



Coordinating bodies and minds: Behavioral synchrony fosters mentalizing



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ARTICLE INFO

Keywords:

Synchrony
Interpersonal coordination
Mentalizing
Perspective taking
Mind perception

ABSTRACT

Behavioral synchrony, physically keeping together in time with others, is a widespread feature of human cultural practices. Emerging evidence suggests that the physical coordination involved in synchronizing one's behavior with another engages the cognitive systems involved in reasoning about others' mental states (i.e., mentalizing). In three experiments ($N = 959$), we demonstrate that physically moving in synchrony with others fosters some features of mentalizing – a core feature of human social cognition. In small groups, participants moved synchronously or asynchronously with others in a musical performance task. In Experiment 1, we found that synchrony, as compared to asynchrony, increased self-reported tendencies and abilities for considering others' mental states. In Experiment 2, we replicated this finding, but found that this effect did not extend to accuracy in mental state recognition. In Experiment 3, we tested synchrony's effects on diverse mentalizing measures and compared performance to both asynchrony and a no-movement control condition. Results indicated that synchrony decreased mental state attribution to socially non-relevant targets, and increased mental state attribution to specifically those with whom participants had synchronized. These results provide novel evidence for how synchrony, a common feature of cultural practices and day-to-day interpersonal coordination, shapes our sociality by engaging mentalizing capacities.

From army drills, prayer prostrations, gospel singing, daily calisthenics in large Japanese corporations, circling the Hajj, dancing the hora, to doing the wave at sporting events, collective cultural practices the world over, and throughout time, are often marked by the presence of some form of synchronized behavior – the act of keeping together in time with others.

Anthropologists have long hypothesized that synchronizing with others is an effective means by which to foster social bonds among unrelated individuals (e.g., Ehrenreich, 2006). McNeill (1995) even suggests that the synchronized army drill may very well be one of history's greatest military innovations for its effects of sustaining the tight bonds that enable groups of individuals to act as singular units. Ehrenreich (2006) and McNeill (1995) argue that synchrony in collective practices may have persisted in the cultural marketplace because of the social benefits it provides to groups. Accordingly, there is considerable experimental evidence that synchronizing behaviors with others, as compared to moving asynchronously (i.e., performing the same actions but at a different time) increases social cohesion and cooperation even in the laboratory, and out of a meaningful or culturally important context (e.g., Fischer, Callander, Reddish, & Bulbulia, 2013; Hove & Risen, 2009; Lakens & Stel, 2011; Miles, Nind, & Macrae, 2009; Valdesolo & DeSteno, 2011; Valdesolo, Ouyang, & DeSteno, 2010; Wiltermuth & Heath, 2009). Furthermore, synchronizing with

conspecifics can strengthen in-group affiliations early in development (Wen, Herrmann, & Legare, 2016) encouraging prosocial helping (Kirschner & Tomasello, 2010), even in infancy (Cirelli, Wan, & Trainor, 2014).

Although there is evidence that synchrony provides social benefits to groups, the precise mechanisms by which physically moving in time with others fosters cooperation and cohesion are still debated. In a review of the evidence, Rennung and Göritz (2016) suggest that other directed attention and self-other blurring may in part explain synchrony's effects on human sociality. Indeed, this self-other blurring may be a consequence of the simultaneous perception of others' actions and activation of the same neural systems in the perceiver that occurs when synchronizing with others (Knoblich & Sebanz, 2006, 2008). And in turn, this has been hypothesized to foster social connection through increased empathy and perspective taking (Kaplan & Iacoboni, 2006; Wheatley, Kang, Parkinson, & Looser, 2012). Here, we provide further rationale and direct tests of this hypothesis that synchrony enhances some aspects of mentalizing – the processes by which we infer and reason about the mental states of others (Baron-Cohen, Leslie, & Frith, 1985; Frith & Frith, 2006).

Mentalizing is a broad term that encompasses a suite of cognitive processes implicated in, for example, agency detection, gaze following, emotion processing, joint attention, and causal reasoning

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(Apperly & Butterfill, 2009). These processes can, with the right combination of cognitive resources and motivation (Converse, Lin, Keysar, & Epley, 2008; Lin, Keysar, & Epley, 2010), lead to the more explicit reasoning about others' affective and cognitive mental states more typically associated with the term (Frith & Frith, 2006). Thus, when we refer to mentalizing, we mean the broad overarching system involved in inferring and reasoning about the mental states of others.

1. Coordinating bodies and minds

Across the lifespan, the temporal coordination of behavior is implicated in the construction and navigation of the boundaries between self and other. Within the first year of life, infants follow the 'gaze' of amorphous blobs (i.e., otherwise non-social targets) when they behave contingently, suggesting that synchrony may be a cue to agency, and that our sensitivity to this cue develops early (Johnson, Slaughter, & Carey, 2000). Furthermore, 4-month olds use this information to inform future interactions and demonstrate preferences for previously socially-contingent others, even after a substantial time delay (Bigelow & Birch, 1999). Feldman (2007) hypothesized that parent-child synchronization scaffolds the development of children's capacities for intention reading and empathy and longitudinally demonstrated that synchrony in the first year of life positively predicted empathic capacities in adolescence. Thus, synchrony not only prompts parts of the mentalizing process (i.e., agency detection and gaze following) but actively contributes to its development.

Across the lifespan, synchrony is employed unconsciously in maintaining and establishing new social relationships. Individuals are more likely to spontaneously synchronize their movements with others they like (Miles, Griffiths, Richardson, & Macrae, 2010), but also to help bridge the psychological distance between members of minimal groups (Miles, Lumsden, Richardson, & Macrae, 2011). Interestingly, adolescents diagnosed along the Autism Spectrum – marked by reductions in mentalizing (Baron-Cohen et al., 1985) – naturally synchronize with others less than typically developing counterparts, and report greater difficulty with intentionally synchronizing their behaviors with others (Fitzpatrick et al., 2016). These results follow from the extensive literature exploring the social consequences of another form of interpersonal coordination – behavioral mimicry – in which mimicked behaviors are similar in form, but are not temporally bound as they are in synchrony. In reviewing the evidence, Chartrand and Lakin (2013) consistently implicate mentalizing as both a motivator and consequence of behavioral mimicry. For example, individuals with a greater propensity for perspective taking are more likely to mimic others' bodily and facial movements (Chartrand & Bargh, 1999). Furthermore, mimicking, and being mimicked, increases mentalizing accuracy (i.e., the ability to accurately estimate the mental states of others; Stel & Vonk, 2010). And similarly, Koehne, Hatzi, Cacioppo, and Dziobek (2016) demonstrated that the degree to which individuals perceived themselves to be synchronizing with another individual in a staged virtual interaction predicted the extent to which participants felt like they could understand the mental states of others. This is suggestive evidence that we synchronize, in part, to mentalize – to glean insights into others' mental states.

Synchronized collective cultural practices may pass on these benefits to individuals in addition to or, perhaps, as a consequence of fostering social cohesion. Indeed, the proposed mechanisms by which synchrony fosters social cohesion – other directed attention and self-other blurring (Rennung & Göritz, 2016) – are also conditions that foster mentalizing. Individuals are prone to both perceiving and attributing mind where there is none (e.g., to electronic gadgets; Waytz, Epley, & Cacioppo, 2010), and failing to acknowledge and attribute mental states where they certainly exist (e.g., in outgroup members; Harris & Fiske, 2006). But, mind perception and mental state reasoning is more frequent among individuals who seek to connect and coordinate with each other (Waytz, Gray, Epley, & Wegner, 2010). Thus, by

focusing one's social attention on their interaction partners (Macrae, Duffy, Miles, & Lawrence, 2008), synchrony may also increase the likelihood that individuals perceive and engage with others' mental states. Furthermore, the accurate perception of a mind does not necessarily result in the accurate estimation of its contents as accurate reasoning about others' mental states is often biased by one's egocentric perspective (Birch, 2005; Epley, Morewedge, & Keysar, 2004). And thus, by blurring the boundaries between self and other, and reducing egocentrism, synchrony may also foster more accurate mental state reasoning.

As reviewed, there are a number of ways in which mentalizing has been operationalized in the literature, with the unfortunate consequence of only sparse work exploring the overlap among them (Lindeman & Lipsanen, 2017; and see Schaafsma, Pfaff, Spunt, & Adolphs, 2015 for a discussion). We contend that there is also an important distinction between mentalizing propensity and mentalizing accuracy – which we argue, are not always clearly, or easily, disambiguated in measures of mentalizing. In the following experiments, we employed a diverse set of measures to assess synchrony's effects in fostering different aspects of mentalizing, including whether these hypothesized effects are specific to propensity or accuracy and whether the targets of mentalizing are socially relevant or not.

2. The current research

In three experiments, we investigated if, and in what ways, synchrony enhances mentalizing. In *Experiment 1*, we examined whether synchronizing with other individuals increased participant's self-reported propensities for considering the mental states of other people in general. In *Experiment 2* we tested the replicability of these initial findings, and whether synchrony would also increase mentalizing accuracy, specifically in emotion recognition from pictures of eyes. *Experiment 3* had two main goals. First, it included a baseline, no movement control condition in addition to the synchrony and asynchrony conditions, to isolate with more precision the source of the effects. That is, we assessed, relative to control, whether synchrony fostered mentalizing, or rather that mentalizing was disrupted by an asynchronous interaction. Second, it probed an increasingly specific set of mentalizing measures to directly test two possible pathways by which synchrony might foster mentalizing: (1) by directing attention to socially relevant minds in one's immediate environment, and (2) by decreasing egocentric biases. *Experiments 2* and *3* also assessed the effects of synchrony on feelings of social cohesion in order to examine whether any observed differences in mentalizing were explained by increases in social cohesion. All study materials, data, and analyses scripts are available at: osf.io/5xmb2. All measures, manipulations and exclusions are fully disclosed in this article. No additional data was collected post-data analysis.

3. Experiment 1

In this first experiment, we investigated whether participation in a synchronized task in the lab would increase self-reported mentalizing. Specifically, we manipulated whether participants moved and sang in or out of synchrony with others in a musical performance task and then measured responses on the Empathy Quotient (EQ; Baron-Cohen & Wheelwright, 2004) – a self-report measure of mentalizing propensities. We hypothesized that EQ scores would be higher, overall, in the synchrony as compared to the asynchrony condition.

4. Methods

4.1. Participants

One-hundred and sixteen (83 females) undergraduate students at a Canadian university completed this study in exchange for course credit.

Power analysis (conducted in G*Power; Faul, Erdfelder, Lang, & Buchner, 2007) indicated that this sample size should be powered (80%) to detect effect sizes of $d = 0.52$ and larger, such as previously reported effects of synchrony on cooperation ($F(1, 28) = 4.33, p = .04, d = 0.74$; Study 2 in Wiltermuth & Heath, 2009).

Participants ranged in age from 17 to 35 ($M = 20.07, SD = 2.61$), were predominantly Asian (62%), Caucasian (33%) or of mixed cultural heritage (5%; see Table S1 for the ethnic composition of the groups across experiments). Participants were randomly assigned to either the synchronous ($n = 54$ in 16 groups of 3, 3 groups of 2) or asynchronous condition ($n = 62$ in 20 groups of 3, 1 group of 2). The distribution of male and female ($\chi^2(1) = 0.01, p = .92$), and Asian and Non-Asian participants ($\chi^2(1) = 0.03, p = .85$) was independent of condition. An additional two groups of three participants were excluded from the synchronous condition, and from all analyses, due to experimenter error in instructing participants on how to perform the task.

4.2. Materials and procedure

Participants were recruited in groups of three (due to no-shows, 4 groups were composed of 2 participants). Upon arrival, the experimenter asked participants if they knew each other beyond recognizing each other from their courses and no participants indicated that they did. They were then seated around a table and told that they would be participating in two tasks - a musical performance task, followed by a questionnaire.

4.2.1. Musical performance task

The musical performance task was the focal manipulation of synchrony in this study, and was adapted from Wiltermuth & Heath (2009; Studies 2 and 3). In this task, participants were taught how to move three plastic cups back and forth across a table in a specific manner to the beat of some music. Wiltermuth and Heath (2009) had American participants move the cups and sing to the Canadian national anthem, and in an attempt to closely replicate their procedure, we had this Canadian sample move and sing to the American national anthem. Crucially, and unbeknownst to the participants, the group had been randomly assigned to either the synchronous or asynchronous condition. In the synchronous condition, participants' headphones were connected to a single MP3 player. Thus, all participants in the synchronous condition listened to the same, standard 128 beats-per-minute (BPM) version of the anthem. As such, when keeping to the beat they heard over their own headphones, participants synchronized in both their movements of the cups and the singing of the lyrics with others in this condition. In the asynchronous condition, participants were connected to individual MP3 players that were loaded with different

versions of the anthem: 128 BPM (standard), 90 BPM (slower) or 165 BPM (faster). As such, when keeping to the beat that they heard over their headphones, participants did not synchronize with each other in either their movements of the cups or the singing of the lyrics. The manipulation lasted for approximately 3 min. For full methodological details of this task see supplemental S.2.1.

Following the musical performance task, participants responded to the Empathy Quotient (EQ; Baron-Cohen & Wheelwright, 2004). This 40-item self-report measure assesses cognitive and affective features of individual propensities in mentalizing, as well as general social skills, as measured either by a single total score or three subscales (Lawrence, Shaw, Baker, Baron-Cohen, & David, 2004): (1) Cognitive empathy, or perspective taking, measures the tendency to engage with, and efficiency at predicting the, mental states of others (e.g., "I am good at predicting how someone will feel"); (2) Affective empathy measures one's tendency to emotionally react to and engage with the mental states of others (e.g., "Seeing people cry doesn't really upset me"); (3) Social skills (e.g., "I find it hard to know what to do in a social situation").

After the EQ, participants reported their age, sex, and cultural background as all of these demographics have been previously implicated in accounting for individual differences in mentalizing (eg., see Baron-Cohen (2009) for discussion of sex differences; see Bernstein, Thornton, & Sommerville (2011) for discussion of age-related differences; and see Willard & Norenzayan, 2013 for a discussion of cultural differences in a comparable sample).

5. Results and discussion

Empathy Quotient scores were higher in the synchronous condition ($M = 46.00, SD = 9.86$) than in the asynchronous condition ($M = 42.21, SD = 10.45$); $t(113.26) = -2.01, p = .047, d = 0.37, .95CI = [0.00, 0.74]$. EQ scores were then analyzed using a random-intercept linear regression model with participant group as a random effect (following Garson, 2013; for analytical rationale see supplemental S.1). This random effect structure matched the small groups methods employed across studies. Thus, even if the random effect of group was small, we retained this random effect structure across all models to account for the nested structure of the data. Coefficients from all mixed-effect models presented across experiments can be interpreted as would be standardized regression coefficients. In this model, the effect of condition on EQ ($b = 0.40, .95CI = [0.05, 0.75], p = .03$) was robust to controlling for demographic influences (i.e., age, sex, participant ethnicity, and ethnic homogeneity of the group) on mentalizing (see Table 1). We then similarly modeled the EQ subscales, and found that this effect was specific to cognitive empathy ($b = 0.38, .95CI =$

Table 1
Random-intercept linear regression models predicting Empathy Quotient.

	Empathy quotient			Cognitive empathy			Affective empathy			Social skills		
	<i>b</i> [.95 CI]	<i>t</i>	<i>p</i>									
Intercept	0.47 [0.12, 0.82]	2.60	.01	0.44 [0.09, 0.79]	2.46	.02	0.32 [-0.05, 0.70]	1.68	.10	0.46 [0.10, 0.83]	2.48	.01
Condition (1 = Synchronous)	0.40 [0.05, 0.75]	2.25	.03	0.38 [0.04, 0.72]	2.17	.04	0.26 [-0.11, 0.63]	1.40	.17	0.27 [-0.08, 0.62]	1.50	.14
Age (years)	0.02 [-0.16, 0.19]	0.22	.83	0.01 [-0.17, 0.18]	0.06	.95	0.04 [-0.14, 0.23]	0.46	.65	-0.02 [-0.20, 0.16]	-0.25	.81
Male	-0.38 [-0.76, -0.01]	-2.02	.045	-0.65 [-1.02, -0.28]	-3.43	.001	-0.03 [-0.43, 0.37]	-0.15	.88	0.03 [-0.36, 0.41]	0.14	.89
Asian	-0.89 [-1.30, -0.47]	-4.19	< .001	-0.70 [-1.12, -0.28]	-3.28	.002	-0.71 [-1.15, -0.26]	-3.11	.003	-0.96 [-1.40, -0.52]	-4.29	< .001
Asian group	0.32 [0.12, 0.52]	3.08	.003	0.26 [0.06, 0.46]	2.57	.01	0.32 [0.11, 0.54]	2.95	.004	0.24 [0.03, 0.45]	2.28	.02
AICc	324.11			323.57			337.90			332.31		
Mean VIF	1.21			1.23			1.22			1.23		
R ² total	.26			0.24			0.15			0.16		
R ² fixed effects	.22			0.22			0.12			0.16		

[0.04, 0.72], $p = .04$; see Table 1). For discussion of the controls and model fit indices see supplemental S.2.2; and for discussion of the influence of a minimal outlier in this first experiment see supplemental S.5.1.

In support of our hypothesis, the results from Experiment 1 suggest synchronizing with others fosters mentalizing, as measured by the EQ. Although we made no a-priori hypotheses regarding the EQ's subscales, we found that this effect was specific to cognitive empathy. We note that this effect was found even though a trait measure of mentalizing was used. In Experiments 2B and 3, we employed additional mentalizing measures to assess the specificity and generalizability of this observed effect of synchrony on mentalizing beyond the EQ. We discuss these issues in detail in the General Discussion.

5.1. Experiments 2A & 2B

Experiments 2A and 2B were designed to replicate and extend our initial findings. Specifically, we collected measures of social cohesion in addition to the EQ. This allowed us to explore whether mentalizing mediated the effects of synchrony on social cohesion, and/or whether social cohesion mediated the effects of synchrony on mentalizing. Following the manipulation in Experiment 2B but prior to the EQ, participants responded to a state measure of accuracy in mental state reasoning in the form of a measure of emotion recognition – the Reading the Mind in the Eyes Test (Baron-Cohen, Wheelwright, Hill, Raste, & Plumb, 2001).

We hypothesized that: (1) EQ scores would be higher in the synchrony condition, (2) that this effect would be specific to the cognitive empathy subscale, and that (3) mentalizing accuracy would be higher in the synchrony condition. Furthermore, we hypothesized (4) that reported social cohesion would be greater in the synchrony condition than in the asynchrony condition. Lastly, we planned to explore whether or not any observed effects on mentalizing were causes, consequences, or independent of any observed increases in social cohesion.

6. Methods

6.1. Participants

Experiment 1 provided a first estimate of the effect of synchrony on mentalizing. Thus, we used the standardized between condition difference in EQ scores observed in Experiment 1 ($d = 0.37$, $.95CI = [0.00, 0.74]$, $t(113.26) = 2.01$, $p = .047$) rather than previously reported effect size estimates from the broader synchrony literature to calculate the suggested minimum necessary sample size for this replication. Power analysis (conducted in G*Power; Faul et al., 2007) suggested a minimum of 116 participants per condition to achieve 80% power to detect a similarly sized effect if it exists on the EQ as observed in Experiment 1.

Participants ($N = 296$; 222 females) were undergraduate students at a Canadian university who participated in exchange for course credit, and ranged in age from 18 to 42 ($M = 20.64$, $SD = 2.66$), and self-identified as Asian (60%), Caucasian (33%), or other (7%). Participants were randomly assigned to either the synchronous ($n = 144$, 41 groups) or asynchronous condition ($n = 152$, 42 groups). The distribution of male and female ($\chi^2(1) = 1.15$, $p = .28$), and Asian and Non-Asian participants ($\chi^2(1) = 2.69$, $p = .10$) was independent of condition. Groups were randomly assigned to the two versions of this experiment. In Experiment 2A (EQ Only; $n = 149$) participants responded only to the EQ. In Experiment 2B (Eyes Test & EQ; $n = 147$), participants first responded to the 'Reading the Mind in the Eyes' Test (Baron-Cohen et al., 2001), followed by the EQ. We note that as fewer participants completed the Eyes Test, we were only statistically powered (80%) to detect a larger effect of $d = 0.47$ or greater on this measure of mentalizing accuracy. These study versions were run separately rather than counterbalancing the questionnaires to avoid

potential dampening of effects from the time-delay between the manipulation and the social cohesion measures.

6.2. Materials and procedure

The musical performance task was modified such that instead of listening to the American national anthem, participants listened to a three-quarter measure metronome beat that was preloaded onto four MP3 players at 65, 90, 128, and 165 BPM; thus removing the singing portion of the task. This allowed us to test for the unique effects of synchronous movement on mentalizing. Participants were recruited in groups of three or four (see supplemental S.3.1 for further details).

6.2.1. Reading the mind in the eyes test

The 'Reading the Mind in the Eyes Test' (Eyes Test; Baron-Cohen et al., 2001) is a 36-item state measure of accuracy in emotion recognition that asks participants to match mental states terms to pictures of eyes. The items can be classified into two categories of mental states – 'thinking' (e.g., fantasizing, suspicious, reflective) and 'feeling' (e.g., uneasy, worried, hostile). The EQ and the Eyes Test were designed to capture distinct facets of mental state reasoning and are not significantly correlated in the general population (Baron-Cohen et al., 2015).

6.2.2. Social cohesion

Participants then completed three measures of perceived group cohesion. These included a 4-item measure of 'Relational Ties' (Gómez et al., 2011) assessing the extent to which participants shared a connection with their group (e.g., "Do you feel like you know any of the other participants very well?"). Group fusion (Swann Jr., Swann, Gómez, Conor, Francisco, & Huici, 2009) – the extent to which the self was subsumed, or 'fused' with that of the group – was assessed using a picture of increasingly overlapping circles. Lastly, an 8-item Group Identification measure assessed the extent to which individuals felt committed to their group (e.g., "How much do you feel you belong to the group?"; Hogg, Sherman, Dierselhuis, Maitner, & Moffitt, 2007). After these measures, participants filled out the demographics as in Experiment 1.

7. Results and discussion

EQ scores were higher in the synchronous condition ($M = 46.38$, $SD = 9.84$) than in the asynchronous condition ($M = 41.58$, $SD = 8.91$); $t(284.96) = -4.36$, $p < .001$, $d = 0.51$, $.95CI = [0.28, 0.74]$. On the Eyes Test, scores did not significantly differ between the synchronous ($M = 25.50$, $SD = 3.65$) and asynchronous ($M = 24.97$, $SD = 3.75$) conditions; $t(144.97) = -0.86$, $p = .39$, $d = 0.14$, $.95CI = [-0.18, 0.47]$. These two measures were not significantly correlated, $r(140) = 0.11$, $p = .19$ (see Table S2 for additional correlations).

In a random-intercept linear regression, the effect of condition on EQ ($b = 0.43$, $.95CI = [0.18, 0.67]$, $p < .001$) was robust to controlling for demographic and participant group differences as in Experiment 1, and to the additional controls for social cohesion (see Table 2; and for discussion of controls see supplemental S.3.2). In an additional model, we found that group identification marginally moderated the effect of condition on EQ (interaction $b = 0.21$, $.95CI = [-0.01, 0.42]$, $p = .06$). When controlling for this interaction, the main effect of condition ($b = 0.42$, $.95CI = [0.18, 0.66]$, $p = .001$) was significant, but that of group identification was not ($b = 0.07$, $.95CI = [-0.10, 0.24]$, $p = .42$). The inclusion of this interaction marginally improved model fit ($\chi^2(1) = 3.66$, $p = .06$; Interaction model R^2 total = 0.27, R^2 fixed effects = 0.20, mean VIF = 1.40). However, corrected-AIC estimates (AICc), which adjusts for the number of parameters in a model, indicated this was a worse fitting model ($\Delta AICc = 1.22$). Thus, we make no strong conclusions about this

Table 2
Random intercept linear regression models predicting Empathy Quotient subscales in Experiments 2A and 2B.

	Empathy quotient			Cognitive empathy			Affective empathy			Social skills		
	b [.95 CI]	t	p	b [.95 CI]	t	p	b [.95 CI]	t	p	b [.95 CI]	t	p
Intercept	0.02 [-0.24, 0.28]	0.16	.87	-0.04 [-0.29, 0.22]	-0.29	.77	0.05 [-0.23, 0.33]	0.35	.73	0.34 [0.08, 0.60]	2.58	.01
Condition (1 = Synchronous)	0.43 [0.18, 0.67]	3.47	.001	0.46 [0.23, 0.68]	3.91	< .001	0.13 [-0.13, 0.39]	0.96	.34	0.16 [-0.07, 0.38]	1.37	.17
Age	-0.01 [-0.12, 0.10]	-0.20	.84	-0.04 [-0.14, 0.07]	-0.72	.47	0.03 [-0.09, 0.14]	0.47	.64	-0.09 [-0.21, 0.02]	-1.64	.10
Male	-0.37 [-0.61, -0.12]	-2.96	.003	-0.61 [-0.85, -0.37]	-4.94	< .001	-0.01 [-0.27, 0.25]	-0.09	.93	-0.08 [-0.33, 0.17]	-0.60	.55
Asian	-0.35 [-0.61, -0.10]	-2.73	.007	-0.15 [-0.40, 0.11]	-1.17	.24	-0.29 [-0.56, -0.02]	-2.13	.03	-0.62 [-0.89, -0.35]	-4.48	< .001
Relational Ties	0.04 [-0.09, 0.16]	0.58	.56	-0.09 [-0.21, 0.03]	-1.51	.13	0.17 [0.04, 0.30]	2.52	.01	0.04 [-0.08, 0.17]	0.67	.50
Fusion	-0.07 [-0.19, 0.06]	-1.02	.31	-0.04 [-0.16, 0.09]	-0.63	.53	-0.13 [-0.26, 0.01]	-1.85	.07	-0.03 [-0.16, 0.10]	-0.51	.61
Group Identification	0.17 [0.04, 0.30]	2.53	.01	0.17 [0.00, 0.33]	1.98	.05	-0.07 [-0.25, 0.12]	-0.71	.48	-0.11 [-0.28, 0.07]	-1.21	.23
Exp. Version (1 = Exp. 2B)	0.16 [-0.08, 0.39]	1.29	.20	0.12 [-0.10, 0.35]	1.05	.30	0.15 [-0.11, 0.40]	1.12	.26	-0.05 [-0.27, 0.17]	-0.42	.67
Asian Group	-0.11 [-0.25, 0.03]	-1.58	.12	-0.16 [-0.30, -0.03]	-2.41	.02	-0.08 [-0.23, 0.07]	-1.10	.27	-0.03 [-0.16, 0.11]	-0.40	.69
Condition*Group Identification	-	-	-	0.05 [-0.17, 0.26]	0.42	.68	0.18 [-0.05, 0.41]	1.53	.13	0.35 [0.13, 0.57]	3.10	.002
AICc	788.78	-	-	783.03	-	-	827.27	-	-	807.71	-	-
Mean VIF	1.23	-	-	1.41	-	-	1.40	-	-	1.43	-	-
R ² Total	.25	-	-	0.26	-	-	0.17	-	-	0.15	-	-
R ² Fixed Effects	.19	-	-	0.22	-	-	0.08	-	-	0.15	-	-

Table 3
Social cohesion measures by condition in Experiment 2.

	Relational ties M (SD)	Group identification M (SD)	Fusion M (SD)
Asynchronous	0.67 (0.89)	4.11 (1.67)	2.91 (1.33)
Synchronous	0.70 (1.04)	4.08 (1.64)	2.92 (1.44)
Test of difference	$t(282.24)$ = -0.25, $p = .80$ $d = -0.03$, .95CI = [-0.26, 0.20]	$t(292.75) = 0.14$, $p = .89$ $d = 0.02$, .95CI = [-0.21, 0.25]	$t(285.17)$ = -0.05, $p = .96$ $d = -0.01$, .95CI = [-0.24, 0.22]

interaction, but include it in subsequent models. Modelling of the Eyes Test with controls did not change our inferences (see supplemental S.3.3).

We then modeled the EQ subscales, and found that condition differences were specific to cognitive empathy (see Table 2). In these models, we found that the moderating effect of group identification was specific to the social skills subscale of the EQ. Thus, it is plausible that this reflects the comparable social nature of the items and not a specific effect on mentalizing. Experiment 2 results were not affected by outliers (see supplemental S.5.2).

We did not find that synchrony increased perceived social cohesion. There were no significant differences in social cohesion between conditions (see Table 3). This suggests that the observed condition differences in mentalizing were not a byproduct of increased social cohesion, but rather a direct effect of the manipulation. As the manipulation did not produce differences in cohesion – we could not test whether synchrony induced mentalizing mediated the effects of synchrony on cohesion or the reversed paths.

In summary, we replicated the key results of Experiment 1. In support of two of our hypotheses, (1) self-reported mentalizing propensities were higher in the synchronous condition, and (2) this difference was specific to cognitive empathy. We did not find that (3) mentalizing accuracy, as measured with the Eyes Test, was greater in the synchronous condition. However, a limitation of the Eyes Test, even as a state measure of mentalizing, is that it measures accuracy in detecting the mental states of unknown others and not those that the participants had specifically synchronized with. Furthermore, (4) we did not observe differences in social cohesion between conditions, therefore social cohesion cannot explain the effect of synchrony on mentalizing. To extend these results, we included in Experiment 3 a broader array of state and trait mentalizing measures that allowed us to assess the scope of the effect of synchrony on mentalizing that also varied in the degree of social relevance of their targets.

7.1. Experiment 3

Increasing the accuracy in mental state perception, and/or decreasing the egocentrism that hinders accurate mental state estimation may be two complimentary mechanisms by which synchrony may foster mentalizing (see Baimel, Severson, Baron, & Birch, 2015 for a discussion). In this third experiment, we tested whether synchrony increases mental state attribution broadly (including to non-human targets and social outgroups) or narrowly (to socially-relevant targets), and whether synchrony suppresses egocentric biases when reasoning about others' knowledge. We hypothesized that synchrony would narrowly increase mental state attribution, and not broadly. We also tested whether the effects on the Empathy Quotient would generalize to a different, but conceptually similar, self-report measure of mentalizing tendencies. We again assessed social cohesion. Finally, and importantly, a true no-movement control condition was included to serve as a baseline for distinguishing the effects of synchrony on mentalizing from those of asynchrony. This is crucial, as the extant synchrony literature is

quite variable in its use of contrasting control conditions (as noted elsewhere; e.g., Tarr, Launay, & Dunbar, 2016).

8. Methods

8.1. Participants

In a power analysis (conducted in G*Power; Faul et al., 2007), we used the lower bound estimate of the standardized mean difference on total EQ scores between the synchronous and asynchronous conditions observed in our data when collapsed across Experiments 1 and 2 to calculate the necessary sample size for our third experiment ($d = 0.47$, $.95\text{CI} = [0.27, 0.67]$, $t(400.95) = 4.74$, $p < .001$). Sample size planning was conducted based on this lower-bound effect size as it was our best estimate, given the results of the previous experiments, of the minimum direct effect of synchrony on mentalizing. This analysis suggested that to achieve expected power (80%), we would need a minimum of 217 participants per condition to detect an effect of this size.

Five hundred and forty-seven (429 females) Canadian undergraduate students completed this experiment in exchange for course credit. Participants ranged in age from 18 to 46 ($M = 20.81$, $SD = 2.55$) and self-identified as Asian (62%), Caucasian (30%), or other (8%). Participants in this experiment were randomly assigned to the synchronous ($n = 218$, 72 groups), asynchronous ($n = 226$, 72 groups), or control condition ($n = 103$, 28 groups). The control condition was added to the random assignment of groups halfway through data collection. Thus, contrasts with the control condition are powered (80%) to detect a slightly larger effect size ($d = 0.33$ and larger), than comparisons between the other conditions ($d = 0.27$ and larger). Fifteen additional participants were excluded from all analyses due to experimenter error ($n = 6$), technical problems ($n = 3$), and failing to complete the study ($n = 6$). The distribution of male and female ($\chi^2(2) = 0.81$, $p = .67$), and Asian and Non-Asian participants ($\chi^2(2) = 2.75$, $p = .25$) was independent of condition.

8.2. Materials

The breadth of participants' mental state attribution was assessed with a measure of (1) anthropomorphism (i.e., mental state attribution to non-human targets; Waytz, Cacioppo, & Epley, 2014); (2) mental state attribution to participants' group members (adapted from Gray, Gray, & Wegner, 2007), and (3) blatant dehumanization (i.e., mental state attribution to social groups; Kteily, Bruneau, Waytz, & Cotterill, 2015). The extent to which participants' estimates of others' knowledge was biased by their own egocentric perspective was assessed using (4) the Curse of Knowledge trivia game (Birch, Brosseau-Liard, Bernstein, Haddock, & Ghrear, 2017). To test whether the previously reported effects generalized to a conceptually similar self-report measure of mentalizing tendencies we included (5) the Interpersonal Reactivity Index (Davis, 1983). And (6), we included a single measure of social cohesion adapted from the items used in the previous experiments (for further discussion of these measures see supplemental S.4.1).

8.3. Procedure

Participants were recruited in groups of three or four and completed the same musical performance task as described in Experiment 2. Due to no-shows, some groups consisted of only two participants. Given the combined length of the target measures, and the lack of evidence regarding the duration of the effects of synchrony in the lab, participants took part in two rounds of the musical performance task. The order of the questionnaires following the rounds of the manipulation was blocked based on the length of time needed to complete them, and was randomized across participants (eight versions in total).

Each group was randomly assigned to the synchronous, asynchronous or a no-movement control condition. In lieu of the first round of

the musical performance task, participants in the control condition were seated at the main table for three minutes and were told that they could talk to each other if they wanted to. After this unstructured interaction (or the first round of the musical performance task in the other conditions), participants were moved to individual computer stations where they responded to the first set of questionnaires. When finished, participants returned to the main table. Participants then completed the second musical performance task in the same condition as the first round. In the control condition, participants regrouped for another three minutes of unstructured interaction, but were asked not to discuss the contents of the questionnaires. Participants then returned to the individual stations to complete the second set of questionnaires, and demographics.

9. Results

Scores across dependent variables did not differ based on survey order or group size (see supplemental S.4.2). Thus, data were collapsed across these variables for all analyses. Given the number of dependent variables employed in this experiment, the critical alpha-level for statistical inferences was Bonferroni adjusted to 0.01. The correlations between mentalizing measures and social cohesion are presented in Table S5.

(1) Anthropomorphism

In a linear regression model predicting IDAQ from condition ($F(2, 531) = 18.22$, $p < .001$, $R_{adj}^2 = 0.06$), we found significant differences between conditions: Compared to control ($M = 4.64$, $SD = 1.51$; b intercept = 0.17, $.99\text{CI} = [-0.08, 0.42]$, $t(531) = 1.78$, $p = .08$), scores in the asynchronous condition did not differ ($M = 4.71$, $SD = 1.59$; $b = 0.04$, $.99\text{CI} = [-0.25, 0.34]$, $t(531) = 0.39$, $p = .70$), but scores in the synchronous condition were lower than control ($M = 3.88$, $SD = 1.42$; $b = -0.49$, $.99\text{CI} = [-0.79, -0.19]$, $t(531) = -4.18$, $p < .001$). Post-hoc Tukey contrasts indicated that scores in the asynchronous condition were higher than in the synchronous condition ($b = 0.53$, $.99\text{CI} = [0.26, 0.81]$, $t(531) = 5.70$, $p < .001$). Thus, mental state attribution to socially irrelevant non-human targets was *lowest* in the synchronous condition (see Fig. 1, Panel A). In a random-intercept linear regression model, this effect was robust to controlling for demographics and social cohesion (see Table 4). Again, post-hoc Tukey contrasts indicated that the IDAQ scores in the asynchronous condition were higher than in the synchronous condition ($b = 0.54$, $.99\text{CI} = [0.24, 0.83]$, $z = 5.30$, $p < .001$). The full model with fixed effects improved model fit compared to a random-effect only model ($\Delta\text{AICc} = -4.73$; $\chi^2(7) = 42.63$, $p < .001$). These results were not affected by outliers (see supplemental S.5.4).

(2) Mental state attribution

In a linear regression model predicting mental state attribution from condition ($F(2, 520) = 11.28$, $p < .001$, $R_{adj}^2 = 0.04$), we found significant differences between conditions: Compared to control ($M = 5.58$, $SD = 1.11$; b intercept = -0.27 , $.99\text{CI} = [-0.55, 0.01]$, $t(520) = -2.54$, $p = .01$), mental state attribution in the asynchronous condition was not different ($M = 5.72$, $SD = 0.98$; $b = 0.14$, $.99\text{CI} = [-0.18, 0.47]$, $t(520) = 1.15$, $p = .25$), but mental state attribution in the synchronous condition was greater than control ($M = 6.07$, $SD = 0.83$; $b = 0.51$, $.99\text{CI} = [0.18, 0.83]$, $t(520) = 4.02$, $p < .001$). Post-hoc Tukey contrasts revealed that scores in the asynchronous condition were lower than in the synchronous condition ($b = -0.36$, $.99\text{CI} = [-0.63, -0.09]$, $t(520) = -3.86$, $p < .001$). Thus, mental state attribution to socially-relevant human targets was greatest in the synchronous condition (see Fig. 1, Panel B). This effect was robust to controlling for demographics and social cohesion in a random-intercept linear regression model (see Table 4). Again, post-hoc Tukey contrasts

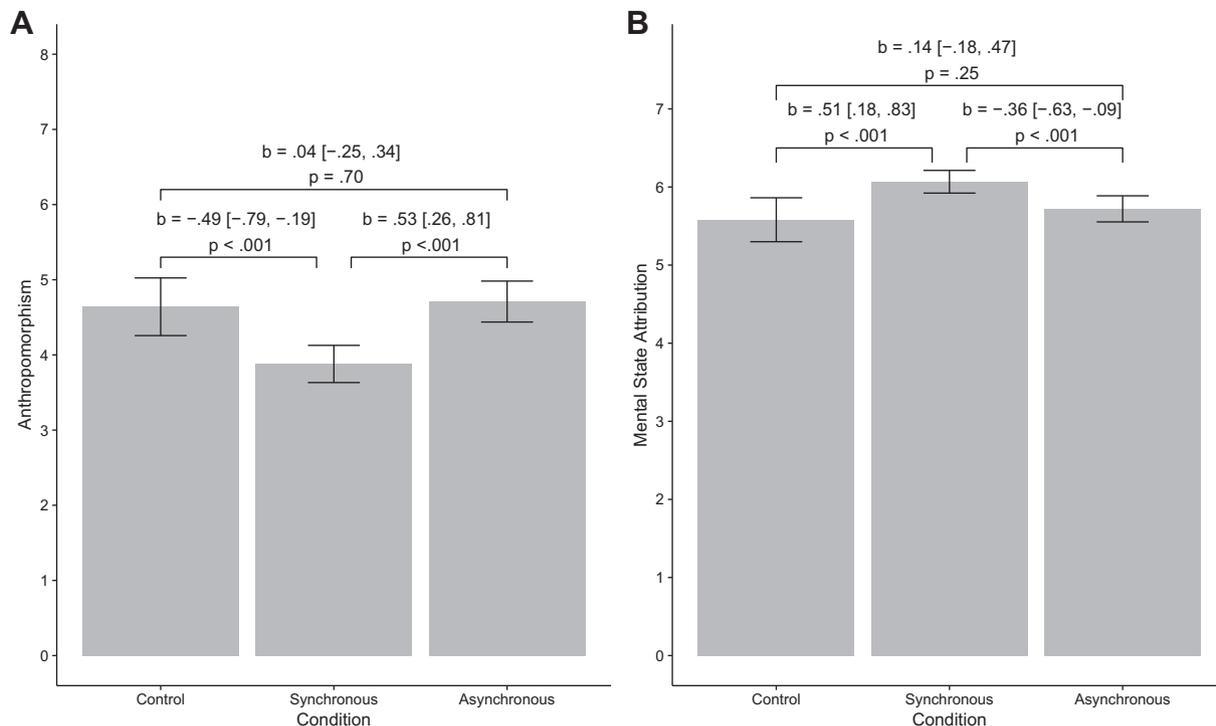


Fig. 1. Anthropomorphism and Mental State Attribution by Condition in Experiment 3.

Notes: Effect sizes are standardized regression coefficients [99% confidence intervals] from a linear regression model predicting mentalizing from condition without controls. Errors bars are 99% confidence intervals around the condition mean.

in the random intercept model revealed that scores in the asynchronous condition were lower than in the synchronous condition ($b = -0.34$, $.99\text{CI} = [-0.67, -0.01]$, $z = -2.95$, $p = .01$). The full model with fixed effects improved model fit compared to a random-effect only null model ($\Delta\text{AICc} = -23.36$; $\chi^2(7) = 61.25$, $p < .001$). These results were not affected by outliers (see supplemental S.5.4).

In linear regression models contrasting (3) Blatant Dehumanization, (4) Curse of Knowledge, and (5) the Interpersonal Reactivity Index scores in the control condition to those in the synchronous and asynchronous conditions, we found no significant differences (see Table 5). Random-intercept linear modelling of these variables did not change our inferences (see Table S6). Post-hoc Tukey contrasts in all models revealed no significant differences between the synchronous and asynchronous conditions (see supplemental S.4.3). Furthermore, we found no differences between conditions on the subscales of the IRI (see Table S7).

Table 4
Random-intercept linear regression models predicting Anthropomorphism and Mental State Attribution in Experiment 3.

	Anthropomorphism			Mental State Attribution			
	<i>b</i> [.99 CI]	<i>t</i>	<i>p</i>	<i>b</i> [.99 CI]	<i>t</i>	<i>p</i>	<i>p</i>
Condition (Ref = Control)							
Synchronous	−0.39 [−0.75, −0.03]	−2.80	.01	0.42 [0.01, 0.83]	2.64	.01	
Asynchronous	0.15 [−0.21, 0.50]	1.04	.30	0.08 [−0.33, 0.49]	0.51	.61	
Age	0.00 [−0.12, 0.11]	−0.11	.92	−0.04 [−0.15, 0.06]	−1.04	.30	
Male	−0.26 [−0.54, −0.02]	−2.39	.02	0.06 [−0.21, 0.33]	0.57	.57	
Asian	0.33 [0.05, 0.61]	2.99	.003	−0.23 [−0.48, 0.02]	−2.40	.02	
Social Cohesion	0.06 [−0.05, 0.18]	1.37	.17	0.26 [0.15, 0.37]	6.41	< .001	
Asian Group	−0.11 [−0.25, 0.04]	−1.95	.05	−0.03 [−0.18, 0.12]	−0.55	.58	
Intercept	−0.06 [−0.42, 0.30]	−0.42	.68	−0.08 [−0.47, 0.31]	−0.53	.59	
<i>N</i> obs.	479			487			
<i>N</i> groups	167			167			
AICc	1358.00			1338.88			
Mean VIF	1.50			1.45			
<i>R</i> ² Total	0.14			0.34			
<i>R</i> ² Fixed Effects	0.09			0.12			

9.1. Social cohesion

In a linear regression model, we contrasted social cohesion in the synchronous ($M = 4.81$, $SD = 0.89$; $b = 0.42$, $.99\text{CI} = [0.08, 0.75]$, $t(517) = 3.22$, $p = .001$) and the asynchronous condition ($M = 4.80$, $SD = 0.81$; $b = 0.41$, $.99\text{CI} = [0.07, 0.74]$, $t(517) = 3.15$, $p = .002$) with the control condition ($M = 4.45$, $SD = 0.90$; b intercept = -0.35 , $.99\text{CI} = [-0.64, -0.06]$, $t(517) = -3.15$, $p = .002$) and found significant positive differences in both conditions, $F(2, 517) = 5.87$, $p = .003$, $R^2_{adj} = 0.02$. Indeed, post-hoc Tukey contrasts indicated that the synchrony and asynchrony conditions were not different from each other ($b = -0.01$, $.99\text{CI} = [-0.29, 0.27]$, $t(517) = -0.11$, $p = .99$). Thus, we found no specific effect of synchrony in fostering social cohesion (for random-intercept model with controls see supplemental S.4.4).

Table 5
Descriptive statistics and linear regression models of Blatant Dehumanization, Curse of Knowledge and the Interpersonal Reactivity Index.

	Blatant Dehumanization				Curse of Knowledge				Interpersonal Reactivity Index			
	<i>M</i> (<i>SD</i>)	<i>b</i> [.99 CI]	<i>t</i>	<i>p</i>	<i>M</i> (<i>SD</i>)	<i>b</i> [.99 CI]	<i>t</i>	<i>p</i>	<i>M</i> (<i>SD</i>)	<i>b</i> [.99 CI]	<i>t</i>	<i>p</i>
Control ^a	1.39 (4.43)	0.01 [−0.29, 0.24]	−0.24	.81	3.05 (9.71)	−0.08 [−0.34, 0.19]	−0.76	.45	3.21 (0.34)	−0.08 [−0.34, 0.18]	−0.79	.43
Synch.	1.44 (3.74)	0.01 [−0.30, 0.33]	0.10	.92	5.22 (15.01)	0.16 [−0.17, 0.49]	1.27	.21	3.23 (0.29)	0.06 [−0.25, 0.38]	0.52	.60
Asynch.	1.60 (5.38)	0.05 [−0.27, 0.36]	0.38	.71	3.60 (13.87)	0.04 [−0.28, 0.36]	0.33	.74	3.25 (0.30)	0.13 [−0.18, 0.45]	1.07	.28
Model summary	$F(2, 533) = 0.10, p = .91, R_{adj}^2 = 0.00$				$F(2, 484) = 1.06, p = .35, R_{adj}^2 = 0.00$				$F(2, 516) = 0.62, p = .54, R_{adj}^2 = 0.00$			

^a Baseline condition in the linear regression and the corresponding regression coefficient is the model intercept. In these models, synchronous and asynchronous conditions were contrasted with the control condition. Post-hoc Tukey contrasts indicated no differences between the synchronous and asynchronous conditions (see supplemental S.4.3).

10. Discussion

In [Experiment 3](#), we probed more specifically the ways in which synchrony could foster mentalizing. Participants responded to various measures of mentalizing that capture distinct processes involved in mental state reasoning: dispositional tendencies (IRI), accuracy in estimating others knowledge and the influence of egocentric biases (Curse of Knowledge), broad mental state attribution to social outgroups (dehumanization), attribution of mental states to non-human targets (IDAQ) and narrow mental state attribution to participants' group members. These various measures were at most weakly correlated, suggesting distinct mentalizing processes (see [Table S5](#)). Taken together, the current findings point to synchrony's effects on increasing the specificity of mental state attribution to socially-relevant targets. That is, synchrony decreased the extent to which participants promiscuously extended their attribution of human mental states to non-human targets, and synchrony did not affect mental state attributions to people in general. Rather, synchrony increased participants' attribution of mental states specifically to their fellow participants who they synchronized with.

With the addition of a no-movement control condition, [Experiment 3](#) established that the effects are due to the synchrony condition. The asynchrony and control conditions did not differ. This also clarified, to some extent, the null effects on social cohesion reported in our previous experiments. Participants in both the synchronous and asynchronous conditions reported greater social connection to their fellow participants as compared to control. Thus, we did not replicate synchrony's unique effects on social cohesion that has been reported in the literature. We did find that simply participating in the musical performance task, in either the synchronous or asynchronous condition, was sufficient to increase social cohesion. As we did not meaningfully modify the synchrony manipulation between experiments, we cannot rule out any methodological contributions to this null finding. Although we are not the first to fail to replicate this effect (e.g., [Schachner & Garvin, 2010](#)), a recent meta-analysis found reliable positive effects of synchrony on social cohesion ([Mogan, Fischer, & Bulbulia, 2017](#)).

We did not find that IRI scores differed between conditions. Upon returning to [Lawrence et al.'s \(2004\)](#) validation study of the EQ, we realized that although the IRI and EQ were modestly correlated overall, none of the subscales of the IRI correlated significantly with the *cognitive* empathy subscale of the EQ. Indeed, both the perspective taking and empathic concern subscales of the IRI were reported to be positively correlated with the *affective* empathy subscale of the EQ. Thus, the IRI may capture another aspect of mentalizing and was not a direct substitute for the EQ – where we observed differences in self-reported tendencies of *cognitive* empathy in [Experiments 1 and 2](#).

As in [Experiment 2B](#), we did not find that synchrony increased accuracy in mental state estimation – even though the targets of the Curse of Knowledge measure were specifically those that the participants had interacted with, as opposed to the pictures of eyes used in [Experiment 2](#). That is, although we observed a clear Curse of Knowledge effect, participants' accuracy in estimating others' knowledge states did not differ between conditions. We hypothesized that synchrony would

increase accuracy in mentalizing by decreasing the egocentric biases that hinder reasoning about other minds, and is implicated in the Curse of Knowledge ([Birch, 2005](#)). But, we did not find support for this hypothesis.

As we did not find that the extent of participant's dehumanization was affected by the synchrony manipulation – the current evidence does not suggest that synchrony broadly increases mentalizing. However, we did find that synchrony decreased anthropomorphism and increased mental state attribution to those the participants synchronized with. Although the IDAQ is typically used as a trait-measure, it has been demonstrated to be situationally malleable ([Waytz, Epley, et al., 2010](#)). As in our previous experiments, these results held after controlling for social cohesion – reflecting the unique effects of synchrony on certain processes involved in mentalizing.

11. General discussion

In three experiments, we examined the effects of synchrony on mentalizing. In [Experiments 1 and 2](#), we found that synchrony increased self-reported perceptions of individuals' tendencies and abilities to consider the mental states of unspecified others. However, we found no evidence for synchrony increasing feelings of emotional empathy or one's ability to, in the moment, accurately reason about the content of other minds ([Experiments 2 and 3](#)). But, we did find that synchrony increased *state* mental state attribution to specifically participant's group members, and suppressed promiscuous mentalizing, that is, anthropomorphism ([Experiment 3](#)). Taken together, these results suggest that synchrony increases perceptions of one's tendencies and capacities to consider others' mental states, and targets attention to the minds in one's immediate environment.

These results offer some evidence that synchrony tunes our minds to the minds of others, and suppresses the projection of mental states to the world writ large. Functionally, an increase in the recognition of and attention paid to the mental states of others should make further coordination with others easier to accomplish (e.g., [Curry & Chesters, 2012; Lin et al., 2010](#)). Broadly, mentalizing has been reliably demonstrated to be recruited in solving a variety of cooperative and coordination dilemmas ([Lissek et al., 2008; McCabe, Houser, Ryan, Smith, & Trouard, 2001](#)). Thus, synchrony may make individuals more willing and able to coordinate by increasing one's willingness to attribute and attend to the mental states of others. Additionally, the attribution of mental states to a target is often followed by an increase in moral concern ([Gray et al., 2007; Loughnan et al., 2010](#)). Thus, synchrony may motivate abilities for coordination as well as desires to protect and sustain the cooperative relationship – a possibility that can be explored in future research.

The current effects of synchrony on mentalizing may speak to the emerging literature exploring the effectiveness of dance-movement therapies for individuals diagnosed along the Autism Spectrum ([Koehne et al., 2016; Behrends, Müller, & Dziobek, 2012; Landa, Holman, O'Neill, & Stuart, 2011; McGarry, 2011; Rabinowitch, Cross, & Burnard, 2013](#)). Indeed, the current results may point to the specificity of synchrony movement in enhancing mentalizing as the mechanism by

which dance-movement therapies encourage social engagement. Certainly however, more research is needed to assess the effectiveness of synchrony as a specific intervention. Koehne et al. (2016) found that perceived synchrony increased perceived ability to take on the perspective of another, but that this effect did not replicate in a sample of individuals diagnosed along the Autism Spectrum. However, their synchrony manipulation occurred during a staged virtual interaction which may lack the specific benefits of coordinating behaviors with physically present others that more closely resembles dance-movement therapy sessions. It remains an open question whether these effects are specific to synchronous actions or whether joint action paradigms, more broadly, would have similar effects (perhaps especially those in which the actions of others are predictable, but not synchronized).

Our results held after accounting for sex, the cultural backgrounds of the participants and the degree of ethnic homogeneity of the experimental groups. Furthermore, the effect of synchrony on mentalizing was not moderated by demographics. Nevertheless, participants across these experiments were sampled entirely from an undergraduate Canadian student population. Therefore, we make no broad claims as to the generalizability of these effects to different cultural milieus. Importantly, we know of only a single experimental study of the effects of synchrony that tests these hypotheses outside of a WEIRD cultural context (Henrich, Heine, & Norenzayan, 2010). Notably, this study found that behavioral synchrony alone was not sufficient to increase cooperation (in Brazil; Cohen, Mundry, & Kirschner, 2013).

The Empathy Quotient was designed to assess trait mentalizing tendencies, and, to our knowledge, has not been used in an experimental context as it was employed here. However, the observed differences indicate that this measure is responsive to experimental manipulations. Indeed, as this measure assesses self-reported perceptions of one's mentalizing propensities, we can infer from these experiments that synchronizing with others fosters a state in which one *feels* like they are more willing and more able to mentalize. Although mentalizing propensities are typically treated as dispositions – self-report measures of state empathy are often comparable in item-content and positively correlated with trait measures (e.g., Shen, 2010). Furthermore, Lamm, Batson, and Decety (2007) found that EQ scores, specifically, were positively correlated with the on-line activation of neural systems implicated in mentalizing during empathy induction – suggesting overlap in state and trait indices of mentalizing.

We did not observe any specific effects of synchrony on feelings of social cohesion. This might be the result of the perils of bringing the study of collective cultural practices into the lab. These practices, such as collective rituals, are often emotionally intense, and have historically been infused with deep religious, spiritual or otherwise culturally-salient meaning. This is certainly not descriptive of the environment we created during these experiments – although anecdotally students did find these experiments quite enjoyable. As the synchrony manipulation employed here did not invoke any meaningful broader cultural context, we can infer that synchronous movement, even out of context, has effects on mentalizing. Importantly, a recent meta-analysis (which included the social cohesion results from Experiments 1 and 2) found a robust positive effect of synchrony on social cohesion (Mogan et al., 2017). This meta-analysis further revealed that synchrony's effects were stronger on behavioral indices of social cohesion than cognitions. Furthermore, synchrony's effects on real-world social cohesion might be dose responsive regarding both repetitive participation and participating with the same set of individuals. We know of only a single longitudinal study of the effects of synchrony (in typically developing children; Wen et al., 2016). Indeed, the utility of synchrony-specific interventions for enhancing mentalizing can only be further specified by longitudinal research.

We proposed that synchrony would foster mentalizing by guiding our attention to minds in our environments and by reducing the egocentrism that hinders accurate reasoning about other minds. The results of the three experiments presented here offer some evidence for how

synchrony increases one's willingness to consider and perceive the mental states of others. However, we did not find support for the latter part of this model – that is, synchrony did not reduce egocentrism in mental state reasoning. Nevertheless, future research should consider the diverse ways to operationalize accuracy in mentalizing, especially beyond relying on self-report measures. Taken altogether, the current findings offer new evidence regarding how a recurrent aspect of cultural collective practices and day-to-day interpersonal coordination – synchrony – shapes a core aspect of social cognition, the tendency to tune our minds to the mental states of others.

Open practices

The experiments reported in this paper earned Open Materials and Open Data badges for open practices. Materials, data, and analysis scripts are available at: osf.io/5xmb2.

Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.jesp.2017.10.008>.

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