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Mutual coordination strengthens the sense of joint agency in cooperative joint action

Nicole K. Bolt[a], Evan M. Poncelet[a], Benjamin G. Schultz[b], Janeen D. Loehr[a]

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[a]Department of Psychology, University of Saskatchewan, 9 Campus Drive, Saskatoon, SK, Canada, S7N 5A5. Email: nicole.bolt@usask.ca, evan.poncelet@usask.ca, janeen.loehr@usask.ca.

[b]Department of Neuropsychology and Psychopharmacology, Faculty of Psychology and Neuroscience, Maastricht University, P.O. Box 616, 6200 MD Maastricht, The Netherlands. Email: ben.schultz@maastrichtuniversity.nl.

Correspondence concerning this article should be addressed to Janeen Loehr, Department of Psychology, University of Saskatchewan, 9 Campus Drive, Saskatoon SK, Canada, S7N 5A5. Email: janeen.loehr@usask.ca.

Abstract

Philosophers have proposed that when people coordinate their actions with others they may experience a sense of joint agency, or shared control over actions and their effects. However, little empirical work has investigated the sense of joint agency. In the current study, pairs coordinated their actions to produce tone sequences and then rated their sense of joint agency on a scale ranging from shared to independent control. People felt more shared than independent control overall, confirming that people experience joint agency during joint action. Furthermore, people felt stronger joint agency when they a) produced sequences that required mutual coordination compared to sequences in which only one partner had to coordinate with the other, b) held the role of follower compared to leader, and c) were better coordinated with their partner. Thus, the strength of joint agency is influenced by the degree to which people mutually coordinate with each other's actions.

Keywords: Agency, joint action, joint agency, shared control, interpersonal coordination

1. Introduction

The *sense of agency* refers to the feeling of generating and controlling actions and their effects (Haggard & Tsakiris, 2009). For example, when someone turns on a light, they have a sense of agency over flicking the light switch and causing the light to come on. Previous research has shown that the sense of agency is driven by a combination of predictive processes as well as postdictive cognitive inferences (Moore & Haggard, 2008; Synofzik, Vosgerau, & Voss, 2013). However, most research on the sense of agency has focused on individuals performing tasks alone. Little research has investigated agency during *joint action*, when two or more individuals coordinate their actions to achieve a shared goal (Sebanz, Bekkering, & Knoblich, 2006). Philosophers have proposed that the experience of agency during joint action might be substantially different than during solo action (e.g., Pacherie, 2012). In addition to experiencing a sense of *self*-agency over actions and their effects (e.g., a sense that "I did it"), people may also experience a sense of *joint* agency over actions and effects (e.g., a sense that "we did it"). The current study investigates people's experiences of joint agency when they engage in cooperative joint action.

1.1. Philosophical accounts of joint agency

Gallotti and Frith (2013) proposed that when people coordinate their actions, they engage in a collective mode of cognition called the *we-mode*. The main idea of the we-mode is that co-agents represent their actions as something they are going to pursue together, as a single unit. This way of cognizing is hypothesized to enlarge people's potential for action by giving them access to more information about their partners' behavior than they would have as mere disembodied observers. This information provides new possibilities for action, allowing people to bring about actions and effects they could not accomplish individually and expanding their agency scope (Pacherie, 2012). For example, two people may be able to lift a heavy object that neither person could lift alone.

Cognition in the we-mode may lead to feelings of joint agency (Dewey, Pacherie, & Knoblich, 2014). Dokic (2010) defines joint agency as "the perceptual sense that we are acting together" (p. 40). Similarly, Seeman (2009) proposes that joint action will involve "a sense of acting together … [which] amounts to a sense of joint control" (p. 504). Pacherie (2012) provides the most specific definition of joint agency, describing it as the sense that one's contribution to a joint goal is equal to the contributions of one's co-agents and that one's coordination relations with co-agents are relatively symmetrical. Pacherie (2012) therefore predicts that the sense of joint agency will be strongest in situations where individual contributions are of similar importance to the joint goal, and where both people coordinate with each other rather than one person coordinating and the other being coordinated. Pacherie also proposes that joint agency may be experienced in two forms: *shared agency*, whereby people experience a sense of joint agency along with an intact sense of self-agency, and *we-agency*, whereby the experience of joint agency is accompanied by a reduction in self-agency. We-agency is thought to be experienced when co-agents perform similar actions with similar effects and synchronous timing. For example, soldiers marching in step may experience a loss of self-agency as their actions become one with the group (McNeill, 1995). However, most everyday joint actions are thought to involve shared agency, as they typically require people to produce coordinated yet distinct and complementary actions.

1.2. Empirical investigations of agency in joint action

A handful of studies have examined people's experiences of agency during cooperative joint action. Some studies have focused on people's sense of control over actions and effects that unfold over time, such as the movement of a cursor on the screen (Dewey et al., 2014; van der Wel, 2015; van der Wel, Sebanz, & Knoblich, 2012). These studies have asked participants to continuously coordinate their actions to elicit a joint effect and then rate the extent to which they felt control while they performed the task. For example, van der Wel (2015) had pairs of participants coordinate their joystick movements to move a single dot from the center of the screen to one of two target areas. Similarly, Dewey et al. (2014) had pairs of participants use joysticks to keep a cursor centered on a moving target. Van der Wel (2015) showed that ratings of control were equally high for the partner who chose which target to move to and for the partner who followed the other's choice. Dewey et al. (2014) showed that participants' ratings of control were higher when both participants' actions contributed to the movement of the cursor compared to when only one participant's actions contributed to its movement, as long as participants' contributions were distinguishable (e.g., each was responsible for one movement direction). These findings indicate that people's sense of control depends on both partners' combined contributions rather than their own individual contributions, suggesting that people may experience joint agency during these tasks. In other words, people may have evaluated their sense of control at the group level rather than at the individual level ("we are in control"; Dewey et al., 2014).

Empirical investigations of agency during joint action have also examined the influences of perceptual and sensorimotor information on people's experiences of control. Van der Wel (2015) showed that participants' ratings of control were positively correlated with the smoothness of both their own movements and their partner's movements. Van der Wel et al. (2012) showed that when pairs coordinated their actions to move a pole back and forth between two targets, each individual's ratings of control were positively correlated with pair-level task accuracy but not with the amount of force exerted by each individual. These findings are consistent with predictive accounts of agency (e.g., Blakemore, Frith, & Wolpert, 1999; Wolpert & Flanagan, 2001) in which agency is derived from comparisons between the predicted and actual consequences of actions; the better the match, the stronger the sense of agency. More specifically, these findings suggest that in joint action, agency may be based predominantly on comparisons between expected and actual perceptual information, to which both people have access, rather than sensorimotor information, to which only individuals have access (see also van der Wel & Knoblich, 2013).

Other empirical investigations of agency in joint action have examined people's sense of causal initiation for brief, jointly-produced action effects, such as a single tone (Dewey & Carr, 2013; Obhi & Hall, 2011). These studies have focused on how people's role in the joint action affects their sense of causal initiation. Obhi and Hall (2011) had pairs of participants coordinate their actions to depress a single button, which evoked a tone. Participants either initiated the button press (leaders) or passively moved their finger along with their partner's action (followers). Participants' ratings of responsibility for producing the tone were polarized such that leaders felt entirely responsible whereas followers felt completely not responsible. Dewey and Carr (2013) had pairs of participants produce either the first button press (leaders) or the second button press (followers) in a two-press sequence. A single tone was evoked at a variable delay after the second button press, and participants rated whether they or their partner had produced the tone. In this study, followers felt more self-agency (and were rated as having more other-agency) compared to leaders, likely because the follower's button press occurred closest in time to the tone and was therefore perceived as having caused it. Together, these studies show that people's roles within a joint action affect their experiences of causal initiation.

In sum, investigations of agency during joint action suggest that people may experience a sense of joint agency that may be influenced by perceptual information and their role in the joint action. However, this interpretation remains tentative because, to date, no study has asked participants specifically about their feelings of joint agency. Investigations focused on the sense of control have asked participants to rate statements such as “how strongly they had experienced to be in control” (van der Wel et al., 2012) or “how effective was your joystick at controlling the dot?” (Dewey et al., 2014). Such statements may be ambiguous regarding whether they refer to self-agency, joint agency, or a mixture of the two (Dewey et al., 2014; van der Wel et al., 2012). As noted above, Dewey et al. (2014) suggested that participants may have interpreted their rating scale in terms of shared rather than individual control, but this has yet to be established directly. Studies of causal initiation have used rating scales focused primarily on self- and other-agency. For example, Dewey and Carr (2013) had participants rate the statements, “Did you/the person sitting next to you produce the tone?”, and Obhi and Hall (2011) used a rating scale that ranged from “completely not responsible” to “entirely responsible” for producing the tone. Although Obhi and Hall’s (2011) scale did include a midpoint of “they pressed the key at the same time,” such a statement could, but does not necessarily, imply joint agency. Thus, the primary goal of the current study was to directly examine people’s experiences of joint agency. To do so, we employed a task designed to induce an experience of shared agency, that is, joint agency along with an intact sense of self-agency (Pacherie, 2012). We then manipulated factors hypothesized to influence joint agency, and employed a rating scale that asked participants specifically about their experience of joint agency. We describe the task, the rating scale, and the factors we manipulated next.

1.3. The current study

The current study employed a sequence production task in which pairs of participants had to coordinate their actions to produce a sequence of tones that matched the pace set by a metronome (see Figure 1). Each participant produced half of the tones in the sequence, and each tone was elicited by a single button press. We expected participants to experience strong, unambiguous self-agency for the tones because a) each person had their own button placed directly in front of them, so there was no spatial ambiguity as to whose button elicited the tone; b) participants pressed their buttons at different times (separated by approximately 500 ms intervals), so there was no temporal ambiguity as to whose button elicited the tone; and c) tones were elicited immediately after each button press, which induces a strong experience of self-agency (e.g., Sato, 2008).

The rating scale we used asked participants to “[r]ate your feelings of control over the timing of the sequence” on a scale ranging from “shared control” to “independent control”. We chose this rating scale for several reasons. First, we focused on people’s sense of control rather than causal initiation because the previous research that most strongly suggests that people may experience joint agency was focused on control. Second, we asked participants to rate their control over the timing of the sequence because a) the task required participants to coordinate their timing to achieve the shared goal of matching the metronome pace, and b) we wanted participants to focus on the timing of the tones rather than the tones themselves, over which we expected them to experience strong and constant self-agency. Third, we used the term “shared control” as the endpoint reflecting joint agency because this terminology is consistent with researchers’ and philosophers’ descriptions of joint agency (e.g., Dewey et al., 2014; Seeman, 2009), and we used the term “independent control” as the other endpoint to capture the opposite of shared control without implicating agency over the tones themselves.

Based on the philosophical and empirical literature reviewed above, we manipulated two factors that we hypothesized would influence the sense of joint agency. First, we manipulated *coordination symmetry* based on Pacherie's (2012) prediction that people are most likely to experience joint agency when coordination relations between them are symmetrical (e.g., when both people in a pair adapt to each other's actions) compared to when they are asymmetrical (e.g., when one person adapts to the other but not vice versa). According to Pacherie, symmetry enhances joint agency by increasing the degree to which (and/or the accuracy with which) people make predictions about the perceptual outcomes of each other's actions. This is consistent with empirical evidence linking perceptual information with the experience of agency during joint action (van der Wel, 2015; van der Wel et al., 2012). Second, we manipulated participants' *role* within the joint action based on Pacherie's (2012) prediction that even small differences in the salience of people's roles may influence their experience of agency. For example, the person who acts first may perceive themselves as the leader of the joint action (Wegner & Sparrow, 2007) and therefore may experience a weaker sense of joint agency. This is consistent with studies showing that people's roles within a joint action influence their ratings of causal initiation (Dewey & Carr, 2013; Obhi & Hall, 2011), although the influence of role on ratings of control has yet to be examined.

2. Experiment 1

Experiment 1 examined the influence of coordination symmetry and role on people's ratings of joint agency. We manipulated coordination symmetry by having participants produce tones either in alternation (ABABABAB, where A and B represent the two partners, respectively) or sequentially (AAAABBBB). The alternating task required symmetrical coordination between partners, because both partners had to adapt the timing of their own actions to the timing of their partner's actions on a turn-by-turn basis. In contrast, the sequential task required asymmetrical coordination between partners because only the second partner had to adapt the timing of their actions to the timing of their partner's actions. We predicted that participants would experience stronger joint agency in the alternating compared to the sequential task. Role was determined based on which partner acted first for a given sequence: leaders produce the first tone(s) in the sequence (i.e., partner A as labeled above) and followers produced the subsequent tone(s) (partner B). We hypothesized that the partner who acted first might experience weaker joint agency than the partner who acted second.

2.1. Method

2.1.1. Participants

Forty-eight University of Saskatchewan students (12 males, mean age = 21.10, *SD* = 3.51) participated in the study in pairs. Thirteen of the pairs had two female partners, 10 pairs were mixed-gender, and one pair had two male partners. Ethical approval was obtained from the institutional review board prior to participant recruitment, and all participants gave informed consent before beginning the study. Participants were compensated with either credit for their introductory psychology course or $10.

2.1.2. Design

Pairs of participants produced sequences of eight tones that matched the pace set by a metronome (i.e., a series of isochronous pacing clicks; see Figure 1) in a 2 (task: alternating, sequential) by 2 (role: leader, follower) within-subjects design. In the alternating task, participants produced tones in alternation with each other (i.e., ABABABAB, where A and B refer to each partner, respectively). In the sequential task, one participant produced the first four tones and the other produced the last four tones (AAAABBBB). For half of the sequences, the participant sitting on the left was the leader (i.e., the participant who produced the first tone(s) in the sequence) and for the remaining sequences the participant on the right was the leader.

2.1.3. Apparatus and materials

Participants sat next to each other on the same side of a table. A computer screen was centered between them and positioned approximately 60 cm from the edge of the table. An Interlink force-sensitive resister (FSR; 3.81 cm^2) was placed directly in front of each participant, approximately 30 cm from the edge of the table. Participants tapped the FSRs with the index finger of their dominant hand. The FSRs registered participants' taps without providing any auditory feedback. Each tap triggered a 1000 Hz tone (100 ms duration, 10 ms rise/fall time) via a WaveShield connected to an Arduino UNO R3 microcontroller. This setup ensured a very short latency between taps and tones (approximately 3 ms; see Schultz & van Vugt, 2015, for technical details). The Arduinos also sent a signal to the Presentation recording software (Neurobehavioural Systems, Inc., Albany, CA, USA) each time a tap was registered. Presentation was used to record the taps and to present the remaining auditory and visual stimuli for the experiment, including the four pacing clicks, which were presented in a snare drum timbre. All auditory stimuli were presented through speakers placed on both sides of the computer screen. In addition, number keypads were placed beside each FSR and covered with occluders so that participants could enter their agency ratings but could not see their partner's ratings.

2.1.4. Procedure

The experiment began with two practice blocks, one for the alternating task and one for the sequential task. Each practice block began with two pre-training trials during which the experimenter controlled the presentation of the events that comprised a trial and explained the tasks. The remainder of each practice block consisted of 10 training trials with the timing described in the next paragraph. Participants then completed a test phase consisting of 16 blocks of 5 trials, also with the timing described in the next paragraph. Participants provided agency ratings after every trial in the test blocks only. Blocks alternated between the alternating and sequential tasks, the order of which was held constant through both the training and test phases and was counterbalanced across participants. One member of the pair was the leader for all trials in a given block. We counterbalanced which partner (sitting on the left or right) was the leader on the first test block across participants. The partner who was the leader for the first block became the follower for the second block. The other partner was then leader and follower for the third and fourth blocks, respectively. These four combinations of task and role were repeated four times in the same order to make up the 16 test blocks (and the last two of the four combinations made up the two training blocks). At the beginning of each block, instructions presented on the computer screen indicated which task was to be performed and which participant was to be the leader.

Each trial began with a visual cue to remind participants of the task and roles. The visual cue consisted of a cartoon face with two arms, one of which was colored red to indicate that the person on that side of the table would produce the first tone(s) in the sequence. The word "alternating" or "sequential" appeared above the face. The cue remained on the screen for 2000 ms. A fixation cross then appeared and remained in the center of the screen until the last tone of the sequence was produced. Four pacing clicks were presented at 500 ms intervals beginning 500 ms after the onset of the fixation cross. Participants were instructed to produce the tone sequence while maintaining the pace set by the clicks. After each sequence, participants were asked to "Rate your feelings of control over the timing of the sequence" on a scale that ranged from 01 (shared control) to 99 (independent control). Participants were instructed to include 0 as the first digit for any ratings less than 10 to prevent their partner from guessing their rating based on the number of keystrokes they entered. Participants were encouraged to take as much time as they needed to complete the ratings. Participants entered their ratings in random order, determined separately for each trial and signaled by which side of the screen the rating scale instructions appeared on first (e.g., the rating scale first appeared on the right side of the screen, signaling the participant on the right to enter their rating, and then switched to the left side of the screen, signaling the participant on the left to enter their rating). After both participants had entered their ratings, feedback indicating whether or not they had correctly matched the metronome pace was presented for 1000 ms in the center of the screen. A green check mark indicated that they had correctly matched the pace and a red “X” indicated that they had not.

Feedback was determined based on whether the average inter-tap interval (ITI) produced by participants fell within a window around the required pace of 500 ms. An adaptive window size was used to ensure that error rates would be similar (approximately 20%) across conditions. The window size was set to 50 ms at the beginning of the experiment (i.e., sequences were considered correct if the average ITI fell within 500 ± 25 ms). After each block, the window size decreased by 10 ms if participants made no errors, stayed the same if they made one error (1 error in 5 trials = 20% error rate), or increased by 10 ms if they made more than one error. The window size was adjusted separately for each combination of task and role, with the exception that the last 5 of the 10 training trials for a given task were used in the first window size adjustment for that task (combined with both roles).

2.1.5. Data analysis

2.1.5.1. Performance errors

We analyzed participants’ agency ratings only for trials on which they received correct feedback, to avoid potential effects of attribution biases that may come into play when errors are made in a joint task (e.g., Mynatt & Sherman, 1975; Taylor & Doria, 1981). Trials were also removed from the analysis if they contained a sequence production error (participants produced their tones in the wrong order) or a rating error (participants entered their ratings in the wrong order or a participant entered an invalid rating). In total, 0.67% of correct trials were removed due to sequence production errors and an additional 5.21% of correct trials were removed due to rating errors. This left an average of 14.58 agency ratings per participant per condition.

2.1.5.2. Linear mixed-effects model analysis

We used a linear mixed-effects model analysis to examine the effects of task and role on agency ratings while accounting for shared variance within pairs. We included fixed factors of task (alternating and sequential) and role (leader and follower). We began with a maximal random effects structure (Barr, Levy, Sheepers, & Tily, 2013; Bates, Kliegl, Vasishth, & Baayen, 2015) that included an intercept and slopes for task, role, and their interaction for pairs; an intercept and slopes for task, role, and their interaction for participants; and an intercept for trial. Model fits were estimated using restricted maximum likelihood via the MIXED command in SPSS Version 23. If the model fitting procedure failed to converge, we removed random effects whose covariance was estimated as zero. We then iteratively refined the random effects structure by checking whether the goodness of fit was significantly reduced after the random effect that accounted for the least variance was removed. Specifically, we compared the estimated deviances (-2 log-likelihood; -2LL) using a likelihood ratio test. This procedure allowed us to remove random effects not supported by the data (Bates et al., 2015). We then tested whether goodness of fit improved by fitting correlation parameters for the remaining variance components and for the residuals (Bates et al., 2015). The final model included an intercept and slopes for role for pairs; an intercept and slopes for task, role, and the task by role interaction for participants; and an intercept for trial. We report F and t tests for fixed effects and post-hoc pairwise comparisons, respectively. Degrees of freedom for these tests were obtained by Satterthwaite approximation.

2.2. Results

Figure 2 shows the estimated mean agency ratings for each task and role. Participants' mean rating was 39.76 overall (95% CI [33.83, 45.69]), indicating that they tended to experience shared rather than independent control when engaged in a cooperative joint action. However, participants' ratings of control differed depending on both task and role. As Figure 2 shows, participants rated their feelings of control as more shared in the alternating task compared to the sequential task, $F(1, 46.90) = 13.10$, $p = .001$. Furthermore, participants rated their feelings of control as more shared when they were the follower compared to the leader, $F(1, 22.64) = 6.10$, $p = .022$. Lastly, there was a significant interaction between task and role, $F(1, 39.15) = 7.13$, $p = .010$. Table 1 shows the estimated mean differences between roles for each task (and between tasks for each role), along with confidence intervals and standardized effect sizes. As the table shows, the difference in agency ratings between leader and follower was significant in the sequential task but not in the alternating task.

2.3. Discussion

Experiment 1 examined whether coordination symmetry and leader/follower roles within a joint action influence people's experience of joint agency. The experiment yielded three main findings. First, people experienced a sense of shared control, rather than independent control, when they engaged in a cooperative joint action in which each partner made distinct contributions to a shared goal. This finding provides direct evidence that people do indeed experience joint agency, as predicted by philosophical accounts (e.g., Dokic, 2010; Pacherie, 2012; Seeman, 2009). Second, people experienced more shared control over a joint action that required symmetrical coordination between partners (i.e., the alternating task) compared to a joint action that required asymmetrical coordination (i.e., the sequential task). This is consistent with the prediction that coordination symmetry influences people's experience of joint agency. Third, leaders experienced a more independent sense of control than followers, but only when the joint action required asymmetrical coordination. This is consistent with the prediction that small differences in the salience of people's roles within the joint action influence their experience of joint agency (Pacherie, 2012; Wegner & Sparrow, 2007), but suggests that the salience of these differences might depend on the nature of the task.

We conducted a second experiment examining people's experiences of joint agency in a cooperative joint action for three reasons. First, we sought to replicate the findings from Experiment 1 with a new sample of participants. Second, we sought to examine whether the *distinguishability of partners' contributions*, rather than coordination symmetry, might explain the differences between the alternating and sequential tasks found in Experiment 1. According to Pacherie's (2012, 2014) theoretical analysis, people may be more likely to experience joint agency when their actions are difficult to distinguish from their partners' in physical form and/or in time (e.g., people should experience the strongest joint agency when they perform the same actions at the same time, as when soldiers march in step with each other). In Experiment 1, coordination symmetry was confounded with the distinguishability of partners' contributions because asymmetrical coordination required contributions that were more temporally distinct (i.e., in the sequential task, partners' contributions were separated in time; each person produced their actions in separate turns that did not overlap), whereas symmetrical coordination required less temporally distinct contributions (i.e., in the alternating task, partners' actions were interleaved rather than separated in time).

Third, we sought to explore a potential mechanism for the link between coordination symmetry (or distinguishability of partners' contributions) and the sense of joint agency. Pacherie (2012, 2014) hypothesized that both factors would enhance joint agency by increasing the degree to which people are able to make predictions about each other's actions. In joint action tasks that require temporal coordination, the better people are able to predict each other's actions, the better coordinated their actions are in time (e.g., Keller, Knoblich, & Repp, 2007; Loehr & Palmer, 2011; Zamm, Wellman, & Palmer, in press). Thus, in Experiment 2 we investigated whether the link between coordination symmetry (or distinguishability of partners' contributions) and joint agency is mediated by the degree of *objective coordination* between partners' actions (i.e., how well coordinated their actions were in time).

3. Experiment 2

Experiment 2 employed the same sequence production task as Experiment 1. We increased the length of the sequence from 8 to 24 tones to facilitate measurement of objective coordination between partners' actions. We included the same leader and follower roles as in Experiment 1. We also included the same alternating and sequential tasks. In the alternating task, partners produced tones in alternation (ABABABAB…). We refer to this as the immediate-alternating task in Experiment 2. In the sequential task, one partner produced all 12 of their tones before their partner produced the 12 remaining tones (AAA…ABBBB…B). To test whether coordination symmetry or the distinguishability of partners' contributions better explains the difference between the alternating and sequential tasks found in Experiment 1, we added a third task in which each partner produced four tones before switching turns (i.e., AAAABBBBAAAABBBB…). We refer to this as the distant-alternating task.

Both the immediate-alternating and the distant-alternating tasks required symmetrical coordination between partners (i.e., both partners had to adapt the timing of their own actions to the timing of their partner's actions on an alternating basis). In contrast, the sequential task required asymmetrical coordination (i.e., only the second partner had to adapt the timing of their actions to the timing of their partner's actions). Thus, if coordination symmetry explains the pattern of agency ratings found in Experiment 1, agency ratings should be similar for the immediate- and distant-alternating tasks, and both should be rated as more shared than the sequential task, in Experiment 2. In contrast, the temporal distinctness of partners' contributions increased from the immediate-alternating task (in which partners' actions were interleaved on every turn) to the distant-alternating task (in which partners' actions were interleaved only every four turns) to the sequential task (in which partners' actions were not interleaved at all). Thus, if the temporal distinguishability of partners' contributions explains the pattern of agency ratings found in Experiment 1, agency should be rated as most shared for the immediate-alternating task, less shared for the distant-alternating task, and least shared for the sequential task in Experiment 2.

Increasing the sequence length to 24 tones allowed us to calculate a mathematical measure of objective coordination using a sine wave transform (Schultz & Demos, in prep). We then followed the causal steps approach to mediation outlined by Baron and Kenny (1986) as a first attempt at assessing whether objective coordination might account for the effects of task on agency ratings.[1]

3.1. Method

3.1.1. Participants

Forty University of Saskatchewan students (9 males, mean age = 22.75, *SD* = 5.41) participated in the study in pairs. Eleven of the pairs had two female partners, 8 pairs were mixed-gender, and one pair had two male partners. Ethical approval was obtained from the institutional review board prior to participant recruitment, and all participants gave informed consent before beginning the study. Participants were compensated with either credit for their introductory psychology course or $10.

3.1.2. Design

Pairs of participants performed the same the sequence production task as in Experiment 1 in a 3 (task: immediate-alternating, distant-alternating, sequential) by 2 (role: leader, follower) within-subjects design. As in Experiment 1, in the immediate-alternating task participants produced tones in alternation with each other (i.e., ABABABAB…, where A and B refer to each partner, respectively), and in the sequential task one participant produced all of their tones before the other participant produced the remaining tones (AAA…ABBBB…B). In the distant-alternating task, each participant produced four tones before switching turns, and participants produced three turns each (AAAABBBBAAAABBBB…). As in Experiment 1, for half of the sequences the participant sitting on the left was the leader (i.e., the participant who produced the first tone(s) in the sequence) and for half of the sequences the participant on the right was the leader.

3.1.3. Apparatus and materials

Experiment 2 used the same apparatus and materials as Experiment 1.

3.1.4. Procedure

Participants first completed six pre-training trials (two per task) during which the experimenter controlled the presentation of the events that comprised a trial and explained the tasks. Participants then completed three blocks of eight training trials followed by 12 blocks of six test trials. As in Experiment 1, participants provided agency ratings after every trial in the test blocks only. Also as in Experiment 1, trials were blocked by task and role, such that participants completed the same task with the same leader/follower roles for all trials within a given block. The order of tasks was counterbalanced across participants using a balanced Latin Square design and was held constant through both the training and test phases. Participants alternated between leader and follower on each block; which partner (sitting on the left or right) was the leader on the first block was counterbalanced across participants. The six combinations of task and role were presented in the same order twice to make up the 12 test blocks (and the last three of the six combinations made up the three training blocks). At the beginning of each block, instructions presented on the computer screen indicated which task was to be performed and which participant was to be the leader.

The sequence of events on each trial was the same as in Experiment 1 with the following exceptions. First, the visual cue that began each trial included the text “1x1”, “4x4”, or “12x12” above the face (indicating that participants should alternate turns every 1, 4, or 12 tones, respectively, corresponding to the immediate-alternating, distant-alternating, and sequential tasks). Second, because we increased the sequence length from 8 to 24 tones in Experiment 2, we presented the four pacing clicks at 400 ms intervals to decrease the total task time. Third, to facilitate participants’ ability to keep track of the 24 tones, we added visual cues on the computer screen during the tone production. Specifically, a white square (approximately 1.5 cm^2) appeared on the screen each time a participant produced a tone. Squares appeared from left to right and were arranged in a grid of 4 columns and 6 rows. This allowed participants to track the tones in multiples of 4, facilitating their ability to switch turns after every 4 tones (1 row) in the distant-alternating task and after 12 turns (3 rows) in the sequential task. We used the same rating scale as in Experiment 1. Feedback was determined using the same adaptive window as in Experiment 1, but the initial window size was set to 100 ms (i.e., sequences were considered correct if participants' average ITI fell within 400 ± 50 ms) to compensate for the increased difficulty of maintaining the metronome pace for 24 rather than 8 tones. After each block, the window size decreased by 10 ms if participants made no errors, stayed the same if they made one error, or increased by 10 ms if they made more than one error. As in Experiment 1, the window size was adjusted separately for each combination of task and role, with the exception that the last 6 of the 8 training trials for a given task were used in the first window size adjustment for that task (combined with both roles).

3.1.5. Data analysis

3.1.5.1. Performance errors

As in Experiment 1, only trials on which participants received correct feedback were analyzed. In addition, trials were removed from the analysis if they contained a sequence production error (participants produced their tones in the wrong order) or a rating error (participants entered their ratings in the wrong order or a participant entered an invalid rating). In total, 3.65% of correct trials were removed due to a sequence production error and a further 5.48% of correct trials were removed due to a rating error. This left an average of 10.36 agency ratings per participant per condition.

3.1.5.2. Objective coordination measure

To measure the degree of objective coordination between participants during each sequence, discrete tap onsets were transformed into a continuous time series using the discrete to dynamic oscillator conversion (DiscDOC) toolbox in MATLAB (Schultz & Demos, in prep). DisDOC converts the discrete signal into a continuous representation similar in appearance to a sinewave, but the peaks of the waves represent the discrete onsets times (see also Demos, Chaffin, Begosh, Daniels, & Marsh, 2012). The DisDOC signals were then cross-correlated between the two partners using the first response of each participant as the starting point of the time series (i.e., lag 0). The maximum cross-correlation coefficient between lags -250 ms and 250 ms (representing antiphase values of ±200 ms with an additional 50 ms tolerance) was used to measure the degree of objective coordination between the two participants. The DisDOC toolbox estimated upper chance levels of correlation coefficients at $r = .23$ for the 12 data points per participant used in the present experiment.

3.1.5.3. Analysis strategy

We used the same linear mixed-effects model analysis strategy as in Experiment 1 to examine the manipulated factors of task and role. We then followed the causal steps approach outlined by Baron and Kenny (1986) to examine whether objective coordination might mediate the effects of task on agency ratings. Specifically, having tested whether agency ratings differed across tasks (Baron and Kenny's Step 1), we next tested whether objective coordination differed across tasks (Step 2). We then included objective coordination as a covariate in the model of agency ratings by task and role (Steps 3 and 4).

3.1.5.3.1. Manipulated factors task and role

As in Experiment 1, we included fixed factors of task (immediate-alternating, distant-alternating, and sequential) and role (leader and follower). We began with a maximal random effects structure that included an intercept and slopes for task, role, and their interaction for pairs; an intercept and slopes for task, role, and their interaction for participants, and an intercept for trial. We refined the random effects structure using the same strategy as in Experiment 1. The final model included an intercept and slopes for task and the task by role interaction for participants. As in Experiment 1, we report *F* and *t* tests for fixed effects and post-hoc pairwise comparisons, respectively, with degrees of freedom obtained by Satterthwaite approximation.

3.1.5.3.2. Objective coordination

First, to examine whether objective coordination differed across tasks, we fit a model with task as the fixed factor and objective coordination as the dependent variable. Role was not included as a fixed factor in this model because objective coordination was measured at the pair level and therefore did not differ between the leader and follower. After refining the maximal random effects structure, the final model included an intercept and slope by task for pairs and heterogeneous variances for the residuals.

Second, we used a step-up strategy (West, Welch, & Galecki, 2015) to add objective coordination as a covariate in our model of agency ratings. We added fixed factors of objective coordination, the objective coordination by task interaction, and the objective coordination by role interaction to the final model of agency ratings that resulted from our initial analysis of the effects of task and role on agency. We compared the estimated deviances (-2LL) using the likelihood ratio test. Because we compared models with different fixed effects, we estimated model fit using full maximum likelihood (ML).

3.2. Results

3.2.1. Manipulated factors of task and role

Figure 3 shows the estimated mean agency ratings by task and role. Similar to Experiment 1, the mean agency rating across tasks and roles was 36.13 (95% CI [25.77, 38.48]), indicating that participants experienced shared control, rather than independent control, when engaged in a cooperative joint action. Furthermore, participants' agency ratings differed depending on both task and role. Specifically, the linear mixed-effects model analysis revealed a significant effect of task, $F(2, 77.96) = 3.84$, $p = .026$, and role, $F(1, 112.93) = 7.97$, $p = .006$. However, there was no interaction, $F(2, 112.92) = 1.28$, $p = .28$. Given that the interaction was not significant, we compared goodness of fit (estimated using ML) for a model that did and did not include the interaction (-2LL = 21263.02 and 21265.61, respectively). Removing the interaction from the model did not significantly reduce the goodness of fit, $\chi^2(2) = 2.59$, $p = .27$. We therefore report the estimated mean differences between tasks and roles, along with confidence intervals and standardized effect sizes, from the final model that excluded the interaction in Table 2 (left side, labeled Model 1.1). As Table 2 shows, mean agency ratings did not differ significantly between the immediate-alternating and distant-alternating tasks. However, participants rated their control as significantly more shared for both alternating tasks compared to the sequential task. This pattern of results is consistent with the hypothesis that coordination symmetry influenced participants' agency ratings rather than temporal distinguishability of partners' contributions. In addition, consistent with Experiment 1, participants rated their feelings of control as more shared when they were the follower in the sequence compared to when they were the leader.

3.2.2. Objective coordination

Figure 4 shows the mean coordination score for each task, and Table 3 reports the estimated mean differences between tasks along with confidence intervals and standardized effect sizes. There was a significant effect of task, $F(2, 37.29) = 86.46$, $p < .001$, such that coordination was higher in the immediate-alternating task compared to the distant-alternating task and compared to the sequential task. Coordination was also higher for the distant-alternating task compared to the sequential task. This confirms that objective coordination did indeed differ across tasks (Step 2 of the causal steps approach).

Table 4 shows the model comparisons for the step-up strategy of adding objective coordination and its interactions with task and role to the model of agency ratings. The table shows that including objective coordination as a fixed factor in addition to task and role significantly improved model fit, whereas including the interactions between objective coordination and task and between objective coordination and role did not significantly increase the fit. We therefore re-estimated the mean differences in agency with a model that included task, role, and objective coordination (shown in the right half of Table 2, labeled Model 1.2). The main effect of objective coordination was significant, $F(1, 2390.63) = 21.79$, $p < .001$, as was the main effect of role, $F(1, 114.37) = 7.78$, $p = .006$. However, the main effect of task was no longer significant when objective coordination was included in the model, $F(1, 80.91) = 2.21$, $p = .116$.

As the right half of Table 2 shows, the estimated slope for objective coordination was negative, indicating that agency ratings decreased (became more shared) as objective coordination between partners increased. Furthermore, comparing the estimated mean differences in Model 1.1 to the estimated mean differences in Model 1.2 reveals that including objective coordination in the model reduced the size of the differences between all three tasks, with the largest reduction occurring for the difference between the immediate-alternating and sequential tasks. This pattern of results is consistent with the hypothesis that objective coordination mediated (at least some of) the effect of task on agency ratings.

3.3. Discussion

The goals of Experiment 2 were threefold. First, we sought to replicate the effects found in Experiment 1. We replicated the effect of coordination symmetry, such that participants rated their feelings of control as more shared in tasks that required symmetrical coordination than in a task requiring asymmetrical coordination. We also replicated the effect of role, such that leaders rated their feelings of control as more independent than followers. However, in contrast to Experiment 1, the interaction between task and role was not statistically significant. The second goal of Experiment 2 was to examine the possibility that the distinguishability of partners' contributions better explained the difference in agency ratings between the alternating and sequential tasks than did coordination symmetry. The pattern of results did not support this possibility; there was no significant difference in agency between symmetrical coordination that entailed less temporal distinction between partners' actions (i.e., the immediate-alternating task) and symmetrical coordination that entailed more temporal distinction between partners' actions (i.e., the distant-alternating task). Instead, both symmetrical coordination tasks yielded similar and more shared feelings of control than the asymmetrical coordination task (i.e., the sequential task).

Finally, the third goal of Experiment 2 was to examine whether objective coordination between partners' actions mediates the effects of task on agency ratings. Our findings indicated that the effects of task on agency ratings were reduced in size when objective coordination was included as a covariate. These findings are consistent with the hypothesis that objective coordination accounts for at least some of the effects of task on participants' ratings of agency.

4. General discussion

The current study examined people's sense of joint agency during cooperative joint action. In two experiments, pairs of people coordinated their actions to produce tone sequences that matched the pace set by a metronome. People reported feelings of joint agency (i.e., shared rather than independent control) over the timing of the sequence, providing the first direct evidence that people experience joint agency when they engage in cooperative joint action. Furthermore, people felt stronger joint agency a) when they produced sequences that required both people to coordinate their actions with each other (symmetrical coordination) compared to sequences in which only one person had to coordinate their actions with the other (asymmetrical coordination), and b) when they held the role of follower compared to leader within the joint action. Finally, people felt stronger joint agency when they were better coordinated with their partner during a given sequence, and this influence of coordination on agency accounted for some of the differences in joint agency brought about by differences in coordination requirements across sequences.

The current study is, to our knowledge, the first to ask people directly about their sense of joint agency over a joint action. Previous studies examining agency in joint action have used rating scales that focused primarily on self- or other-agency (e.g., Dewey & Carr, 2013; Obhi & Hall, 2011) or were ambiguous as to whether they referred to self-agency, joint agency, or a mixture of the two (e.g., Dewey et al., 2014; van der Wel et al., 2012). The current study asked people to indicate the degree to which they experienced shared rather than independent control over the timing of a jointly-produced action sequence. People rated their control as more shared than independent across different types of sequences, roles, and degrees of coordination between partners. This finding provides empirical support for philosophical accounts that posit that people will experience joint agency when they engage in cooperative joint action (Dokic, 2010; Pacherie, 2012; Seeman, 2009). This finding also supports Dewey et al.'s (2014) inference that people evaluate their sense of control at a group level (i.e., "we did it") and experience joint agency when they continuously coordinate their actions with each other to produce a joint action effect.

The current study also showed that people experience a stronger sense of joint agency for tasks that require both partners to mutually coordinate their actions with each other compared to tasks that require only one partner to coordinate their actions with the other. This finding is consistent with Pacherie's (2012) hypothesis that people will experience stronger joint agency when coordination is symmetrical (when both people adapt to each other's actions) compared to asymmetrical (when only one person adapts to the other). Joint actions that require mutual coordination encourage people to take each other's actions into account and shift to we-mode processing (Gallotti & Frith, 2013), whereas people may not take each other's actions into account to the same degree when they perform joint actions that require only one person to coordinate with the other. This finding also sheds light on Pokropski's (2014) proposal that people will only experience a sense of joint agency when they mutually coordinate with each other. In the current study, mutual coordination strengthened joint agency but was not required to induce it: People rated their agency as more shared than independent even when coordination was asymmetrical. Moreover, within asymmetrical coordination, leaders reported feeling joint agency even though they did not have to adapt to their partners' subsequent actions. Thus, our findings indicate that mutual adaptation is not strictly required to induce an experience of joint agency.

Experiment 2 showed that there was no difference in the strength of joint agency elicited by symmetrical coordination that entailed less temporal distinguishability between partners' contributions (i.e., when partners' actions were interleaved on every turn) and symmetrical coordination that entailed more temporal distinguishability between partners' contributions (i.e., when partners' actions were interleaved only every four turns). This finding at first seems to contradict Pacherie's (2012, 2014) hypothesis that people should experience stronger joint agency when their actions are more difficult to distinguish from their partner's. However, it is important to note that we manipulated temporal distinguishability at the level of the sequence (people's actions were more or less interleaved), but not at the level of the tones themselves (each person's tones were separated from their partner's nearest tone by approximately 400 ms). Temporal distinguishability may have a stronger impact on joint agency when people's action effects are closer together in time than they were in the current study. For example, differences in joint agency might be evident if interleaved tapping were compared with synchronous tapping, which would entail much smaller separation between partners' tones. A second possibility is that the distinguishability of partners' contributions only affects joint agency when people produce actions simultaneously or nearly simultaneously (e.g., when tapping in synchrony or working together to produce a single action effect, such as moving a single cursor across a screen). In such cases, physical or spatial distinguishability (e.g., whether people make the same or different movements, or move in the same or opposite directions) might have a larger impact on joint agency than temporal distinguishability. A final possibility is that distinguishability does not influence joint agency but instead determines whether the experience of joint agency is accompanied by intact or blurred self-agency. These possibilities remain important avenues for future research.

Experiment 2 also provided evidence that the influence of coordination symmetry on joint agency may be mediated by the degree of objective coordination elicited by symmetrical vs. asymmetrical coordination. Specifically, Experiment 2 showed that a) symmetrical coordination elicited a greater degree of objective coordination than asymmetrical coordination; b) the better coordinated the two members of a pair were on a given trial, the stronger their experience of joint agency; and c) differences in joint agency between symmetrical and asymmetrical coordination were reduced when the degree of objective coordination was controlled for. Previous research has shown that the better people are able to predict each other's actions, the better coordinated their actions are in time (Keller et al., 2007; Loehr & Palmer, 2011; Zamm et al., in press). Our findings show that the better coordinated people's actions are in time, the stronger their experience of joint agency. Thus, our findings are consistent with predictive accounts of agency in which the experience of agency depends on how well the predicted and actual consequences of an action match (Blakemore et al., 1999; Wolpert & Flanagan, 2001). More specifically, our findings are consistent with a predictive account of joint agency whereby joint agency is derived from predictions about both one's own and one's partner's actions (Pacherie, 2012). Our finding that objective coordination may mediate the effect of coordination symmetry also supports Pacherie's (2012) hypothesis that coordination symmetry enhances joint agency by increasing the degree to which people are able to make predictions about each other's actions. One possibility is that symmetrical coordination induces people to reduce the variability of their action timing to make their actions more predictable (cf. Vesper et al., 2011), which would facilitate precise objective coordination and strengthen joint agency. Consistent with this possibility, reduced variability of one's own and a partner's action timing are associated with stronger joint agency (Bolt & Loehr, 2016).

Joint agency also differed depending on people's role within the joint action. Leaders (people who produced the first tone(s) in the sequence) experienced weaker (more independent) joint agency compared to followers (who produced subsequent tones). This finding is consistent with previous research showing that people who initiate a joint action, even by a split second, perceive themselves as the leader of the joint action (Wegner & Sparrow, 2007) and experience a sense of self- rather than other-agency (Obhi & Hall, 2011). Whereas previous work has shown that people's role within a joint action influences their sense of causal initiation over a jointly-produced action effect (Obhi & Hall, 2011; Dewey & Car, 2013), our findings indicate that people's role within a joint action influences their experience of control over a continuous action sequence that unfolds over time. Our findings thus contribute to a more complete understanding of the influence of leader/follower roles on the sense of agency, which includes causal initiation but also encompasses control over unfolding action sequences (Gallagher, 2012; Haggard & Tsakiris, 2009; Pacherie, 2007).

One potential explanation for the finding that leaders experienced weaker joint agency than followers is that leaders may coordinate less with their partners. Previous research examining the emergence of spontaneous leader-follower relationships during joint tapping tasks has shown that leaders have increased frontal alpha suppression compared to followers, suggesting that leaders spend more resources self-processing rather than adapting to their partner's actions (Konvalinka et al., 2014). Similarly, leaders employ less error correction and focus more on their own tapping performance compared to followers (Fairhurst, Janata, & Keller, 2014). The possibility that leader-follower differences in joint agency may depend on the degree to which the leader or follower coordinates with their partner is supported by our finding that leader-follower differences in agency ratings depended on the coordination requirements within a given task. In Experiment 1, leaders felt weaker joint agency than followers when coordination was asymmetrical, but there was no difference between leaders and followers when coordination was symmetrical. Similarly, in Experiment 2, there was a larger numerical difference in agency ratings between leaders and followers when coordination was asymmetrical compared to symmetrical (see Figure 3), although the interaction did not reach statistical significance. Thus, asymmetrical coordination in which leaders were not required to coordinate with followers yielded larger differences in joint agency between roles, whereas symmetrical coordination that required leaders and followers to mutually coordinate with other yielded smaller or no differences in joint agency between roles.

There are two differences between Experiments 1 and 2 that, combined, may account for the smaller (and statistically non-significant) interaction between task and role in Experiment 2. First, in Experiment 2, two of the three tasks that participants performed required symmetrical coordination, whereas in Experiment 1, only one of two tasks required symmetrical coordination. Second, Experiment 2 employed longer sequences than Experiment 1 (24 vs. 8 tones per sequence, respectively). Spending a larger proportion of time on tasks requiring mutual coordination and producing longer sequences overall may have allowed people to better learn about their partner's action timing and adjust their own action timing accordingly. Although we focused our manipulations and analyses on the degree to which people coordinated with each other *within* trials, it is possible that people also adapted to each other's action timing *across* trials. For example, if one partner tended to produce their tones relatively fast compared to the pacing sequence, the other partner could have learned this over the course of the experiment and begun to produce their own tones more slowly to compensate. Thus, during asymmetrical coordination leaders may have adapted their own actions *in anticipation* of how the follower's action timing would unfold. This may have occurred to a greater degree in Experiment 2 compared to Experiment 1 because people spent more time overall adapting to their partner's actions in Experiment 2, resulting in smaller differences in joint agency between leaders and followers during asymmetrical coordination in Experiment 2. This explanation is consistent with Konvalinka et al.'s (2014) finding that partners become mutually adaptive over time during a joint tapping task. In their study, shorter sequences led to asymmetrical adaptation between leaders and followers (whereby followers adapted more to leaders than vice versa) whereas longer sequences resulted in more similar adaptation between leaders and followers. However, the influence of across-trial adaptation (compared to within-trial adaptation) on joint agency should be examined in more detail in future research.

The current findings suggest several other potential avenues for future research. First, future studies could examine people's experience of joint agency when they fail to achieve the shared goal of a joint action (e.g., on incorrect trials). We focused on correct trials because they provided the clearest test of whether our manipulations influenced joint agency. Specifically, focusing on correct trials allowed us to examine people's experience of joint agency independent of objective attributions of blame (e.g., if one person produced a tone noticeably late relative to the other tones) or more subjective attributions of blame, such as a self-serving bias to deny responsibility for failures (Miller & Ross, 1975; Whitley & Frieze 1985) or a group-serving bias to attribute failures to individuals rather than the group (Taylor & Doria, 1981). We also provided feedback about whether people successfully matched the metronome pace only after participants made their agency ratings. Thus, future research could also examine whether feedback provided before people make their agency ratings directly influences the experience of joint agency.

Second, future studies could examine the relationship between explicit measures of joint agency and implicit measures of agency in joint action. Whereas explicit measures use rating scales to directly probe people's judgments of agency, implicit measures of agency rely on perceptual differences between self- and externally-generated action effects (Dewey & Knoblich, 2014). Studies of agency during joint action have revealed dissociations between explicit and implicit measures. For example, Obhi and Hall (2011) examined both explicit ratings of agency and intentional binding, an implicit measure in which self-generated actions and effects are perceived as closer together in time than externally-generated actions and effects. They found that although partners' explicit ratings of agency were polarized to self- or other-agency, both partners demonstrated similar intentional binding. Other studies have likewise found comparable implicit agency between partners for actions produced in interactive contexts (Weiss, Herwig, & Schutz-Bosbach, 2011; Strother, House, & Obhi, 2010). These findings suggest that people may experience joint agency at a pre-reflective level during joint action (Obhi & Hall, 2011). Future studies could examine whether explicit measures of joint agency (as opposed to self- or other-agency) are associated with implicit measures of agency for jointly-produced action effects, which could indicate that implicit measures tap into joint agency rather than self-agency when people engage in joint action. Finally, future studies could use explicit measures of joint agency to examine whether the degree of control people experience over a joint action (e.g., more vs. less control) varies independently of the type of control they experience (i.e., shared vs. independent control). A challenge for this work will be to develop a measure (or set of measures) that can take into account variation along both dimensions of the agency experience.

5. Conclusion

In sum, the current study confirmed the theoretical prediction that people will experience joint agency when they engage in cooperative joint action. The current study also revealed that the strength of joint agency is determined in part by the degree to which people should, and do, mutually coordinate with each other's actions, as well as by people's role within the joint action. Understanding the factors that increase the strength of joint agency has implications for contexts in which people strive to achieve a sense of group cohesion. For example, Overy and Molnar-

Szakacs (2009) suggest that “the feeling of being together” is an important component of music therapy. Similarly, Carron, Shapcott, and Burke (2007) suggest that creating a sense of group identity in team sports contributes to group success. Although more research is needed to further elucidate the mechanisms that contribute to the experience of joint agency, our findings provide the first evidence that factors that encourage better coordination between the individuals engaged in a joint action also enhance their experience of joint agency over the joint action.

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Footnote

[1]Although researchers have proposed more direct methods for assessing mediation than the causal steps approach (e.g., bootstrapping and Monte Carlo methods; see Preacher & Selig, 2012), methods for applying these approaches to data with the multilevel structure reported here (i.e., repeated measures within participants who are nested within pairs) have not yet been established (see Bauer, Preacher, & Gil, 2006, for discussion of some of the issues involved in assessing mediation in multilevel models). We therefore employ the causal steps approach but limit our conclusions accordingly (i.e., we interpret our results as preliminary rather than strong evidence for mediation).

Table 1

Estimated mean differences in agency ratings between roles for each task.

	Mean diff. [95% CI], Cohen's *d*	*df*	*t*	*p*
Follower vs. Leader [a]				
Alternating	-2.0 [-5.8, 1.7], 0.13	32.5	1.1	.274
Sequential	-6.2 [-9.9, -2.5], 0.41	32.3	3.4	.002*
Alternating vs. Sequential				
Follower	-7.6 [-13.2, -2.0], 0.50	54.5	2.7	.008*
Leader	-11.8 [-17.3, -6.2], 0.77	54.5	4.2	<.001*

[a]Mean differences are defined as the second listed condition subtracted from the first listed condition (e.g., Follower – Leader).

$*p < .05$

Table 2

Estimated mean differences in agency ratings between tasks and roles for Models 1.1 and 1.2.

	Model 1.1: Task + Role				Model 1.2: Task + Role + Coordination			
	Mean diff. [95% CI], Cohen's *d*	*df*	*t*	*p*	Mean diff. [95% CI], Cohen's *d*	*df*	*t*	*p*
I-Alt vs. D-Alt[a]	-1.5 [-7.1, 4.2] 0.10	77.9	0.5	.61	0.5 [-6.2, 5.2] 0.03	81.4	0.2	.86
D-Alt vs. Seq	-6.0 [-11.6, -0.3] 0.39	77.8	2.1	.039*	-5.5 [-11.1, 2.1] 0.36	78.0	1.9	.059
I-Alt vs. Seq	-7.4 [-13.1, -1.8] 0.48	78.2	2.6	.011*	-4.9 [-10.7, 0.8] 0.32	83.6	1.7	.092
Foll vs. Lead	-4.0 [-6.8, -1.2] 0.26	114.5	2.8	.006*	-4.0 [-6.8, -1.2] 0.26	114.4	2.8	.006*
Coord	-	-	-	-	-10.3[b] -0.07	2390.6	4.7	< .001*

[a]I-Alt = Immediate-alternating; D-Alt = Distant-alternating; Seq = Sequential; Foll = Follower; Lead = Leader; Coord = Objective coordination. Mean differences are defined as the second listed condition subtracted from the first listed condition (e.g., I-Alt – D-Alt).

[b]Because objective coordination was a continuous covariate, the understandardized regression coefficient represents its slope rather than a mean difference, and we report the standardized regression coefficient as a measure of effect size.

**p < .05*

Table 3

Estimated mean differences in coordination between tasks.

	Mean diff [95% CI], Cohen's *d*	*df*	*t*	*p*
I-Alt vs. D-Alt[a]	.19 [.15, .23], 1.55	34.2	9.9	< .001*
D-Alt vs. Seq	.05 [.01, .09], 0.28	41.6	2.5	.016*
I-Alt vs. Seq	.24 [.20, .28], 1.66	37.4	12.2	< .001*

[a]I-Alt = Immediate-alternating; D-Alt = Distant-alternating; Seq = Sequential; Mean differences are defined as the second listed condition subtracted from the first listed condition (e.g., I-Alt – D-Alt).

$^*p < .05$

Table 4

Model comparisons for the effect of objective coordination on agency ratings.

Model Comparison	Change in *df*	Deviance (-2LL)	χ^2	*p*
Model 1.1		21265.61		
Model 1.2 [vs. 1.1]	1	21243.99	21.62	< .001*
Model 1.3 [vs. 1.2]	2	21242.27	1.72	.42
Model 1.4 [vs. 1.2]	1	21243.98	0.01	.92

Note.

Model 1.1 = Task + Role

Model 1.2 = Task + Role + Coordination

Model 1.3 = Task + Role + Coordination + Coordination*Task

Model 1.4 = Task + Role + Coordination + Coordination*Role

$*p < .05$

Figure captions

Figure 1. Schematic illustration of the sequence production task in Experiment 1. Following instructions and fixation, participants heard a series of isochronous pacing tones (illustrated by eighth note symbols) and then produced a sequence of tones (illustrated by combined button press and eighth note symbols, labeled A and B for the two participants, respectively). After producing the last tone, each member of the pair provided an agency rating. The pair then received feedback indicating whether the sequence they produced matched the pace set by the isochronous tones.

Figure 2. Estimated mean agency ratings (±*SE*) by task and role in Experiment 1.

Figure 3. Estimated mean agency ratings (±*SE*) by task and role in Experiment 2.

Figure 4. Estimated mean coordination (±*SE*) as a function of task in Experiment 2.

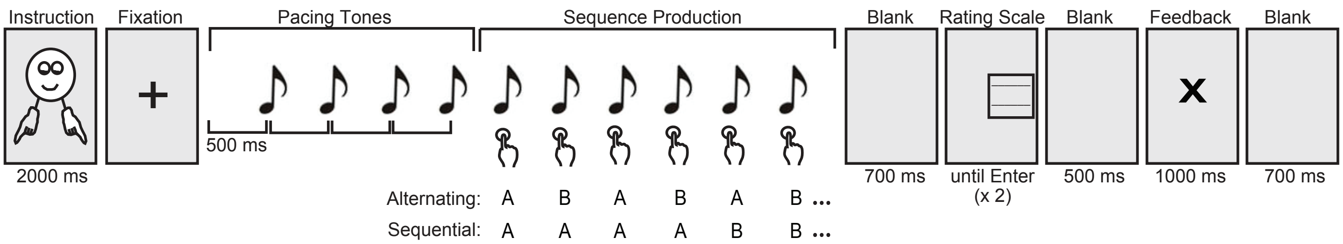
Instruction
Fixation
Pacing Tones
Sequence Production
Blank
Rating Scale
Blank
Feedback
Blank
+
x
2000 ms
500 ms
Alternating: A B A B A B ...
Sequential: A A A A B B ...
700 ms
until Enter (x 2)
500 ms
1000 ms
700 ms

Figure 1

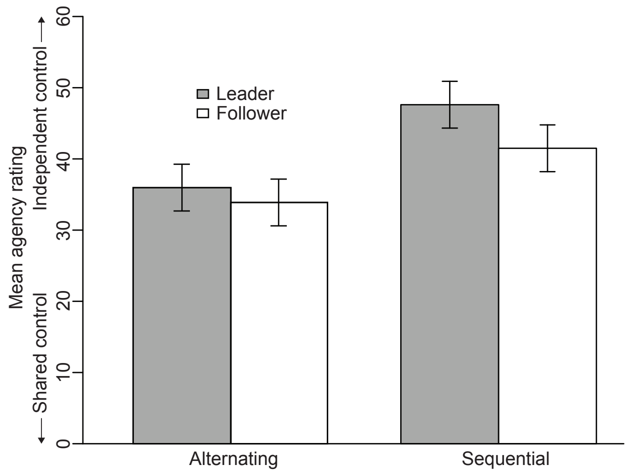
Mean agency rating
← Shared control
Independent control →
0
10
20
30
40
50
60
Leader
Follower
Alternating
Sequential

Figure 2

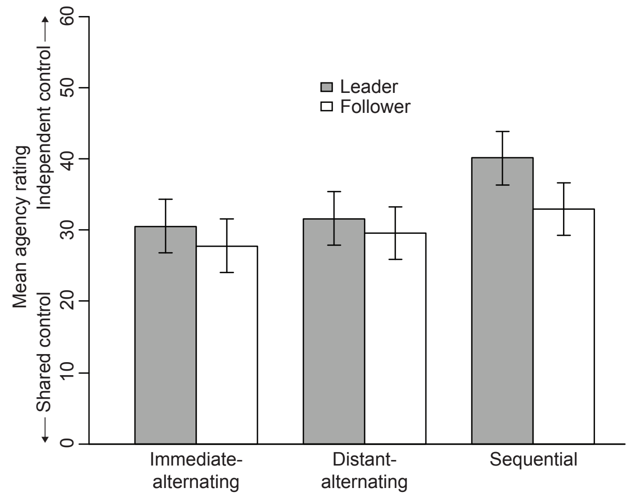
Mean agency rating
← Shared control
Independent control →
0
10
20
30
40
50
60
Leader
Follower
Immediate-alternating
Distant-alternating
Sequential

Figure 3

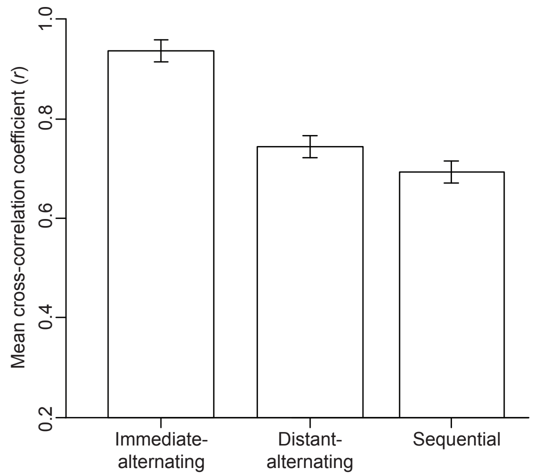
Mean cross-correlation coefficient (r)
1.0
0.8
0.6
0.4
0.2
Immediate-alternating
Distant-alternating
Sequential

Figure 4

Highlights

- We investigated the sense of joint agency during joint action.
- Partners coordinated actions and then rated their feelings of shared control.
- Mutual coordination elicited stronger joint agency than non-mutual coordination.
- Followers felt stronger joint agency than leaders.
- Better coordination between partners was associated with stronger joint agency.