Manipulations to practice organization of golf putting skills through interleaved matched or mismatched practice with a partner

April Karlinsky

and

Nicola J. Hodges[1]

School of Kinesiology

University of British Columbia

210-6081 University Boulevard

Vancouver, BC

V6T 1Z1

Canada

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[1]Corresponding Author:

Nicola J. Hodges

E-mail: nicola.hodges@ubc.ca

Telephone: 604-822-5895

Fax: 604-822-6842

Abstract

Alternating physical and observational practice with a partner for the same skill can benefit learning compared to practice alone. What is unknown is whether a partner's interleaved practice impacts multi-skill learning, when the partner either matches or mismatches the skill of their partner. We therefore studied dyad practice in a multi-skill learning protocol and manipulated partners' practice schedules of two golf putting skills. Partners either practiced the same ("matched") or different skills in alternation ("mismatched"). Based on previous research where demonstrations have induced contextual interference effects, we hypothesized that a mismatch between what is observed and physically practiced on consecutive trials should promote interference in practice and hence aid learning. A third control group was tested, where only one partner practiced while the other observed. All groups practiced for two days, with individual retention tests at the start of day 2 and one week later. Taking turns practicing and observing a partner did not benefit learning compared to pure physical practice and the matched and mismatched groups did not differ in outcomes. There was evidence, however, that partners were adapting their actions (i.e., compensating for over or undershooting of the target) based on the shots of their partner, in a similar manner to how they were adapting to their own errors. Thus, although partners were influencing each other's performance, it was not ultimately to the benefit (or cost) of overall learning. Partner-mismatching of skills through alternating practice was not sufficient to promote interference in practice and ultimately promote learning.

Keywords: dyad practice, contextual interference, observational learning, motor learning, motor skills, joint action

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1 Introduction

People often practice motor skills in social settings, with similarly skilled peers. In these contexts, individuals not only engage in their own physical practice, but they also have the opportunity to observe others. In such settings, we ask the question as to how practice should be organized in order to maximize learning for all co-learners? Here we limit our focus to practice in pairs (or 'dyads'), where partners take turns, such that they alternate physical practice trials with rest and observation of the partner. From an applied perspective, alternating practice has been proposed as a simple way to capitalize on the delays often required between physical practice attempts during training (e.g., when recovery periods are necessary between skill attempts, or when there is less equipment or fewer coaches than trainees; Shea, Wulf, & Whitacre, 1999; Simon & Bjork, 2002). From a theoretical perspective, testing if and how partners impact the learning of a peer helps to understand when and why demonstrations and dyad practice are effective alternatives to pure physical practice and may implicate theories related to practice organization.

Previous research has shown that taking turns with a partner can enhance the learning of a single skill compared to pure physical practice alone (e.g., Granados & Wulf, 2007; Shea, Wright, Wulf, & Whitacre, 2000; Shea et al., 1999; *cf.* Karlinsky & Hodges, 2018b). Taking turns allows individuals to observe a co-learner's practice in between their own physical practice attempts. These interleaved periods of observation are thought to promote additional information processing activities which impact subsequent practice and retention (e.g., Simon & Bjork, 2002; Wulf & Shea, 2002). Specifically, the peer observation involved in dyad practice is thought to afford the benefits of observing a learning model (e.g., Brown, Wilson, Obhi, & Gribble, 2010; Granados & Wulf, 2007; Shea et al., 1999). These benefits include the opportunity to engage in error-detection, problem solving, and strategy evaluation (e.g., Adams, 1986; Lee, Swinnen, & Serrien, 1994). These processes also contribute to observers' pick-up and understanding of action strategies that could be used to "solve" the requirements of the motor skill (e.g., Hodges & Franks, 2002, 2004; Horn & Williams, 2004). In addition to these potential informational gains, observing a learning model is also thought to afford motivational benefits. For instance, peer observation is thought to promote learners' sense of self-efficacy regarding their own motor performance, as watching similar others perform successfully (i.e., "vicarious experience") can help observers to believe they are also capable of achieving similar levels of competency (Bandura, 1977). Observing a peer is also thought to promote individuals' motivation to improve (e.g., Schmidt & Lee, 2014), potentially by adding a sense of competition to the practice context (e.g., McNevin, Wulf, & Carlson, 2000).

When partners take turns practicing a single skill, the action they observe matches what they have just done and what they are about to do (e.g., Granados & Wulf, 2007; Karlinsky & Hodges, 2018b; Shea et al., 1999, 2000). In such single-skill protocols, the partner's action and associated outcome feedback are therefore immediately relevant to the observer, helping to refine their own movements. According to schema theory (Schmidt, 1975, 2003), such observational practice with feedback would help parameterize the observer's own actions through experience of variability in a wide(r) range of parameters. It is unclear, however, how interleaved demonstrations impact learning in pairs when partners practice multiple skills. In these multi-skill cases, what the learner observes in a partner's trial may or may not be the same as their own next task.

Manipulating whether demonstrations either match or mismatch the movement to be executed next has been shown to impact the effectiveness of multi-skill learning, when individuals practice alone (e.g., Lee, Wishart, Cunningham, & Carnahan, 1997; Simon & Bjork, 2002). A protocol that has been used is one where errorless demonstrations were interleaved between an individual's physical practice trials (e.g., Lee et al., 1997; Simon & Bjork, 2002). In one study, participants followed a blocked (repetitive) or random practice schedule to practice three, 5-keystroke sequences, each with a different timing goal (Simon & Bjork, 2002). Demonstrations (computerized run-throughs of the correct key sequence and timing) depicting a task that either matched or mismatched the imminent task were presented before each physical practice trial to diminish or augment the degree of between-trial interference, respectively. Consistent with the typical contextual interference (CI) effect, random groups performed worse in practice but more accurately in retention (for a review of the CI effect, see Lee, 2012). Importantly, mismatched demonstrations, which served to bring observation-induced interference into practice, hence making them more 'random', also impaired acquisition but ultimately augmented retention, compared to interleaved matched demonstrations. Benefits in retention for mismatched demonstration groups were observed despite lower perceptions of competence for the mismatched as opposed to the matched groups (indexed by lower judgments of learning).

These data concerning interference-related benefits of interspersed demonstrations when learning multiple skills, provide evidence that demonstrations that promote between-trial interference and cognitive effort can enhance learning. This is also congruent with evidence of CI effects following observational practice of low versus high interference practice schedules (e.g., Blandin, Proteau, & Alain, 1994; Wright, Li, & Coady, 1997). Of note, Richardson (as cited in Guadagnoli & Lee, 2004) showed a different pattern of results when demonstrations were interleaved into the physical practice of keystroke sequences requiring complex relative timing goals (as opposed to the absolute timing goals used by Lee et al., 1997 & Simon & Bjork, 2002). In this case, matched models in combination with random practice actually *enhanced* learning, yielding lower error on both immediate and delayed tests of retention compared to pure blocked and random practice. These findings suggest that matched demonstrations, which ease practice by providing the "solution" to the task, may be more beneficial for the learning of more nominally difficult skills (although mismatched demonstrations were not tested for comparison).

We have recently shown that a partner's interleaved practice schedule impacted self-controlled practice organization for a keypress, absolute-timing task, but the effects were more noticeable in decision-making behaviours rather than learning outcomes (Karlinsky & Hodges, 2018a). Partners also switched turns after blocks of 9 trials (rather than on every trial) in order to make their practice organization salient (i.e., either blocked or random). However, this block-to-block form of taking turns prevented insight into if and how a partner's practice influences performance of the partner, with respect to error compensation or augmentation on the next trial. Consequently, we do not know how observing a partner's practice impacts the learning of multiple skills, when partners switch turns physically practicing on consecutive trials and their interleaved practice schedules either augment between-trial interference (through mismatching of skills) or minimize such interference (through matching of skills). The primary aim of the current study was to address this question.

Individuals practising alone are biased by their own previous motor responses. For example, when aiming in alternation to targets with amplitudes of 20° and 60°, people tended to overshoot the shorter distance target and undershoot the longer distance target (e.g., Sherwood, 2010; Sherwood & Fosler, 2013; Sherwood & Rothman, 2011). In paired action contexts, a growing body of research also provides evidence that individuals monitor a partner's performance and adapt their own behaviours in response to observed errors in a peer, similar to as if they were their own (e.g., de Bruijn, Mars, Bekkering, & Coles, 2012; de Bruijn, Miedl, & Bekkering, 2011; *cf*. Picton, Saunders, & Jentzsch, 2012). As an illustrative example, individuals have been shown to exhibit "post-error slowing" following both their own and a co-actor's errors in reaction time tasks (e.g., de Bruijn et al., 2011, 2012). Thus, a secondary aim of our study was to assess how observing a partner's errors in a practice context impacts the co-learner's performance on the next trial and whether they potentially correct for a partner's errors similar to how they would their own.

In summary, there is evidence that alternating physical and observational practice with a partner can enhance the learning of a single skill compared to pure physical practice (e.g., Shea et al., 1999, 2000), and that interleaved demonstrations can impact the effectiveness of multi-skill practice schedules for individuals (e.g., Lee et al., 1997; Simon & Bjork, 2002). In the current study, we report a novel dyad practice protocol wherein we manipulated the relationship between what is observed and physically practiced on consecutive trials. A golf putting task was used with two different putters requiring standing or seated putting. Partners either used the same putter as their partner or used a different putter on consecutive trials. In the former case, there was a match between what was observed and performed, minimizing between-person interference and making practice easier, but potentially at the cost of learning. However, in this matched case, there would be immediate opportunities to learn from a partner's performance to aid with execution of the upcoming skill. We expected to see evidence of "post-error compensation" in the matched condition, where over or undershooting behaviours in the partner would be “corrected” for in the partner by opposite patterns of errors. In the latter case, where partners perform and observe different skills in alternation, observation-induced, between-person interference would be high. As such, we would expect practice to be more cognitively effortful, potentially aiding long-term retention. However, the information gained from watching should be less, given that a partner’s performance and feedback would not be immediately relevant to the current skill. This should show up in between-trial behaviours of partners, where compensatory-type errors are not observed (or are not as pronounced) as in the matched pair group. We additionally tested a control dyad group, where partners either just practiced or just watched, thus controlling for the presence of a partner (for a review of “audience effects”, see Hamilton & Lind, 2016). We anticipated that physical practice would enhance learning compared to observational practice (e.g., Blandin et al., 1994; Karlinsky & Hodges, 2014; Shea et al., 2000; Wright et al., 1997), but that alternating practice with a partner would be more effective than pure physical practice (e.g., Granados & Wulf, 2007; Shea et al., 1999, 2000).

As a final aim, we sought to gain insight into learners’ subjective experience of the practice conditions. Because there is some suggestion that mixing physical and observational practice has a motivational benefit (e.g., Schmidt & Lee, 2014; Shea et al., 1999), participants answered questions about interest/enjoyment (indexing motivation), competence, choice, and pressure/tension (Intrinsic Motivation Inventory, n.d.). We hypothesized that the alternating pairs would rate their interest/enjoyment of the practice experience more positively than individuals engaging in just physical practice. It is possible, however, that taking turns with a partner would also augment the pressure/tension experienced than the more independent physical practice control condition (e.g., Rhea, Landers, Alvar, & Arent, 2003) and that mismatched practice would be associated with lower perceptions of competence (see Simon & Bjork, 2002).

Participants additionally responded to a customized paired practice experience questionnaire, probing their desire to outperform their partner (see McNevin et al., 2000) as well as other perceptions of the practice experience.

2 Methods

2.1 Participants

Seventy-six right-handed females (*M* = 22.1 yr, *SD* = 5.3) volunteered to participate and were paid $10/hr. We chose to restrict participant eligibility to females in order to limit any potential within-dyad (e.g., Carli, 1989; Harskamp, Ding, & Suhre, 2008) or participant-experimenter sex-related effects (e.g., Rumenik, Capasso, & Hendrick, 1977), as a female experimenter conducted the data collection. There is also some evidence that joint (dyadic) action effects are larger for same-sex pairs compared with opposite-sex pairs (Mussi, Marino, & Riggio, 2015; van der Weiden, Aarts, Prikken, & van Haren, 2016). All participants had normal or corrected-to-normal vision, no known neurological or motor disorders, and limited golf putting experience. Informed consent was obtained, and the study was conducted in accordance with the Behavioural Research Ethics Board of the University.

2.2 Groups and Partners

Participants were strangers and paired based on chance and availability. Pairs were assigned to either the matched, mismatched, or control group (*n* = 12 pairs/group). Two additional pairs were tested but were excluded post-testing due to experimenter mistakes in the order of testing and participant ineligibility due to previous golf experience. Within the matched and mismatched groups, one member of each pair was randomly assigned to be Partner 1 (P1), which indicated that they would perform the first trial (and all subsequent odd number trials) of the acquisition sessions. The other member of the pair was assigned to be Partner 2 (P2), which meant that they would perform the second trial (and all subsequent even number trials) of the acquisition sessions. The order of practice trials for P1s and P2s as a function of group is shown in **Figure 1** (note for brevity, we have only shown the first 36 trials out of 72 trials/day, but this order was repeated for the last 36 trials). We manipulated the putting schedule of the matched and mismatched group P1s such that the selected putter (standard or miniature putter) would serve as a demonstration of the upcoming putt for P2s in the "matched" group, but be a demonstration of a different putt, with a different putter, for the "mismatched" group. All P2s practised in the same practice order regardless of group (see **Figure 1**). Importantly, both partners within a group practised under the same matched or mismatched conditions in a semi-blocked order of trials, except that P2s matched their partner's task (i.e., putter) on every trial (or mismatched, depending on group). Because P1s went first, on the first trial of every block of new trials they did not match (or mismatch; 14% of trials).

Within the control group, one member of each pair was assigned to be the "Actor", who physically practiced during the acquisition sessions, while the other partner was assigned to be the "Observer", who watched the Actor's acquisition sessions. Actors completed the same order of trials and putters as Partner 2s.

2.3 Task and Apparatus

Participants had to putt a golf ball so that it stopped in the centre of a target. Two different right-handed putters were used: a "standard" putter (91 cm) and a "miniature" putter (40 cm). The standard putter was used standing, sideways to the target and the miniature putter was used seated, facing the target. Standard white golf balls were used throughout the study. Putts were made on a flat green felt surface (2 m wide × 7 m long) to a target (black cross) 150 cm away. Participants wore Plato liquid crystal occlusion glasses (Translucent Technologies, Ontario) and ear protectors (3M Optime 101) during the no-feedback (FB) pretest and retention tests.

2.4 Materials

We used two questionnaires. The Task Evaluation Questionnaire (TEQ) is a 22-item version of the Intrinsic Motivation Inventory designed to assess interest and attitudes toward an experimental task (Intrinsic Motivation Inventory, n.d.).[1] The TEQ consists of four subscales: interest/enjoyment (7 items), perceived competence (5 items), perceived choice (5 items), and pressure/tension (5 items). The wording was modified for golf putting and items were answered on a 7-point, Likert scale (1 = *not at all true* to 7 = *very true*). We also had a customized paired practice experience questionnaire which included three questions (detailed in the Results, **Table 3**) which were rated on the same 7-point Likert scale for truthfulness.

2.5 Procedure

Details regarding the experimental phases are provided in **Table 1**. The study was conducted over three days, with days 1 and 2 partially completed in pairs and "day 3" (one week later) completed alone. On day 1, partners introduced themselves and were allowed time to chat. They were informed that their goal was to learn how to hit the golf ball with two different putters, so that it landed as close as possible to the target centre. Instructions on how to hold the standard and miniature putters were provided and the experimenter demonstrated the general technique without hitting a ball.

Following a short familiarization phase (3 attempts/putter with order counterbalanced), participants completed a 6-trial pretest without feedback (3 trials/putter with order counterbalanced, wearing occlusion glasses and ear protectors). Outcome feedback was removed to prevent learning in these pretest trials. The experimenter manually occluded the occlusion glasses upon ball-strike. The ball's distance from the target was recorded and the ball was removed before restoring the participant's vision. Partner 1 (P1) always performed the familiarization and pretest first, while P2 responded to questions regarding their previous golf-related experiences (e.g., number and approximate dates of prior miniature golf, 9-hole, and 18-hole golf experiences)[2] and a handedness questionnaire (Oldfield, 1971) in another room. These questionnaires were used to confirm the eligibility criteria of limited golf experience and right-hand dominance.

In acquisition, partners in the matched and mismatched groups were instructed to watch one another's practice from a designated observation spot (from the side and near the target so they could see both the putting action from the side and the ball's outcome), but not to communicate about the putting tasks. They were encouraged to pay attention to their partner's practice trials to help them learn, given that they would be tested using both putters the following day/week. Partners alternated turns for a total of 72 trials (36 physical practice trials and 36 observation trials per partner), with P1 putting on trial 1 and P2 using the same putter (matched group) or different putter (mismatched group) on trial 2 etc. Whether P1 used the miniature or standard putter first was counterbalanced (see **Figure 1** for sample practice schedules). Although partners alternated turns on each trial, each participant followed a semi-blocked physical practice schedule. Participants used their assigned putter for six trials before switching to the other putter, for a total of 6 physical practice blocks per partner (3 blocks/putter). Outcome feedback was given on every trial.

In the control group, Observers watched their Actor partner practice from the same observation location as the pairs in the matched and mismatched groups. Observers were also encouraged to attend to the Actor's practice and told they would also be tested on the putting skills the following day/week. The Actors' inter-trial breaks were controlled to match the timing of the other two groups. As such, Actor controls only differed from the other two groups in terms of the opportunity to observe their partner between practice attempts. They were exactly matched to the putter practice order of the P2s.

Day 2 of the study took place ~24 hours later and began with two retention tests conducted alone; without vision (identical to the pretest) and then with vision. The no-vision retention test was always completed first to allow us to assess how well the golf putting skills were retained while preventing further learning. The subsequent, with-vision test matched the conditions of practice and provided an index of participants' capabilities without the potentially perturbing effects associated with the removal of vision. Each retention test comprised 12 trials, where participants used each putter for three trials in a row before switching to the other putter (counterbalanced order). Participants then completed another period of acquisition, identical to day 1. Afterwards, all participants (except the Observers) completed the Task Evaluation Questionnaire (Intrinsic Motivation Inventory, n.d.), responding with respect to how they generally felt during both paired practice sessions.

Day 3 of the study took place ~1 week after day 2 and was completed individually. Participants completed the same two retention tests as performed on day 2 and responded to a paired practice experience questionnaire, which consisted of three questions about the practice experience (full text questions are presented in the Results, **Table 3**). Participants were then fully debriefed and compensated for their time.

2.6 Measures and Analysis

2.6.1 Error

The distance of the ball to the target centre was measured as constant error (CE) in the x- and y-dimensions after every trial. These data were transformed into radial error (RE) and bivariate variable error (BVE) for two-dimensional measures of accuracy and consistency, respectively (see Hancock, Butler, & Fischman, 1995). RE was calculated for each trial based on the CE in the x- and y-dimensions using Pythagoras' theorem: RE = $(x^2 + y^2)^{1/2}$. BVE was calculated as a standard deviation (SD) measure across two dimensions, given by the square root of a participant's k shots' mean squared distance from their centroid (x_c, y_c), where the centroid is defined as the positionally typical shot (average x and y position) for a participant's block of trials: BVE = $\{(1/k)[\sum_{i=1}^{k}(x_i - x_c)^2 + (y_i - y_c)^2]\}^{1/2}$.

Our primary analyses were on testing for differences between the matched and the mismatched groups (n=24/group). Secondary analyses were conducted to compare the Actors who did not alternate with a partner to the Partner 2s only (n=12/group). We also compared the Actors to the Observers, mostly for descriptive purposes, to determine any benefits from pure observation in this golf putting task (the absence of a no-practice control group prevented a strong test of this question).

Errors for the matched and mismatched groups were compared in 2 Group (matched, mismatched) × 2 Partner (P1, P2) × 2 Putter (standard, miniature) mixed ANOVAs, with repeated measures (RM) on the last factor. We mostly included Putter as a blocking variable as participants were more accurate with the standard than miniature putter in all phases of testing. Only relevant group interactions for putter are discussed. Separate analyses were conducted for each error measure (RE and BVE), for the Pretest, Acquisition, and Retention phases. Acquisition data were grouped into 6-trial blocks (one putter/block), resulting in an additional RM factor of Block. Linear trend analysis was conducted on this Block factor. In retention, an additional RM factor of outcome feedback was included. In secondary analyses, comparisons were also made across the P2s and Actors only, and across the Observers and Actors. The same tests as above were used, with the group factor reflecting these additional comparisons.

2.6.2 Behaviours

Compensatory (or corrective) behaviours were defined and measured in practice. These behaviours were first determined based on whether an overshoot or undershoot of the target in the y-dimension resulted in compensation (i.e., a shorter or longer putt, respectively) on the subsequent trial for that individual (self-referenced correction) and/or for their partner (partner-referenced correction). These data (percentages) were analyzed in a 2 Group (matched, mismatched) × 2 Partner (P1, P2) × 2 Day (Day 1, Day 2) × 2 Correction-type (self-referenced, partner-referenced) mixed ANOVA, with RM on the last two factors. For comparison, we also analyzed the percentage of self-referenced corrections of the Actor group and compared these statistically to the Partner 2s from each of the dyad groups, in a 3 Group × 2 Day mixed ANOVA.

2.6.3 Questionnaires

We used Cronbach's alpha to assess the internal consistency of the items within each subscale of the Task Evaluation Questionnaire (TEQ), to provide an index of each subscale's reliability. For the current sample, Cronbach's alpha values were good for the interest/enjoyment (α = .93), perceived competence (α = .86), and pressure/tension (α = .86) subscales; however, reliability for the perceived choice subscale was weak (α = .69), so we do not report these data. Participants' average scores for each subscale of the TEQ were submitted to separate ANOVAs. We first tested for differences between the matched and mismatched alternating groups using separate two-way ANOVAs for each TEQ subscale, with Group (matched, mismatched) and

Partner (P1, P2) as the between-factors. Similar analyses were run on the paired practice experience questions. Secondary analyses of the TEQ subscales was performed comparing the Actors to the matched and mismatched Partner 2s for each subscale (one-way ANOVAs).

For all analyses Greenhouse-Geisser corrections were applied to the degrees of freedom for violations to sphericity. Significant effects and interactions were followed up with Tukey's HSD procedures (all *p*s < .05 reported). Partial eta squared (η_p^2) and Cohen's *d* values are reported as measures of effect size for ANOVAs and *t*-tests, respectively. Power values (1 – ß) are given for non-statistically significant effects where $F > 1$. We only report full results when $F > 1.5$.

3 Results

3.1 Error

To summarize the results for error, although there was evidence of improvement in groups, there were no significant group differences in acquisition or retention. Detailed analyses for relevant comparisons are given below.

3.1.1 Pretest

The matched and mismatched groups' mean RE (indexing accuracy) and BVE (indexing consistency) in testing are presented in **Figure 2**. The groups did not differ on RE ($F = 1.1$; **Figure 2A**). There were also no Partner differences, $F(1, 44) = 2.41$, $p = .13$, 1 – ß = .33; but the Group × Partner interaction was close to accepted significance levels, $F(1, 44) = 3.86$, $p = .056$, 1– ß = .48. In the mismatched group, there was a trend for P2s to be more accurate than P1s.

The BVE data (**Figure 2B**) showed the same pattern of results (no differences between groups or partners with respect to consistency, *F*s < 1; Group × Partner, $F(1, 44) = 1.67$, $p = .20$, 1 – ß = .24).

Comparisons between the Actors and Partner 2s (n=12/group) showed no initial between-group differences in RE (F = 1.4) or BVE, $F(2, 33) = 1.73$, $p = .19$, 1 – ß = .34 (see **Figure 3**). Similarly, as illustrated in **Figure 4**, comparisons of the Actors with the Observers showed no differences in pretesting in terms of RE, $F(1, 22) = 2.06$, $p = .17$, 1 – ß = .28, or BVE ($F < 1$).

3.1.2 Acquisition and retention

As illustrated in Figure 5, participants in the matched and mismatched groups improved their accuracy (RE) and consistency (BVE) across blocks on day 1, confirmed by linear trends (*p*s < .001), and for RE but not BVE on day 2 ($p = .004$). These data are illustrated in **Figure 5**. However, the groups did not differ on either day of practice (*F*s < 1). There were also no partner-related effects during practice, with respect to putting accuracy (RE: *Fs* < 1) or variability (BVE: day 1, $F(1, 44) = 2.69$, $p = .18$, 1 – ß = .36), nor any Group × Partner interactions (*F*s < 1).

There were also no group-related differences in retention, as illustrated in **Figure 2**. For RE on day 2, Group × Partner, $F(1, 44) = 1.52$, $p = .22$, 1 – ß = .23. For BVE, all *F*s < 1. Having vision and outcome feedback aided accuracy (lower RE), $F(1, 44) = 44.44$, $p < .001$, $\eta_p^2 = .50$, and consistency (lower BVE), $F(1, 44) = 13.92$, $p = .001$, $\eta_p^2 = .24$. When tested one week later on “day 3”, there were again no group or partner-related retention effects with respect to RE or BVE (*F*s ranged from < 1 – 1.4).

Secondary analysis of the Actor group, in comparison to the Partner 2s from the alternating dyad groups, also failed to show differences in acquisition (not shown) and retention (**Figure 3)**. Only on day 1 of practice were the group-related F values >1.5, due to trends for the Actor group to perform with greater accuracy (lower RE), $F(2, 33) = 2.91$, $p = .069$, 1 – ß = .53, and consistency (lower BVE), $F(2, 33) = 2.77$, $p = .077$, 1 – ß = .51.

Comparisons of the Actors to the Observers showed only trends for the Actors to be more accurate and consistent. As shown in **Figure 4**, on day 2 retention, the Actors had lower RE and BVE, although neither difference was statistically significant, RE: $F(1, 22) = 3.93$, $p = .060$, 1 – ß = .48, BVE: $F(1, 22) = 3.43$, $p = .077$, 1 – ß = .43. Similar trends were seen the following week, although again no significant group differences for both RE, $F(1, 22) = 2.74$, $p = .11$, 1 – ß = .35, and BVE, $F(1, 22) = 1.38$, $p = .25$, 1 – ß = .20. The only other group difference was a Group (Role) × Putter interaction for BVE, $F(1, 22) = 5.07$, $p = .035$, $\eta_p^2 = .19$. Actors were more consistent than observers when using the miniature putter.

3.2 Behaviours

As illustrated in **Figure 6**, all participants showed a high frequency of compensatory behaviours (~70-76% of trials). The frequency of corrections was similar for self-referenced corrections (increasing or decreasing the shot if the target was under- or over-shot on the person’s previous trial) and partner-referenced corrections (increasing or decreasing the shot if the target was under- or over-shot on the partner’s preceding trial). Statistical analysis did not yield differences between self- and partner-referenced compensations ($F < 1$). Contrary to expectations, there were also no group-related effects (all *F*s < 1 with the exception of Group × Day × Correction-type, $F(1, 44) = 2.07$, $p = .16$, 1 – ß = .29). There was an effect of Partner for this analysis, with the Partner 2s, who followed Partner 1s, compensating more frequently (M = 74%, SD = 7%), than their leading Partners (M = 71%, SD = 14%), $F(1, 44) = 6.34$, $p = .016$, $\eta_p^2 = .13$. Although there was an interaction with Correction-type, $F(1, 44) = 6.43$, $p = .015$, $\eta_p^2 = .13$, this was not a result of more partner-related compensations. Rather P2s made more self-referenced corrections (M = 76% of trials, SD = 7%) than P1s (M = 68%, SD = 11%).

When looking at the percentage of compensation behaviours for the Actors, a similar percentage of corrections was seen as noted for the dyad groups (*M* = 74% of all acquisition trials, *SD* = 7%). Statistical comparisons of the Actors with the Partner 2s did not yield group differences (Fs < 1; Group × Day: $F(2, 33) = 1.21$, $p = .31$, 1 – ß = .25; data not shown).

Because such corrective behaviours cannot definitively be attributed to a person's preceding physical practice trial or an observed partner's trial, we conducted a descriptive analysis to help further determine whether corrections were likely self- or partner-referenced. Four types of corrections were identified as illustrated in **Figure 7**. Putts were classified based on the presence of a corrective behaviour in both the individual and their partner (a), the absence of a corrective behaviour in both partners (b), the presence of corrective behaviour in just the person putting (c, self-only), or corrective behaviour in just the partner (d, partner-only). The vast majority of trials showed compensatory behaviours which could be classed as both self- and partner-referenced (57%). The percentage of trials which showed only partner-referenced corrections or only self-referenced corrections, although markedly smaller, did not differ (17% for each).

3.3 Questionnaires

Mean responses to the Task Evaluation Questionnaire subscales are presented in **Table 2**. Practicing under matched versus mismatched conditions did not differentially impact perceptions. Both groups generally reported medium to high ratings for interest/enjoyment and competence, and low ratings for perceived pressure/tension (interest/enjoyment: Group, $F(1, 44) = 1.91$, $p = .17$, 1 – ß = .27; pressure/tension: Partner, $F(1, 44) = 2.53$, $p = .12$, 1 – ß = .34; all other Fs < 1.1). There were also no significant differences between the Actors and the Partner 2s from the alternating dyad groups (Fs < 1).

The paired practice experience questionnaire items and results are presented in **Table 3**. Regardless of whether schedules were matched or mismatched, watching a partner's practice was rated as moderately helpful (Group, $F(1, 44) = 1.83$, $p = .18$, $1 – ß = .26$), and not interfering (Group, $F(1, 44) = 2.11$, $p = .15$, $1 – ß = .30$), for participants' own performance/learning. In terms of the desire to be more accurate than their partner, ratings were relatively high and again these did not vary as a function of group (all Fs < 1).

4 Discussion

Our primary aim was to assess if and how practice with a partner who engages in the same (matched) or different order of trials (mismatched) impacts motor learning. We hypothesized that alternating turns with a partner who practices a different skill (mismatched) should promote between-person, observation-induced interference, which would facilitate retention in comparison to matched-skill practice (Lee et al., 1997; Simon & Bjork, 2002). However, contrary to our hypotheses, the matched and mismatched alternating practice groups showed essentially the same outcomes, in terms of errors in acquisition and retention and perceptions of the practice experience. Although we discuss potential reasons for the lack of difference below, it may just be that observation-induced interference in the form of alternating practice for novices is not sufficient to bring about differences in performance and learning when the actual physical practice experiences for these participants remains the same across groups.

In addition to a lack of difference between groups that alternated turns practicing and observing, these groups did not outperform an Actor group who essentially practiced alone with a passive observing partner. This conflicts with results from some previous dyad learning literature, where alternating practice enhanced the learning of a single skill compared to practice alone (e.g., Granados & Wulf, 2007; Shea et al., 1999, 2000; yet see Karlinsky & Hodges, 2018b). Given that we controlled for inter-trial intervals in this design as well as social facilitation/audience effects potentially associated with having a partner watch, suggests that previous benefits of dyad practice on learning may be related to these inter-trial rest or social facilitation factors.

Despite the fact that there were no outcome-related differences between groups, there was evidence that pairs were sharing in the practice experience. Looking at the alternating behaviours of the dyads, there was evidence of compensation between and within individuals. Overshooting (or undershooting) the target on trials was equally likely to lead to a shorter (or longer) putt on the next turn regardless of whether it was in response to the person's own previous trial or their partner's. Although it is difficult to know exactly how many of the trials were actual compensations, these behaviours were seen on ~70-76% of all trials and were of a similar frequency regardless of whether they were compensations to one's previous trial or to a partner's previous trial. When we looked at trials which could be classified as only self- or only partner-referenced compensations, although much lower (17% for each), these types of compensations were not different. Although there was some indication that the Partner 2s who always followed the partner on each block of trials showed a higher frequency of compensations, this was not a result of more compensations to their partner's errors.

These results related to partner-referenced behaviours are in line with research showing that co-actors monitor each other's performance and adapt their behaviours in response to a partner's errors and/or task constraints, similarly to as if they were their own. For example, in addition to post-error slowing (e.g., de Bruijn et al., 2011, 2012), individuals have been shown to adjust their own movement kinematics (to lift their arm higher) when they know a co-actor needs to clear an obstacle (e.g., Schmitz, Vesper, Sebanz, & Knoblich, 2017; van der Wel & Fu, 2015).

Of note, there were no costs to demonstrating partner-referenced compensatory behaviours in the current practice context, but it is possible that adapting in response to observed errors could have negative implications in other performance settings (e.g., de Bruijn, Miedl, & Bekkering, 2008). For instance, if platform divers or gymnasts (unnecessarily) "corrected" their own actions after viewing a peer over- or under-rotate a flip, this could be detrimental to their own success (or worse, their safety). Determining when and why learners are susceptible versus resistant to partner-related behavioural adaptations will be important for better understanding the potential costs and benefits of shared learning conditions.

The addition of the Actor group that did not alternate, but was observed, allowed us to compare physical versus observational practice for these golf putting skills. As anticipated, the Actors generally outperformed the Observers on the retention tests, consistent with previous research (e.g., Blandin et al., 1994; Karlinsky & Hodges, 2014; Shea et al., 2000; Wright et al., 1997). The results also suggest that observational practice aided skill development, in view of the similarity between the Actors' and Observers' performance on the final, with-feedback retention test. This is in line with research showing that benefits of observation are not fully appreciated until individuals have had the opportunity to receive knowledge of results (KR) on their own performance (which the Observers received during the day 2 with-feedback retention test; e.g., Andrieux & Proteau, 2013; Deakin & Proteau, 2000). Although the inclusion of retention tests, both without and with KR, allowed us to assess learning for observers once they received feedback on their own performance, this did render the ratio of practice to retention trials relatively high. Even though learning should have been prevented on the no-KR trials and the KR trials allowed us to assess learning specific to the conditions of practice, larger final differences between groups (especially Observers and Actors) might have been expected if these test trials had been limited. In the future, it may be of interest to try and enhance the experience of the observing partner in such dyad learning scenarios. For example, observers could estimate the outcomes of their partner's actions (e.g., Andrieux & Proteau, 2014; Ikegami & Ganesh, 2014), or make practice-related decisions for a partner (e.g., Karlinsky & Hodges, 2014; McRae, Patterson, & Hansen, 2015), or perhaps engage in practice observation under the belief they will have to teach another learner (see Daou, Buchanan, Lindsey, Lohse, & Miller, 2016).

It is important to consider potential factors that differed between the current study and previous research, which may have contributed to the pattern of results and specifically the lack of group differences in outcome scores. Here we used relatively complex golf putting skills, as opposed to relatively simple keystroke timing tasks which have yielded the strongest contextual interference (CI) effects and influence of observation or interleaved demonstrations on learning (Lee et al., 1997; Simon & Bjork, 2002; Wright et al., 1997). It may be that interference-enhancing protocols (such as mismatched demonstrations) impact more on simpler, laboratory-type tasks, as compared to more complex, applied skills, because the former are intrinsically less interesting and challenging (e.g., Brady, 2004; Guadagnoli & Lee, 2004; Lee & White, 1990; Wulf & Shea, 2002). Because of the relative difficulty of putting compared to attaining timing goals on a keypad, we also required our participants to practice only two skills rather than three or more, which is more typical in CI research. There is evidence that CI effects arise when practice involves just two different tasks (e.g., Maslovat, Chua, Lee, & Franks, 2004; Simon, 2007) and also for putting skills (e.g., Guadagnoli, Holcomb, & Weber, 1999; Hwang, 2003). As such, although task differences may be a reason for a lack of group effects as a result of a partner's schedule, it may be that observation-induced interference effects, especially as brought about by the schedule of another person, are not strong enough to impact on individual motor learning.

Another difference and potential explanation for the equivocal effects of matched versus mismatched practice is that here we used a real person as the model, who was also learning the task and making errors, as compared to the computer-based models of perfect performance used to-date (Lee et al., 1997; Simon & Bjork, 2002). Thus, the partner’s trial could act both as a template/reference for what to do – or not to do, depending on the outcome – as well as serve as feedback. Indeed, in other studies of dyad practice where individuals took turns practicing a balance task (Shea et al., 1999) or a complex cup-stacking task (Granados & Wulf, 2007), taking turns benefited learning over individual practice, even though participants were continually matching their partner’s task. Alternatively, it is possible that learning models in this multi-skill learning protocol brought too much interference into practice, hindering learning of this task. Further testing of this factor is required, whereby individuals are either paired with similarly skilled novice peers or with partners who are more experienced.

Surprisingly, the mismatched group did not show higher ratings in terms of their perceptions of practice with respect to the degree of interference experienced as compared to the matched group. Neither did the matched group perceive watching their partner to be more helpful than that reported by the mismatched group. It may be that observing a partner’s practice is helpful (and perceived as such) regardless of whether it matches or mismatches one’s own next to-be-performed task, so long as it is practice-relevant. There is some evidence supporting this in terms of the patterns of between-person compensatory errors which were observed, regardless of group. One way to test this idea in the future would be to pair learners who were each assigned to learn different skills. Whether or not observing non-practice relevant demonstrations is detrimental to learning (and/or whether such exposure affords any incidental learning of the co-learner's task(s)) would have implications for the design of applied learning conditions. In a similar vein, assigning co-learners to practice different skills (so that partners receive only physical or observational practice of each task), but with the knowledge they will eventually be tested on both, would also provide insight into the learning that can be achieved via shared practice.

In summary, we studied multi-skill practice organization in pairs, where partners practiced the same or different skills in alternation (n=24/group for the dyad pairs and n=12/group for Actors and Observers). Mixed physical and observational practice brought about by a partner did not enhance (or impede) learning compared to pure physical practice, nor did matched versus mismatched practice schedules with a partner differentially impact skill acquisition. Given that partners bring more variability into practice than "perfect" demonstrations on an upcoming skill, it might be that the degree of interference was too high to benefit learning in this task. However, we had no evidence of interference in practice for the mismatched group in terms of outcome error (as compared to the matched dyads), nor higher perceptions of interference. Because there was no group that had higher or lower within-person CI, we are not able to say whether this task (i.e., golf putting with 2 skills) would be sensitive to traditional CI effects and as such make comparisons between within- and between-person CI. This will be important for future studies as we evaluate these dyad practice conditions. Considering the inter-trial breaks commonly incorporated into real-world training for a variety of reasons, it will be important to continue such inquiries into how to organize paired multi-skill practice to optimize learning. However, on the basis of these data, we have no evidence that partner-interleaved practice serves to bring between-person CI into practice, or if it does, such observation-induced interference is not sufficient to enhance learning in such semi-blocked practice schedules as adopted here.

Footnotes

1. The Task Evaluation Questionnaire and additional variations of the Intrinsic Motivation Inventory can be accessed for scholarly use by registering with the Self-Determination Theory Organization through the link provided in the reference list (Intrinsic Motivation Inventory, n.d.).
2. As Partner 2s always completed the golf experience questionnaire before completing the pretest, it is possible that this opportunity to reflect upon their previous golf-related experiences could have affected their perceptions of self-efficacy, and as a potential corollary effect, their performance and learning. However, given the lack of partner-related effects throughout the experiment, this was unlikely the case in the current study.

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Table 1: *Procedure details*

	Day 1		Day 2 (~24h later)			Day 3 (~1 week later)	
	Pretest	Acquisition	Retention	Retention	Acquisition	Retention	Retention
Alone or Paired	Alone	Paired	Alone	Alone	Paired	Alone	Alone
Feedback	X	✓	X	✓	✓	X	✓
# Trials/Putter	3	18	6	6	18	6	6
# Trials/Partner	6	36	12	12	36	12	12

Note. During the paired acquisition sessions, partners alternated turns on consecutive trials using the same putter (matched group) or different putters (mismatched group), or the Actor physically practiced while their Observer partner watched (control group).

Table 2: *Mean ratings (and SDs) for the Task Evaluation Questionnaire subscales*

	Matched		Mismatched		Actors
Subscale	P1s	P2s	P1s	P2s	
Interest/enjoyment	5.2 (1.5)	4.8 (1.1)	4.3 (1.4)	4.6 (1.1)	4.7 (1.3)
Competence	3.9 (1.1)	3.3 (1.2)	3.7 (1.1)	3.8 (1.0)	3.7 (1.0)
Pressure/tension	2.1 (0.9)	2.5 (1.0)	2.3 (1.3)	2.8 (0.8)	2.1 (1.1)

Note. Scales ranged from 1–7.

Table 3: *Mean ratings (and SDs) for the paired practice experience questionnaire*

	Matched		Mismatched	
Item	P1s	P2s	P1s	P2s
1. Watching my partner helped my own performance/learning	3.9 (1.5)	4.7 (1.2)	3.6 (1.8)	3.8 (1.5)
2. Watching my partner interfered with my own performance/learning	1.8 (1.0)	2.8 (1.7)	2.9 (1.8)	3.1 (1.7)
3. I wanted to be more accurate than my partner	5.0 (1.3)	5.2 (1.4)	5.0 (1.7)	4.8 (1.5)

Note. Scales ranged from 1–7.

Figure captions

Figure 1. A sample of the dyad practice schedules (36 trials out of a total of 72 trials per acquisition session on each day), where partners alternated turns physically practicing and observing one another perform the same skill (matched group) or different skills (mismatched group) on consecutive trials. Partner 1s always initiated the practice block (6 trials each/block) as shown in bold. Partner 2s always went second and had exactly the same order of putting conditions irrespective of group.

Figure 2. A) Radial error (and *SE* bars) and B) Bivariate variable error (and *SE* bars) in testing, as a function of matched versus mismatched alternating dyad practice. FB = feedback.

Figure 3. A) Radial error (and *SE* bars) and B) Bivariate variable error (and *SE* bars) in testing for the Actors and Partner 2s (P2s), as a function of pure physical practice (Actors) or matched versus mismatched alternating practice. FB = feedback.

Figure 4. A) Radial error (and *SE* bars) and B) Bivariate variable error (and *SE* bars) in testing, as a function of physical practice (Actors) versus observational practice (Observers). FB = feedback.

Figure 5. A) Radial error (and *SE* bars) and B) Bivariate variable error (and *SE* bars) in acquisition, as a function of matched versus mismatched alternating dyad practice. All blocks consisted of 6 trials/putter.

Figure 6. Mean percentage of acquisition trials where matched and mismatched alternating dyads demonstrated corrective behaviour in response to y-dimension error in the preceding performed (self-referenced) or observed (partner-referenced) trial.

Figure 7. Schematic of potential corrective behaviours to errors in the y-dimension (i.e., overshooting or undershooting the target), along with percentage of acquisition trials on

which the matched and mismatched dyad groups showed each type of scenario (collapsed across groups and days). Examples pertain to the outcome of ‘trial 2’ (“T2”) with reference to the P2’s own previous trial (black “T1”) or their partner’s previous trial (grey “T1”). Outcomes could reflect A) compensation with respect to both their own and their partner’s previous error, B) no compensatory behaviour, C) compensation with respect to only their own previous error, or D) compensation with respect to only their partner’s previous error.

Figure 1

	Acquisition Trials																	
	1	2	**3**	4	**5**	6	**7**	8	**9**	10	**11**	12	**13**	14	**15**	16	**17**	18
Matched	**S**	S	**S**	S	**S**	S	**S**	S	**S**	S	**S**	S	**M**	M	**M**	M	**M**	M
Mismatched	**M**	S	**M**	S	**M**	S	**M**	S	**M**	S	**M**	S	**S**	M	**S**	M	**S**	M

	19	20	**21**	22	**23**	24	**25**	26	**27**	28	**29**	30	**31**	32	**33**	34	**35**	36
Matched	**M**	M	**M**	M	**M**	M	**S**	S	**S**	S	**S**	S	**S**	S	**S**	S	**S**	S
Mismatched	**S**	M	**S**	M	**S**	M	**M**	S	**M**	S	**M**	S	**M**	S	**M**	S	**M**	S

Note. A full acquisition session consisted of 72 trials (36 physical practice trials/partner). Odd number trials were completed by Partner 1 and even number trials were completed by Partner 2. All participants completed six trials in a row using the same putter before switching to the other putter (starting putter was counterbalanced). S = standard putter. M = miniature putter.

Figure 2

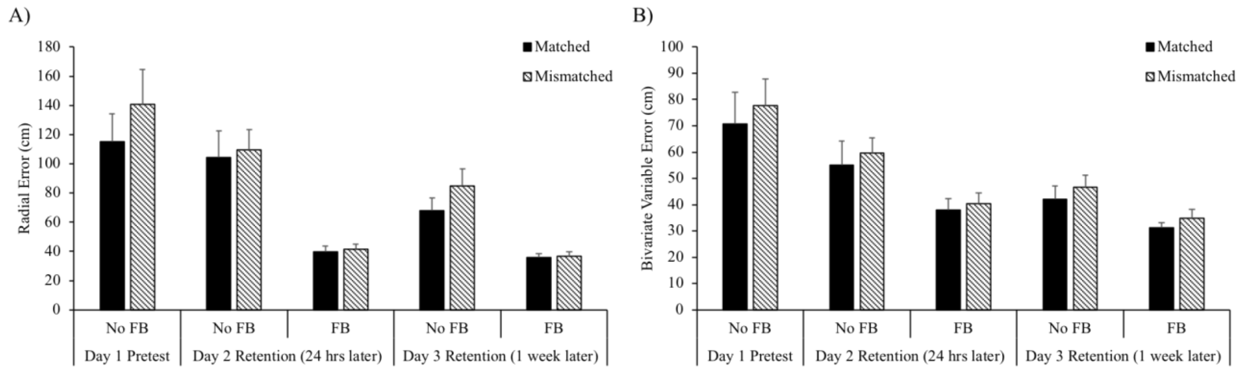
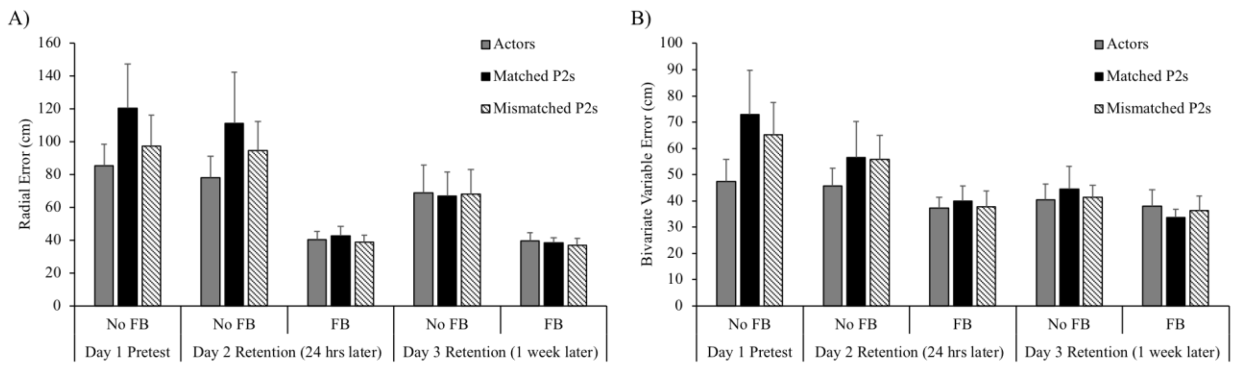

A)
Radial Error (cm)
0
20
40
60
80
100
120
140
160
180
Matched
Mismatched
No FB
No FB
FB
No FB
FB
Day 1 Pretest
Day 2 Retention (24 hrs later)
Day 3 Retention (1 week later)
B)
Bivariate Variable Error (cm)
0
10
20
30
40
50
60
70
80
90
100
Matched
Mismatched
No FB
No FB
FB
No FB
FB
Day 1 Pretest
Day 2 Retention (24 hrs later)
Day 3 Retention (1 week later)

Figure 3



A)
Radial Error (cm)
160
140
120
100
80
60
40
20
0
Actors
Matched P2s
Mismatched P2s
No FB
No FB
FB
No FB
FB
Day 1 Pretest
Day 2 Retention (24 hrs later)
Day 3 Retention (1 week later)
B)
Bivariate Variable Error (cm)
100
90
80
70
60
50
40
30
20
10
0
Actors
Matched P2s
Mismatched P2s
No FB
No FB
FB
No FB
FB
Day 1 Pretest
Day 2 Retention (24 hrs later)
Day 3 Retention (1 week later)

Figure 4

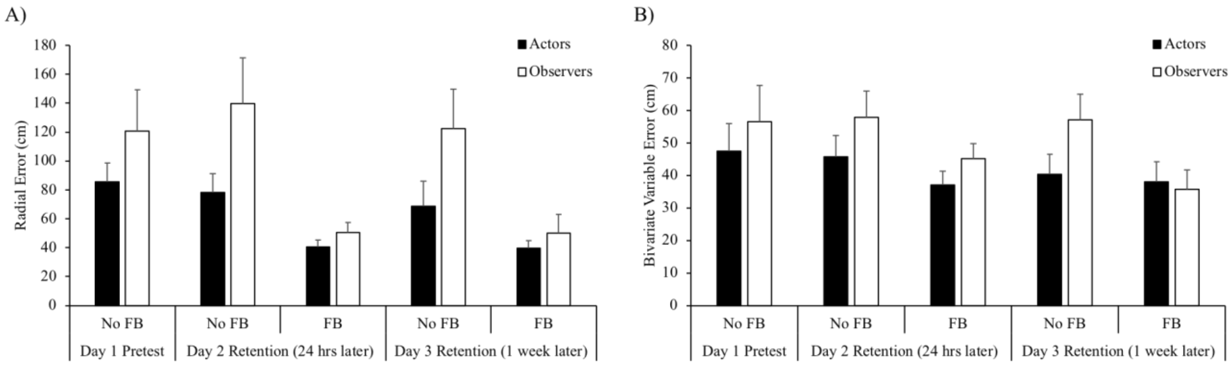

A)
Radial Error (cm)
180
160
140
120
100
80
60
40
20
0
Actors
Observers
No FB
No FB
FB
No FB
FB
Day 1 Pretest
Day 2 Retention (24 hrs later)
Day 3 Retention (1 week later)
B)
Bivariate Variable Error (cm)
80
70
60
50
40
30
20
10
0
Actors
Observers
No FB
No FB
FB
No FB
FB
Day 1 Pretest
Day 2 Retention (24 hrs later)
Day 3 Retention (1 week later)

Figure 5

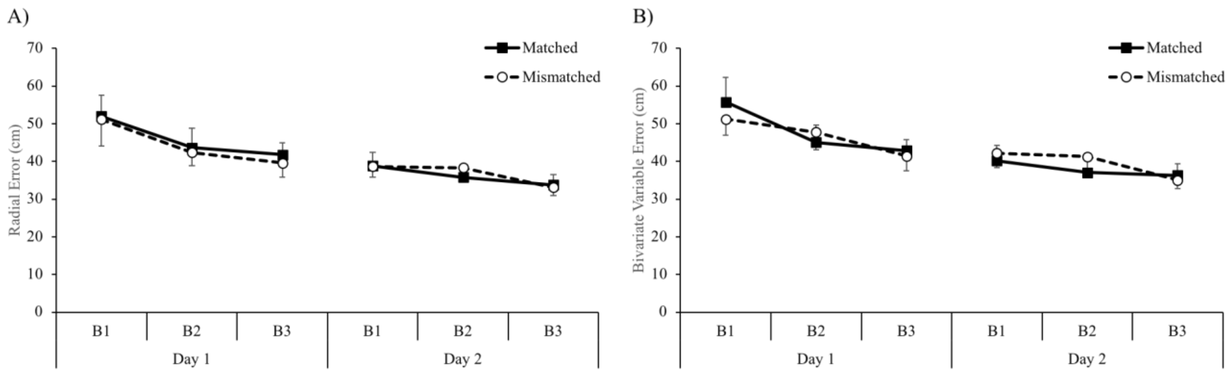

A)
Radial Error (cm)
Matched
Mismatched
Day 1
Day 2
B1
B2
B3
B)
Bivariate Variable Error (cm)
Matched
Mismatched
Day 1
Day 2
B1
B2
B3

Figure 6

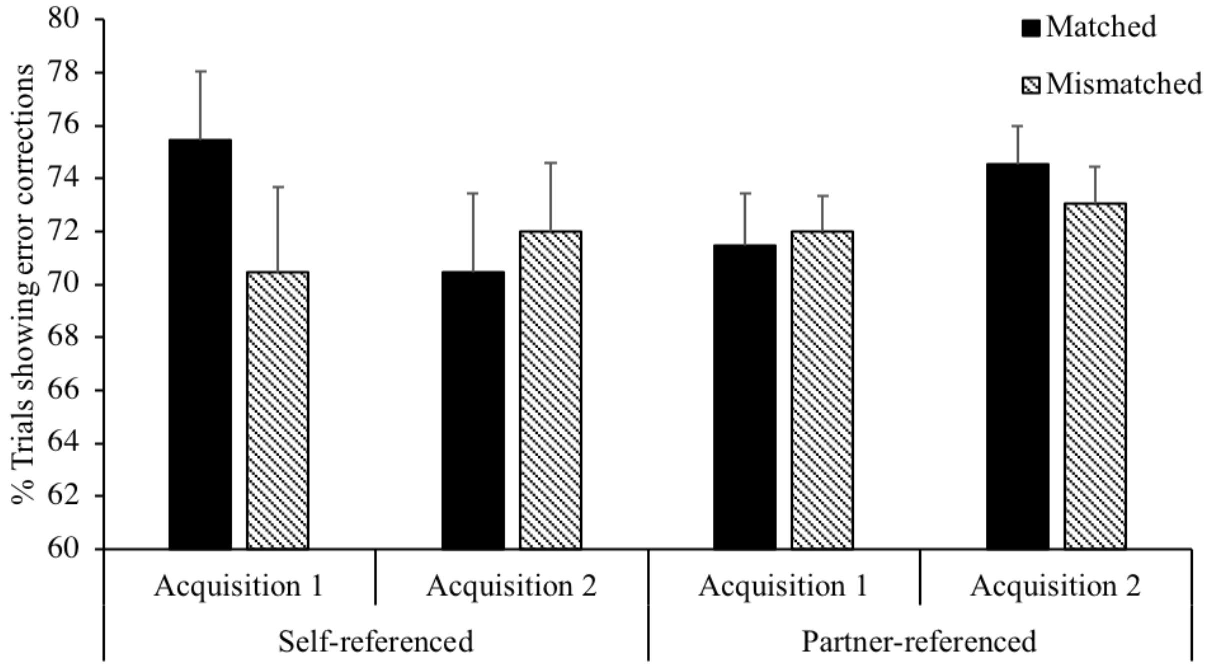
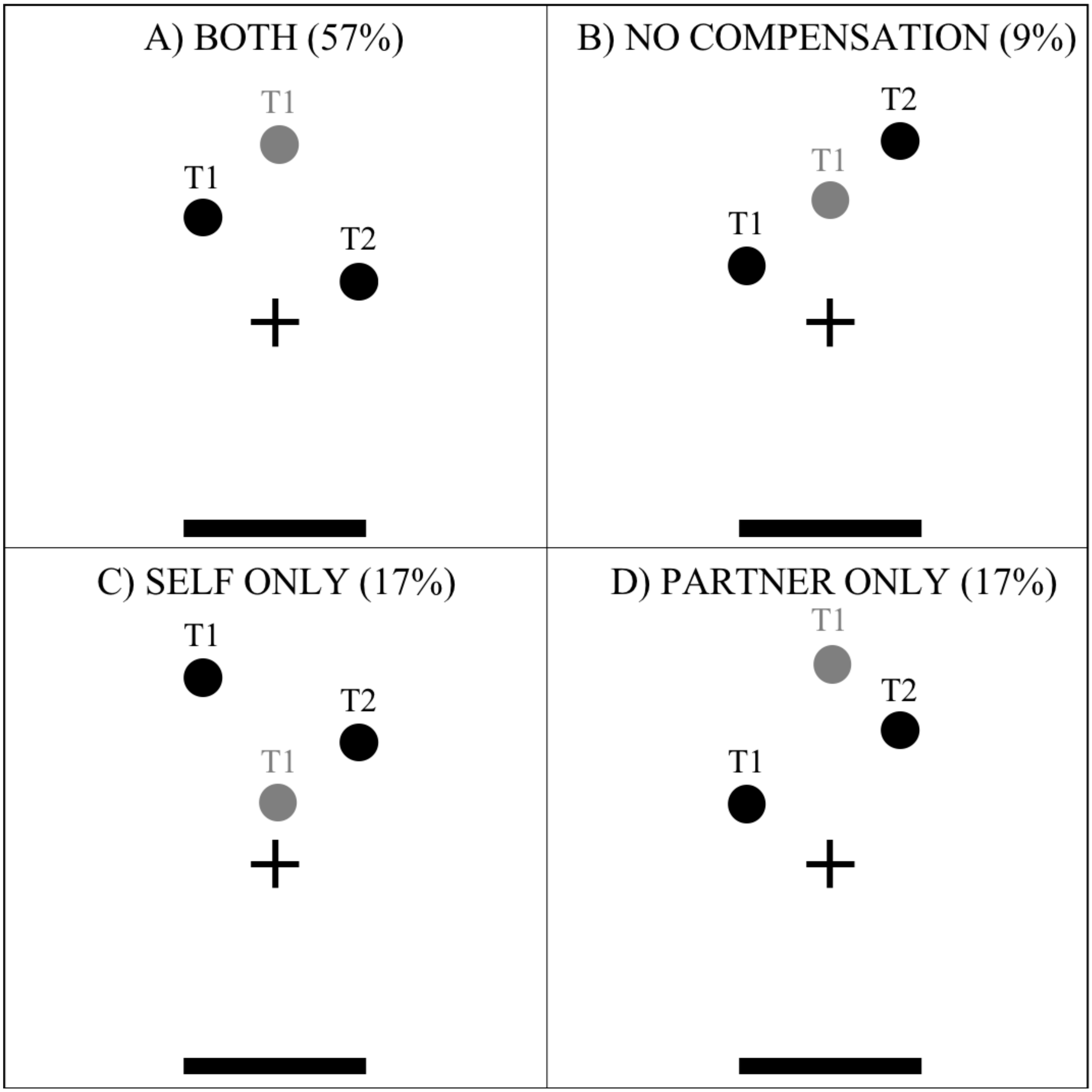

% Trials showing error corrections
80
78
76
74
72
70
68
66
64
62
60
Matched
Mismatched
Acquisition 1
Acquisition 2
Acquisition 1
Acquisition 2
Self-referenced
Partner-referenced

Figure 7


A) BOTH (57%)
T1
T1
T2
B) NO COMPENSATION (9%)
T2
T1
T1
C) SELF ONLY (17%)
T1
T2
T1
D) PARTNER ONLY (17%)
T1
T2
T1