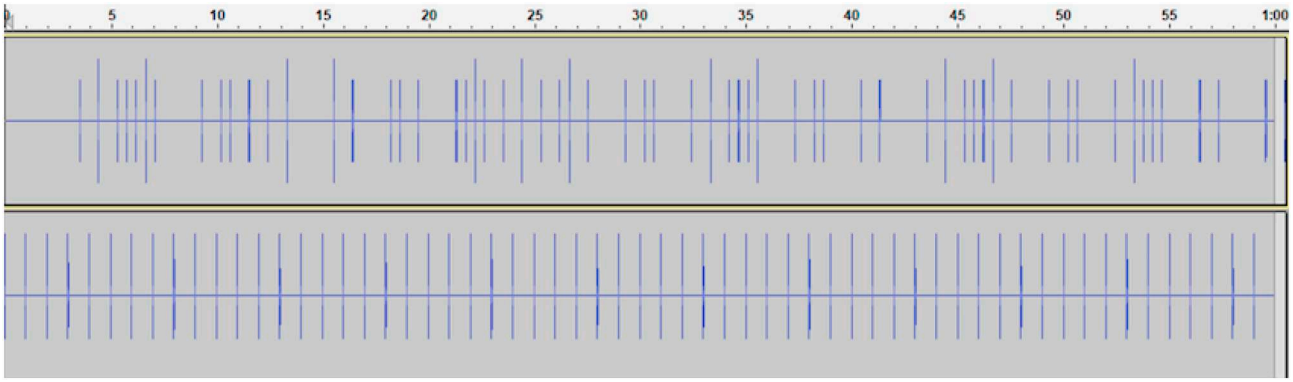


**Fig. 2.** Auditory stimuli for the tapping task, in the coordinated conditions both participant and experimenter heard and tapped to the bottom metronome, while in the uncoordinated condition the participants still heard and tapped to the bottom stimuli, while the experimenter heard and tapped to the top stimuli.

**Table 2**
Descriptive statistics for the affiliation and group categorisation questions form study 2.

| | | Affiliation | Self-categorisation item | Experimenter categorisation item |
|---|---|---|---|---|
| Uncoordinated | Mean | 38.93 | 22.93 | 42.6 |
| | SD | 29.15 | 26.65 | 32.08 |
| | SE | 5.32 | 4.87 | 5.86 |
| | Median | 37.0 | 17.5 | 35.0 |
| | Range | 0–100 | 0–100 | 0–100 |
| Coordinated | Mean | 39.93 | 38.33 | 60.43 |
| | SD | 24.63 | 28.25 | 25.178 |
| | SE | 4.5 | 5.16 | 4.6 |
| | Median | 41.5 | 35.5 | 65.5 |
| | Range | 0–100 | 0–100 | 0–100 |

the coordinated condition rated themselves as more like group members than unique individuals ($U = 606.5$, $p = .02$, $r = 0.3$) and the experimenter as being a part of the same group as themselves ($U = 592.5$, $p = .035$, $r = 0.27$) than those in the uncoordinated condition. Table 2 gives the descriptive statistics for each of those questions by condition.

There was a positive correlation between the two items measuring group categorisation ($r = 0.265$, $p = .041$) and between whether someone completed the survey and the Self-Experimenter categorisation question ($r = 0.278$, $p = .032$). The correlation between whether someone completed the survey and Self-categorisation question was only significant at the 0.1 level ($r = 0.229$, $p = .079$). The odds ratio shows that participants were 3.33 times more likely to complete the additional survey if they had participated in the coordinated condition than in the uncoordinated condition. Controlling for the self-categorisation question, however, reduced the odds ratio to 2.861. Controlling for the group-categorisation question reduced the odds ratio even more than the self-categorisation question to 2.53. Controlling for affiliation did not substantially decrease the odds ratio. In fact, the odds of someone completing the additional survey if they had participated in the coordinated condition rose slightly to 3.593 when controlling for affiliation.

In line with the findings of Study 1, Study 2 confirms that affiliation is not likely to be mediating the relationship between coordination and helping behaviour, while group level categorisation may be partially mediating this relationship, that is, both changes in how people see themselves in group vs individual terms, and seeing themselves and their previously coordinated co-actor as part of a common group.

## 3. General discussion

It has previously been hypothesised that throughout human history, moving in time with others has led to long-lasting bonding between co-actors and therefore strengthened group ties, leading to increased co-operation and helping amongst people (Brown, 2000; Hagen & Bryant, 2003; Huron, 2001; McNeil, 1995; Merker, 2000). The findings reported here provide empirical support for this hypothesis. Specifically, we demonstrate that after only a short, un-strenuous, five-minute period of rhythmic coordination, participants rated themselves and their co-actor in more common groups terms, they were also more likely to give time to help their co-actor 24 hours after the coordination period had taken place. This is a first step towards establishing the persistence of interpersonal coordination's pro-social effects over time.

Future work should aim to continue this line of inquiry to establish whether greater pro-sociality post coordination is maintained after even longer periods of time. It would also be of interest to establish whether the degree of pro-sociality differs as a function of such a lag. In particular, does pro-sociality post coordination decrease incrementally over time, or does it remain stable until a specific drop-off point in which effects begin to decay or are no longer observed? Such questions require incorporating memory models within the current embodied movement framework, which would help address the function of memory both during periods of coordination and at different time periods post-coordination.

Furthermore, this work provides additional weight to a growing body of literature which does not situate social affiliation as a mediator of the positive relation between coordination and other forms of pro-sociality such as cooperation and helping (i.e. Cross et al., 2016, 2017; Fessler & Holbrook, 2014, 2016; Lang et al., 2017; Reddish et al., 2013, 2016). Although it is possible that the sample sizes utilised here were unable to detect consitent affiliation effects, this is unlikely as similar sample sizes were used as previous work exploring coordination's effect on affiliation, a greater amount of which shows no stable effect of coordination on affiliation (Cross et al., 2016, 2017; Cross, Turgeon, & Atherton, 2019a, 2019b; Fessler & Holbrook, 2014, 2016; Lang et al., 2017; Reddish et al., 2013, 2016) than that which supports this link (Reddish et al., 2014; Wiltermuth, 2012; Wiltermuth & Heath, 2009).

Still, the pro-social effects following coordination do not appear to be a by-product of increased liking, closeness and similarity for a co-ordinated co-actor in the findings reported here. This is further exemplified by contrasting findings seen in the present experiments concerning affiliation: while study 1 revealed greater affiliation post coordination vs control, study 2 did not replicate this finding; on the other hand, greater helping post coordination (vs control) was observed across both studies. Instead, changes in pro-sociality, such as helping behaviour, appear more likely to be driven by changes in group-level evaluations (as has previously been suggested by Pearce et al., 2017). Similar arguments have also been made by Rabinowitch and Meltzoff (2017), Tunçgenç and Cohen (2016), and Wolf, Launay, and Dunbar (2016).

A further possibility, not examined here, is that the lower degree of pro-social motivation in the asynchronous condition compared to the synchronous condition may be driven in part by annoyance with the partner for failing to coordinate and instead producing distracting signals. There are two reasons, however, why we do not believe that this could fully explain our results. First, for study 1, the PLD was not active after the first 15 s of the trial in the uncoordinated condition (this was implemented in order to prevent interference, based on the findings reported in Cross et al., 2016). Secondly, there was no stable effect of affiliation across experiments, which should be expected if this explanation were correct.

Of course, it is possible that multiple mechanisms contribute to the effects of coordination upon pro-sociality, and that the relative contributions of different mechanisms may differ according to circumstances and from one individual to the next. As such, it would be valuable for future research to investigate individual differences in the underlying pro-social motivations in tasks like the ones employed here (Böckler, Tusche, & Singer, 2016; Peysakhovich, Nowak, & Rand, 2014). More generally, it would also be fruitful to extend the current findings to a broader sample of participants and to probe to what extent cultural and/or socioeconomic differences may play a role in mediating the effects of coordination upon pro-sociality. A further avenue for future research would be to explore changes in how participants view their own self-construal, specifically in independent/interdependent terms, as changes in self-construal have also been shown to mediate the relationship between mimicry, another form of interpersonal coordination, and pro-sociality (Ashton-James, Van Baaren, Chartrand, Decety, & Karremans, 2007).

Mimicry is a form of coordination that often involves complementary rather than synchronous actions with a co-actor (Chartrand & Bargh, 1999). Work by Cross, Wilson, and Golonka (2019) showed that people rated their identity items pertaining to gender, ethnicity, nationality and sports team allegiances as less important post coordinated movement. Work by Koudenburg, Postmes, Gordijn, and van Mourik Broekman (2015) has shown that both synchronous and

complementary coordination affected co-actors' sense of identity by producing feelings of common group membership. However, they found that complementary as opposed to synchronous coordination additionally led participants to experience a greater sense of personal value to the group. Thus, it would be valuable for future work to explore how complementary, in contrast to synchronous, movements affect social outcomes such as pro-sociality as well as the role of group identification.

In conclusion this work demonstrates that people report feeling less like individuals and more like group members after coordination, which may in turn be related to other pro-social effects of interpersonal coordination. Future work should explore changes in self-categorisation and self-construal alongside continuous measures of pro-sociality and other social consequences of coordination in order to further illuminate the effects of coordination upon prosocial motivation. This work also provides the first empirical evidence that the pro-social effects of a single coordination episode do persist after a temporal delay. A great deal of work has now shown that moving in time with each other can create a whole host of prosocial outcomes across those involved. This work demonstrates that these effects persist outside of the lab, after a delay, even after a relatively short coordination period. This is an important finding for work concerning the application of pro-social consequences of moving together, and strengthens the case for interpersonal coordination being employed as a tool to foster greater prosociality across individuals and perhaps even competing groups.

## Credit authorship contribution statement

**Liam Cross:** Conceptualization, Data curation, Formal analysis, Investigation, Methodology, Project administration, Resources, Software, Supervision, Validation, Visualization, Writing - original draft, Writing - review & editing. **John Michael:** Funding acquisition, Methodology, Validation, Writing - original draft, Writing - review & editing. **Luke Wilsdon:** Investigation, Project administration. **Agnes Henson:** Resources, Software, Data curation, Formal analysis. **Gray Atherton:** Conceptualization, Investigation, Methodology, Project administration, Resources, Software, Supervision, Validation, Writing - review & editing.

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