MOTOR LEARNING THROUGH A SOCIAL LENS

by

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Abstract

The goal of this dissertation was to study the impact of a co-learner on individuals' motor performance, learning, and perceptions of the practice experience. In Experiment 1, we introduced "concurrent" practice, where partners practiced and observed one another simultaneously. Concurrent practice promoted movement coupling within pairs and was perceived as more interfering than individual and turn-taking practice of a balance-related task. However, these differences did not impact error during practice or testing. In Experiment 2, we studied whether matching or mismatching a partner is better for multi-skill learning. Partners practiced the same or different golf-putting skills in alternation. Although contextual interference would be higher for the mismatched group, mismatching did not modulate performance in practice or retention compared to matching or pure physical practice. In Experiments 3–5, we studied multi-skill learning when learners have control over how to practice. Experiment 3 tested self- versus peer-directed practice, when one partner practiced and the other passively observed or made task-switching decisions for the performer. Both self- and peer-schedulers made performance-dependent decisions, choosing to switch tasks based on timing error. Although peer-schedulers chose to switch more frequently, the groups did not differ in retention. In Experiments 4 and 5, we assessed the impact of a partner on self-directed practice choices, when partners switched turns after 9-trial blocks or after every trial. Self-directed learners showed partner-dependent practice, with the partner's practice impacting sequence selection and switching frequency. Importantly, self-directed learners did not sacrifice the performance-dependent nature of their task-switching, suggesting that some practice behaviours are resistant to a partner's practice while others are susceptible to modulation. Overall, this dissertation provides evidence that practice in pairs influences the practice decisions/behaviours of learners

and provides efficiency benefits, as two people can be trained in the time otherwise devoted to one learner. However, practice in pairs did not improve learning compared to practice alone. Merely practicing with a partner, even if they exert some influence on decisions, was not enough to yield motor learning benefits for the individual. There is a need for well-powered studies to explore the conditions which might promote such benefits.

Lay Summary

I investigated how practice with a partner impacts motor performance, learning, and perceptions of the practice experience. I studied different forms of paired practice, including concurrent practice (partners practice and watch one another at the same time), alternating practice (partners take turns practicing and watching one another), and peer-directed practice (one partner makes practice decisions for the learner). These paradigms allowed me to address how a partner influenced the learning of one versus multiple skills within a practice session, as well as the learning of relatively simple versus more complex skills. Overall, a partner impacted performance and choices in practice, but had limited effects on longer-term learning. Contrary to recent suggestions, paired practice was not more motivating/enjoyable or effective compared to practice alone. Paired practice did, however, provide efficiency benefits because two people could be trained in the same amount of time that it takes to train a single person.

Preface

This dissertation is a result of my supervisor, Dr. Nicola Hodges, providing invaluable guidance and support throughout all phases of the research. Dr. Hodges and I were responsible for the conception and design of the experiments. Ruslan Amarasinghe provided technical support throughout this dissertation and conducted the programming of Experiment 1 (presented in Chapter 2), based on original programming by Graeme Kirkpatrick. I conducted the programming of Experiments 3–5 (presented in Chapters 4–6), based on original programming by Dr. Desmond Mulligan. I was involved with all data collection, along with the following undergraduate research assistants: Brynn Alexander (Chapters 2 and 6), Jaspreet Dhillon (Chapter 3), Amanda de Faye (Chapter 4), and Teresa Chang (Chapter 5). I was responsible for the data analysis and manuscript preparation (including tables and figures), and made modifications based on feedback from Dr. Hodges, my committee, and anonymous journal reviewers.

All experiments in this dissertation were conducted on human participants and granted ethical approval by the University of British Columbia's Behavioural Research Ethics Board. Chapter 2 was covered under ethics certificate number H14-01796 (Balance-platform pairs study). Chapter 3 was covered under ethics certificate number H13-02459 (Practice scheduling and dyad learning in golf putting). Chapters 4–6 and Appendix B were covered under ethics certificate number H09-01122 (Practice variability in motor skill learning: A comparison of self and teacher-directed practice methods).

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A version of Chapter 2 has been published. Karlinsky, A., & Hodges, N. J. (2018b). Turn-taking and concurrent dyad practice aid efficiency but not effectiveness of motor learning in a balance-related task. *Journal of Motor Learning and Development, 6*(1), 35–52.

A version of Chapter 3 has been accepted for publication. Karlinsky, A., & Hodges, N. J. (accepted). Manipulations to practice organization of golf putting skills through interleaved matched or mismatched practice with a partner. *Human Movement Science.*

A version of Chapter 4 has been published. Karlinsky, A., & Hodges, N. J. (2014). Evaluating the effectiveness of peer-scheduled practice on motor learning. *Journal of Motor Learning and Development, 2*(4)*,* 63–68.

A version of Chapter 5 has been published. Karlinsky, A., & Hodges, N. J. (2018a). Dyad practice impacts self-directed practice behaviours and motor learning outcomes in a contextual interference paradigm. *Journal of Motor Behavior, 50*(5), 579–589.

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List of Symbols

α	alpha
ß	beta
d	Cohen's d
f	Cohen's f
n	number of participants
p	p-value
η_p^2	partial eta squared
r	Pearson's correlation coefficient

List of Abbreviations

AE	absolute error
AIM	active interlocked modeling
ANOVA	analysis of variance
B	blocked
BVE	bivariate variable error
CE	constant error
CI	contextual interference
cm	centimetre
EEG	electroencephalogram
F	F ratio
FB	feedback
hr	hour
HSD	honestly significant difference
Hz	hertz
IMI	Intrinsic Motivation Inventory
in	inch
KR	knowledge of results
M	mean
m	metre
MANOVA	multiple analysis of variance
ms	millisecond

MT	movement time
MTE	movement time error
P1	Partner 1
P2	Partner 2
R	random
RE	radial error
RM	repeated measures
RMSE	root-mean-square error
s	second
SD	standard deviation
SDM	self-determined motivation
SDT	self-determination theory
SE	standard error
t	trial
TMS	transcranial magnetic stimulation
TOT	time-on-target
VE	variable error
yr	year

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For my parents.

Chapter 1: General Introduction

Motor learning is a ubiquitous aspect of everyday human life, and the desire to better understand how motor skills are acquired motivates researchers and practitioners alike. The overarching goal of my dissertation was to contribute to the motor learning discourse with respect to the acquisition of motor skills within paired practice contexts (wherein two people are practicing) and ultimately to the design of efficient and effective practice conditions. Motor skills can be operationally defined as movements that require practice to learn and perform (as opposed to being genetically defined) and that are voluntarily produced (Schmidt & Lee, 2011). Motor learning has been described as a set of internal processes that yield a relatively permanent gain in the capability for skilled performance. These processes are not directly observable and must be inferred from a persistent improvement in performance as a result of practice or experience (Schmidt & Lee, 2011). In this dissertation, I will present five studies where I have manipulated practice conditions with respect to the presence and activities of a co-learner. These studies provide insight into the mechanisms subserving motor learning in social (i.e., multi-person) contexts and the potential benefits (or costs) such practice conditions confer.

Individual training sessions have typically been considered the best way to train people. For researchers, individual training sessions allow control over the learner's practice experience and the experimental variables of interest, while for practitioners, these sessions allow undivided attention towards the individual client, who in turn is free from peer-related distractions (Shea, Wulf, & Whitacre, 1999). Indeed, private lessons in a variety of sports remain highly lucrative industries (e.g., tennis, golf, skiing; Shea et al., 1999). Within the last few decades, however, the potential benefits of social practice, particularly practice in pairs (also known as dyads), has begun to receive more empirical attention. This attention has been spurred by an interest in how

individuals influence each other in performance conditions (so termed joint action; e.g., Sebanz, Bekkering, & Knoblich, 2006), as well as by research in motor learning based on motivational aspects associated with social-relatedness (e.g., Lewthwaite & Wulf, 2012). Overall, this dissertation provides evidence that social learning conditions do not necessarily benefit motivation or training effectiveness compared to practice alone, but that practice in pairs can influence learners' practice decisions/behaviours and provide efficiency benefits, as two people could be trained in the time otherwise devoted to one learner. Mechanisms which have been proposed to impact dyad practice are detailed in the next section, followed by an overview of the dyad learning literature to-date.

1.1 Variables impacting dyad practice

1.1.1 Social facilitation, audience effects, and co-action effects

Evidence that social performance contexts can affect behaviour has been around for over 100 years (e.g., Triplett, 1898). Such effects have been captured in terms of *social facilitation, audience effects,* and *co-action effects*, all of which can factor into dyad practice contexts, depending on how the practice conditions are organized. Social facilitation can be caused simply by the presence of a conspecific (regardless of whether they are watching or interacting with the actor; for reviews, see Bond & Titus, 1983; Strauss, 2002). Social facilitation effects have been documented in a range of species and are thought to be driven by arousal or "drive" (Zajonc, 1965). In humans, social facilitation effects are typically associated with increases in the performance of simple/well-learned tasks and decreases in the performance of more complex tasks (Strauss, 2002).

Audience effects refer to changes in behaviour that arise as a function of being observed – or by believing oneself to be observed – by another person (for a review, see Hamilton & Lind,

2016). These effects require individuals to detect the eyes of the observer(s) and to understand that they are being watched, and yield larger effects than the mere presence of others (e.g., Cottrell, Sekerak, Wack, & Rittle, 1968). Various theories have been posited to underpin audience effects, from drive theory mentioned above (Zajonc, 1965) to self-awareness (e.g., Silvia & Duval, 2001) and self-presentation theories (e.g., Baumeister & Hutton, 1987; Bond, 1982). These latter two theories are broadly related to the desire of individuals to demonstrate competence and maintain a good self-image. More complex still are co-action effects, which refer to changes in behaviour when individuals work on the same task (e.g., in joint action contexts; Sebanz et al., 2006). Typically, any positive outcomes associated with practicing with a partner have been shown to be more than the product of any of the above-mentioned effects, as studies have controlled for such contributions by comparing multiple forms of dyad practice (e.g., Day, Paulus, Arthur, & Fein, 2003; Granados & Wulf, 2007; Shea et al., 1999; Shea, Wright, Wulf, & Whitacre, 2000). For example, alternating dyad practice with a partner on every trial on a balance-task was more beneficial for learning than performing all physical practice followed by a block of just watching the partner (or vice versa). These differences were noted despite both dyad practice protocols providing the same opportunities for social facilitation, audience effects, and co-action effects (Shea et al., 1999).

1.1.2 Observational learning

The advantages of paired practice are most often attributed to the opportunity for observational learning (e.g., Granados & Wulf, 2007; McNevin, Wulf, & Carlson, 2000; Shebilske, Regian, Arthur, & Jordan, 1992; for a review of observational learning, see Hodges & Ste-Marie, 2013). Specifically, in paired practice, each co-learner typically serves as a learning model for the other. A learning model is one who is observed engaging in physical practice,

including committing errors, receiving feedback, and improving over time (Adams, 1986). It is thought that watching this skill acquisition process allows the observer to engage in cognitive activities akin to the performing learner, including performance evaluation, error detection, and consideration of potential corrective responses (e.g., Adams, 1986; Black & Wright, 2000; Lee & White, 1990; for a review, see 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). Peer observation can also help to promote individuals' 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).

There is also a neurophysiological mechanism that helps to explain the process of observational learning. The observation of a motor skill is thought to activate the action representations that are also activated during the physical execution of the skill (for a review, see Rizzolatti & Craighero, 2004). Hence, the observation of the action during practice can generate similar neural activations and, under certain conditions, lead to similar adaptations in the learner as physical practice (see Ray, Dewey, Kooistra, & Welsh, 2013; Stefan et al., 2008). The benefits of these observation-related processes are augmented in dyad practice by the opportunity to use the information gained in one's own physical practice (e.g., Hebert, 2018).

1.1.3 Motivation-related factors

Some researchers have suggested that any benefits associated with practice in pairs might be due in part to motivational benefits of practicing in a social setting with a peer (e.g., Granados & Wulf, 2007; Lewthwaite & Wulf, 2012; Shea et al., 1999). Efforts to measure motivation-

related indices in the context of dyad practice have been relatively limited, but to our knowledge, there is currently no evidence that dyad practice conditions are more motivating than individual conditions. Nevertheless, shared practice contexts inherently provide opportunities for social comparisons. Researchers have demonstrated that relative (comparative) feedback (either about a peer or average performance on a given task) can influence competency beliefs and motor skill learning (e.g., Wulf, Chiviacowsky, & Lewthwaite, 2010; yet see Ong & Hodges, 2018). Such comparisons are typically designed to make learners perceive themselves as "better" or "worse" than others, serving to motivate and enhance learning or to demotivate and impede learning, respectively. The significant role perceptions of competence can play towards motor learning was highlighted in the recently proposed OPTIMAL theory of motor learning (Wulf & Lewthwaite, 2016). This theory incorporates aspects of Deci and Ryan's self-determination theory (e.g., Deci & Ryan, 2008; Ryan & Deci, 2000), including the importance of satisfying the psychological needs of competence, autonomy, and relatedness, as a means of bringing a more cognitive-affective perspective to our understanding of the mechanisms of motor learning. Of note, social comparison and competency-related effects are usually based on virtual or experimenter-provided information (e.g., Wulf et al., 2010; Wulf, Lewthwaite, & Hooyman, 2013). It may be that a real partner is a more valid source of information for making comparisons, potentially leading to stronger or weaker "comparative" effects. For instance, perceiving oneself to be worse than a partner may actually be motivating in a real social context, triggering individuals' desire to improve to keep up with peers and/or avoid embarrassment (e.g., Rhea, Landers, Alvar, & Arent, 2003). Further research is needed to better understand the impacts of genuine (peer-related) comparative feedback both on behavioural outcomes and motivation to practice/learn.

Interestingly, social comparisons have been shown to be more important for the performance of group members with low to average levels of motivation, compared to their more highly motivated counterparts (Prislin, Jordan, Worchel, Semmer, & Shebilske, 1996). It is likely that more motivated learners are less in need of social comparisons to encourage effort and augment performance (or might be less affected by negative social comparisons). For less motivated individuals, perceiving oneself to be worse than peers may actually be motivating, triggering individuals' desire to keep up with peers, in line with the self-awareness and/or the self-presentational theory of social facilitation (Rhea et al., 2003). Relatedly, practicing with a peer might also impact learners' motivation by adding a sense of competition to the practice context (McNevin et al., 2000). This sense of competition could prompt learners to set higher goals, in order to perform closer to (or above) their peers (McNevin et al., 2000; Rhea et al., 2003).

1.1.4 Dialogue

In some cases, the opportunity to engage in discussion with co-learners has been shown to enhance motor learning. These opportunities for discussion seem to be particularly important for sharing knowledge/strategies that might not be easily seen. For example, during training of a complex visuo-motor task (the "Space Fortress" video game), unguided group discussion was found to yield three general discussion orientations: advice, social comparison, and motivation, and resulted in enhanced learning compared to a no-discussion control group (Prislin et al., 1996). Discussion of strategy in particular predicted final individual motor performance (Prislin et al., 1996). In comparison, dialogue did not facilitate learning above and beyond the opportunity to observe a peer, nor indeed above practice alone, when learning a task for which the strategy was provided (Granados & Wulf, 2007). For challenging tasks, sharing strategies

might increase group members' sense of responsibility for and their involvement in the training process, encouraging them to engage in more information processing activities and invest more cognitive effort than elicited by individual conditions (McNevin et al., 2000).

1.1.5 Distributed practice

Another mechanism that potentially underpins dyad practice benefits relates to the opportunity for rest in between trials. Resting between physical practice trials – that is, receiving "distributed" or "spaced" practice – has been shown to confer learning benefits for individuals, compared to more "massed" practice conditions (for reviews, see Donovan & Radosevich, 1999; Lee & Genovese, 1988). These distributed practice effects have been related to memory consolidation processes (through which motor skills are encoded and refined), as well as cognitive effort or fatigue, in that the quality of practice is better when individuals are not tired (Lee & Genovese, 1988). Yet, despite rest intervals being a key component of most dyad practice protocols to-date (during which time a co-learner can be observed), the contribution of rest to paired practice effects may have been understated.

1.2 Methods of dyad practice

A summary diagram illustrating the various forms of dyad practice which have been studied (including novel variations introduced in this dissertation), along with a brief description of each, is presented in Figure 1. This diagram guides how this section is structured and provides a reference for terms. I have also included a table in which I specify where each dissertation study fits in relation to each paired practice method discussed, as well as the methods' general success with respect to long-term learning (see Table 1). I first review dyad practice methods where learners physically practice and observe one another at the same time. Such "concurrent" practice scenarios were historically studied under "part-task" practice conditions, where partners

performed complementary tasks. In Experiment 1 of my thesis (Chapter 2), I report the first test of a concurrent “whole-task” dyad practice protocol, where partners simultaneously performed the entire task independently. I then review research involving “non-concurrent” paired practice, where only one person physically practices at a time. This often occurs in the form of alternating (turn-taking) practice of a single or multiple skills but can also arise when only one co-learner is assigned to physically practice (the “actor”) while the other observes. These latter cases are discussed in the context of peer-directed practice.

Figure 1. Summary diagram illustrating the various forms of paired practice which have been studied along with a brief description of each.

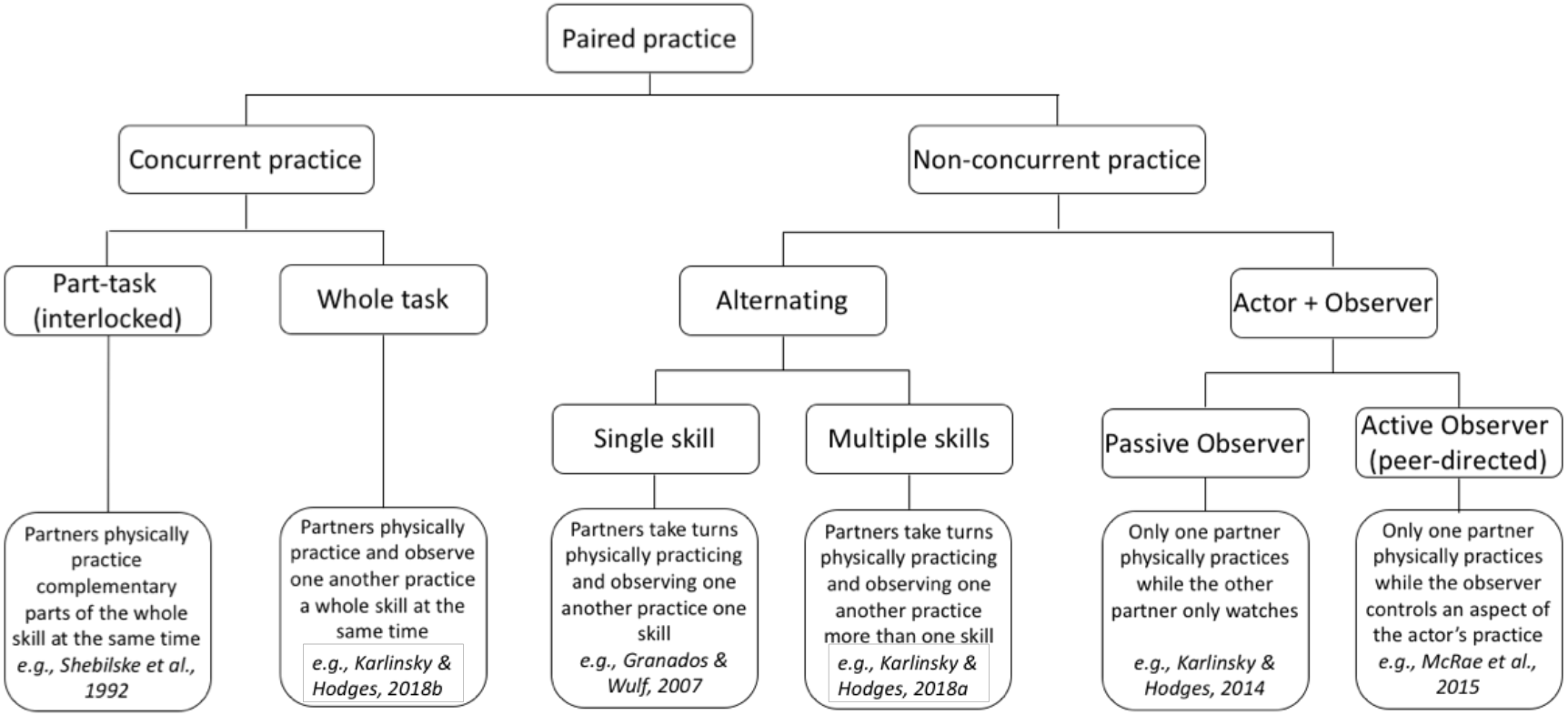

Table 1. Summary of paired motor learning studies.

Paradigm + Description	Sample study	Skill(s) practiced	Experimental groups	Summary of retention results
CONCURRENT PRACTICE AND OBSERVATION				
Part-task practice Partners practice complementary parts of the whole skill at the same time	Shebilske et al. (1992)	Space Fortress computer game	-Individual (bimanual practice) -Dyad-part (unimanual practice)	No differences between individuals and pairs, despite pairs receiving half the physical practice trials and never practicing the task components together.
Whole-task practice Partners practice a whole skill at the same time	Study 1	Balance platform	-Individual -Concurrent pairs -Turn-taking pairs	No differences between individuals and pairs or between pair groups.
NON-CONCURRENT PRACTICE AND OBSERVATION				
Alternating practice of a single skill Partners take turns physically practicing and observing one another practice a single skill	Study 1	Balance platform	-Individual -Concurrent pairs -Turn-taking pairs	No differences between individuals and pairs or between pair groups.
Alternating practice of multiple skills Partners take turns physically practicing and observing one another practice more than one skill	Study 2	Golf putting	-Actor + Observer (passive) -Matched-skill turn-taking pairs -Mismatched-skill turn-taking pairs	Actors were more accurate than Observers. No differences between Actors and pairs or between pair groups.
	Study 4	Keystroke sequences	-Blocked + Self-directed partner -Random + Self-directed partner -Self-directed + Self-directed partner (only one partner could observe)	Random pairs had lower timing error than blocked pairs on the post-test and on 1/4 retention tests. No differences between self-controlled partners who observed vs. did not observe the partner's practice.
	Study 5	Keystroke sequences	-Blocked + Self-directed partner -Random + Self-directed partner -Self-directed (alone)	Random pairs had lower timing error than blocked pairs on 1/4 retention tests. Imposed-schedule partners had lower timing error than self-directed partners on 1/4 retention tests. No differences between self-directed learners who practiced with a partner vs. alone.
Actor + Observer The "actor" physically practices while an "observer" watches (passively or with some control over the actor's practice)	Study 3	Keystroke sequences	-Actor (self-directed) + Observer (passive) -Actor (peer-directed) + Observer (peer-scheduler)	Actors had lower timing error than Observers. No differences between self- vs. peer-directed Actors or between passive vs. peer-scheduler Observers.

1.2.1 Concurrent practice and observation

1.2.1.1 Concurrent part-task practice

It is common for the practice of skills with complex components to break them down into smaller, simpler, easier to achieve units. This method has been known as part-task practice, which is distinguished from whole-task practice (e.g., Hansen, Tremblay, & Elliott, 2005; for a review, see Fontana, Furtado, Mazzardo, & Gallagher, 2009). The earliest research on paired practice was related to this idea of part practice and has stemmed in particular from a seminal study conducted by Shebilske and colleagues (Shebilske et al., 1992). Using the militaristic computer game "Space Fortress", individuals alternated turns practicing only part of the task, such as performing the 'pilot' (joystick) tasks or the 'copilot' (mouse) tasks (which involved different hands; Shebilske et al., 1992). This type of divided, part-task practice was referred to as *active interlocked modeling* (AIM), due to the fact that partners practiced together (interlocked) and could learn from their partner before switching roles. When partners engaged in this type of practice, alternating turns at each role, they learned to perform the entire game individually, performing as well as participants who had trained bimanually on the whole task. This shared efficiency in practice was achieved despite the fact that these pairs completed only half the physical practice on each task component compared to individual learners and never practiced the components at the same time. Thus, this part-practice, interlocked protocol afforded a 100% increase in training efficiency, as two people could be trained in the time it took to train one person.

Extensive research has since been conducted using the Space Fortress task and the part-task, interlocked practice protocol, providing insight into factors that influence this distinct form of shared learning (for a review and meta-analysis of the AIM literature, see Jordan, 1997).

However, it has been argued that this form of part-task practice is not realistic for the training of many motor skills (Shea et al., 1999). In sports, for example, it is typical to practice an entire skill independently, even when practice is undertaken with peers (e.g., physical education classes, team sports). Thus, in more recent years, the dyad learning research has broadened to consider the mechanisms and effects of paired practice when each co-learner physically performs and observes the whole motor skill.

1.2.1.2 Concurrent whole-task practice

There are many physical activity contexts where individuals do not typically share task demands, but rather practice whole skills simultaneously with co-learners. Consider for example, yoga, dance, and martial arts or team-based sports where drills are performed at the same time. In such scenarios, how does what learners see influence how they perform and learn? At a neurophysiological level, evidence has been provided showing how concurrent observation when physically performing can both enhance or interfere with how actions are later retained. Experiments were performed using a technique called transcranial magnetic stimulation (TMS), to probe cortical (re)organization following physical or observational practice of simple thumb movements (e.g., Celnik et al., 2006; Classen, Liepert, Wise, Hallett, & Cohen, 1998; Ray et al., 2013). Observing congruent actions during practice enhanced motor memory formation relative to physical or observational practice alone. Observing incongruent actions (i.e., thumb flexion when extension was required) interfered with memory formation compared to just physical practice (Stefan, Classen, Celnik, & Cohen, 2008).

There is a growing body of research showing that action observation can disrupt the observer's motor control, referred to as "motor contagion" (for reviews, see Blakemore & Frith, 2005; Schütz-Bosbach & Prinz, 2007). Even when simply asked to stand still, watching another

person's movements can elicit unintentional behavioural responses (Sebanz & Shiffrar, 2007; Tia, Paizis, Mourey, & Pozzo, 2012; Tia et al., 2011). These observation-induced movements have been referred to as "ideomotor movements", reflecting the link between the idea (the representation of the perceptual consequences of the action) and the action. This terminology is derived from the ideomotor theory (James, 1890) and its later extensions (including the theory of event coding), which have provided a framework for conceptualizing links between the imagination, perception, and production of action (e.g., Hommel, Müssler, Aschersleben, & Prinz, 2001; Prinz, 1997).

There is also compelling evidence that watching movements that differ to one's own actions interferes with motor performance. As an illustrative example, participants' movement variance was increased (indexing interference in motor control) when observing a model make vertical arm movements during their own horizontal arm movements (or vice versa; e.g., Bouquet, Gaurier, Shipley, Toussaint, & Blandin, 2007; Kilner, Hamilton, & Blakemore, 2007; Kilner, Paulignan, & Blakemore, 2003). While it is clear that concurrent action observation can affect motor behaviour, this line of research has been largely limited to the immediate effects on action production, rather than on long-term retention. Although there is some evidence that motor contagion can be inhibited following motor training (Roberts et al., 2016), this research was limited to people practising alone. An interesting question is whether training in the presence of visual interference, such as with a learning partner, might make people less susceptible to motor contagion during later performance contexts (e.g., competition). Moreover, while practice conditions that place higher demands on learners' information processing (e.g., through increased interference) can impair immediate performance, they have been shown to

benefit learning in the long run compared to less challenging conditions (see Guadagnoli & Lee, 2004; Lee et al., 1994; Schmidt & Bjork, 1992).

Insight into the potential pitfalls of concurrent action observation for retention and the importance of cognitive effort during practice was shown in a study where individuals practised the American manual alphabet. Participants either imitated the demonstrated handshapes at the same time they were shown or imitated after watching three different demonstrations (Weeks, Hall, & Anderson, 1996). Concurrent imitation facilitated performance in practice, but impaired learning compared to delayed imitation. The authors suggested that the delayed imitation condition required more cognitive effort to retain and retrieve the modeled information, which promoted retention. However, the sign-language task tested by Weeks and colleagues was about acquisition of the correct hand shapes, a taxing memory task, rather than one which taxed motor performance and quality of execution. For practice of tasks where individuals already know what to do, but just are not very good at doing it, it is possible that gains might be had from physically practising alongside a partner due to the sharing of information about movement quality.

1.2.2 Non-concurrent practice and observation

1.2.2.1 Alternating practice of a single skill

A method of paired practice that is frequently seen in applied settings involves taking turns. In this form of alternating practice, members of a pair take turns physically performing and observing one another practice. Considering that physically and/or cognitively complex skills often require breaks between trials, observing a partner during these rest intervals offers a prime opportunity for learners to remain engaged in practice-relevant processing while still allowing for recovery time (Shea et al., 1999).

Alternating practice was first shown to be more effective than pure physical practice in a study requiring individuals or pairs of individuals to learn to balance on an unstable platform (Shea et al., 1999). In the alternating practice group, partners alternated between physically practicing and observing on each trial. They were also allowed to discuss the task with one another. Alternating practice led to better (lower error) performance than individual practice in a delayed retention test completed alone the following day. This alternating group also outperformed a non-alternating, paired-control group. This group received the same amount of physical practice and observation trials, but in a blocked format (with a discussion period at the end of all practice). Thus, there were benefits of alternating practice which were based on more than simply the opportunity for both physical and observational practice (or discussion). The opportunity to take turns and interact at an appropriate time appears to be important to the success of paired practice. Similar recommendations have been made based on the observational learning literature, where there are benefits associated with such alternating practice methods (for a review, see Ong & Hodges, 2012). However, the research on alternating practice has been constrained to the learning of a single skill (e.g., stabiliometer task, Shea et al., 1999; computer-based task, Shea et al., 2000; cup-stacking, Granados & Wulf, 2007). Because of these single-skill paradigms, it has been difficult to infer under what conditions dyad practice is an effective and/or efficient method of practice.

1.2.2.2 Alternating practice of multiple skills

1.2.2.2.1 Experimenter-controlled practice schedules

An important factor to consider with respect to multi-skill motor learning is the organization of practice, that is, the order, or schedule, in which learners practice various motor skills or motor task components. The significant impact practice scheduling can have on learning

has been evidenced in terms of the "contextual interference" (CI) effect, which was first documented in a motor learning context by Shea and Morgan (1979). The CI effect refers to the cognitive interference experienced when practicing multiple skills consecutively. Low CI emerges from practicing one skill repeatedly before switching to a different skill, whereas high CI emerges from practicing multiple skills unsystematically (referred to as blocked and random practice schedules, respectively). The CI effect captures the paradox that although blocked schedules are associated with better performance during practice, random schedules typically result in better skill retention and transfer (for reviews, see Lee, 2012; Wright et al., 2016). While this effect has been widely studied within individual learners, it is possible that CI is not so straightforward in social practice conditions, and the question of how the practice schedule or degree of between-skill "interference" in the practice of an individual influences the performance and learning of other people around them is an interesting and important one. There is the potential of between-person interference in social settings, where the partner's practice impacts on the learner in unintentional ways. Between-person CI has the potential to aid or hinder learning, depending on the degree of interference from the co-learner and the existing CI for the individual.

Although there has been little study of multi-skill learning in pairs, matched or mismatched demonstrations have been interspersed in practice to bring about between-trial interference. Demonstrations showing the next skill to be practiced have been shown to impact both immediate motor performance and longer-term learning. For example, interspersed demonstrations that caused a physical practice schedule of a sequence-timing task to be more blocked/repetitive (i.e., the demonstrated skill was the same as the next skill) facilitated acquisition but impaired retention (Lee, Wishart, Cunningham, & Carnahan, 1997; Simon &

Bjork, 2002). In comparison to this matched schedule, interleaved demonstrations that increased the randomness associated with the order of skills (i.e., the demonstrated skill was different to the next skill) hindered performance in practice but enhanced retention (Simon & Bjork, 2002). Therefore, what happens between trials – in this case watching a matched or mismatched demonstration – impacts learning, presumably through the information processing demands imposed on the learner. These observation-related findings give reason to suspect that a partner's practice would impact the interference experienced by a learner or teammate and ultimately how well they learn.

1.2.2.2.2 Learner-controlled practice schedules

1.2.2.2.2.1 Self-directed practice

The studies described thus far have involved predetermined practice, that is, the learners' practice conditions have been determined by the experimenter in advance of the actual practice session. In the motor learning literature, there is significant interest in the effects and mechanisms of *self-directed* practice and associated benefits (for a review, see Sanli, Patterson, Bray, & Lee, 2013). The commonly proposed mechanisms thought to underpin benefits of self-directed practice relate to what have been termed motivational influences (e.g., Lewthwaite & Wulf, 2012; Wulf & Lewthwaite, 2016) and information-processing differences (e.g., Carter, Carlsen, & Ste-Marie, 2014; Hodges, Lohse, Wilson, Lim, & Mulligan, 2014).

The former motivational explanation builds on arguments that the opportunity to exercise choice is generally autonomy-supportive and intrinsically rewarding, serving to enhance motivation and catalyze learning (for reviews, see Sanli et al., 2013; Wulf & Lewthwaite, 2016). However, such benefits are largely assumed and researchers have been encouraged to attempt to quantify the putative cognitive-affective benefits of such practice contexts (e.g., Carter et al.,

2014; Sanli et al., 2013). The inclusion of the Intrinsic Motivation Inventory (IMI; Deci & Ryan, n.d.) in empirical studies has been deemed particularly relevant towards such endeavours (e.g., Chiviacowsky, Wulf, & Lewthwaite, 2012; Sanli et al., 2013; Ste-Marie, Vertes, Law, & Rymal, 2013). The IMI is designed to assess participants' interest and attitudes toward the experimental task, including their perceptions of interest/enjoyment, competence, choice, effort, and pressure/tension (see Appendix A for further information about this measure).

The information-processing explanation is more closely tied to the processes underpinning learners' decision-making itself, such as performance evaluation and detection and correction of error. There is accumulating evidence that allowing learners to make decisions regarding their practice enhances learning compared to predetermined or yoked practice conditions, wherein the latter, practice is matched to another group's schedule (for a review, see Sanli et al., 2013). Such learning benefits have emerged whether the learners' control was over amount of practice (e.g., Post, Fairbrother, & Barros, 2011) or task difficulty (e.g., Andrieux, Danna, & Thon, 2012), as well as regarding when to receive augmented feedback (e.g., Ali, Fawver, Kim, Fairbrother, & Janelle, 2012), view demonstrations (e.g., Wulf, Raupach, & Pfeiffer, 2005), or use physical assistance devices under both individual (e.g., Hartman, 2007) and paired turn-taking practice conditions (Wulf, Clauss, Shea, & Whitacre, 2001). Of particular relevance to this dissertation, giving learners control over when to switch tasks during motor skill acquisition has been shown to enhance learning compared to experimenter-imposed (including yoked) schedules (e.g., Brydges, Carnahan, Rose, & Dubrowski, 2010; Bund & Wiemeyer, 2004; Hodges, Edwards, Luttin, & Bowcock, 2011; Jowett, LeBlanc, Xeroulis, MacRae, & Dubrowski, 2007; Keetch & Lee, 2007; Wu & Magill, 2011).

Allowing learners to make decisions about their practice on a trial-by-trial basis enables them to customize their practice to suit their needs (e.g., Chiviacowsky & Wulf, 2002, 2005). An illustrative example of such performance-contingent practice comes from Keetch and Lee's (2007) investigation into the effects of self-directed practice and task difficulty on scheduling strategies and learning. Participants were required to use a computer mouse to practice four aim-and-click movement patterns of high or low difficulty, under blocked, random, self-directed, or yoked task-switching schedules. The self-directed group was the only group to improve their performance from acquisition to retention, irrespective of task difficulty. These results emerged even though the self-directed group chose to practice under task-switching conditions that were lower in CI than those imposed upon the random schedule group and that were replicated in the yoked group. The self-directed group further differed from those following experimenter-determined and yoked schedules in terms of the performance-contingent nature of their switching schedule. They appeared to make their decisions to switch based on their trial-to-trial performance, switching to a different task following better (i.e., faster, more accurate) performance. A similar pattern of results was seen by Wu and Magill (2011), who had participants practice a 3-keystroke sequence with three different relative timing structures. Irrespective of the amount of CI incorporated into practice, the self-directed group showed less error compared to a yoked group on a 24-hr transfer test. When self-directed participants decided to switch between tasks, they tended to do so in a performance-contingent manner, choosing to switch following good trials. It is likely that switching following relatively successful trials reflects insight into when challenge might be best applied.

In addition to being influenced by their current performance, self-directing learners have also been shown to be influenced by their previous practice experience (Hodges et al., 2011,

2014). For example, participants assigned to random practice of three different keystroke sequences later chose to switch between tasks more frequently than those who had experienced blocked practice (Hodges et al., 2014). It is not known whether exposure to certain forms of practice organization via a partner's practice would modulate the task-switching strategies self-directed learners choose to adopt, compared to those seen when practicing alone.

Under paired learning conditions, self-directed practice has previously been studied with respect to how learners request to use physical assistive devices. Partners took turns practising on a ski simulator task, with one member of the pair given control over when to use assistive devices (ski poles) to help them make wider and more fluid movements (Wulf et al., 2001). The other member of the pair was "yoked" to their partner's practice, such that they also had to use the ski poles (or not) on the same trials as chosen by their partner. Although the partners did not differ in terms of movement amplitude and movement frequency, the self-directed partners demonstrated a movement profile which was indicative of better movement control (i.e., they exerted force on the simulator at a more optimal time to gain efficiencies in control). There was, however, no individual practice condition which would allow conclusions about potential paired practice benefits. The authors suggested that self-control likely benefits the acquisition of movement characteristics that are difficult to pick up just from watching, but require physical practice and active awareness (e.g., relative force onset). In contrast, paired practice likely supports the learning of movement characteristics that are easily observable (e.g., movement amplitude and frequency). The authors did not report when the self-directed learners chose to use the assistive devices (e.g., following relatively good or poor trials), so we do not know how such decisions related or not to their own or their partner's performance.

1.2.2.2.2.2 Peer-directed practice

It is not unusual in physical education and sports settings for participants to passively observe their peers' practice, for instance when injured, sharing equipment, or resting between physically demanding activities. Rather than simply watching from the sidelines, there might be ways to involve the observer more actively within the practice session, ideally to optimize their observational learning, but also potentially to provide input into the practice of the person they are watching to aid the other person's learning. While this line of research is still in its infancy, there are potentially numerous benefits of involving an observer more actively within a practice session. Such study of *peer-directed* practice – where an observer is given control over aspects of a partner's practice – has the potential to offer insight into the efficacy of paired-practice methods as well as motor learning theory.

The practice choices of learners and observing peers were recently tested with respect to the provision of feedback about timing outcomes in a computerised timing task (McRae, Patterson, & Hansen, 2015). Two groups were compared, with one group allowed to decide for themselves when to receive outcome feedback (self-directed group) and the other provided feedback only at the request of their observing partner (peer-directed group). While the two groups requested feedback with similar frequency, they differed in when feedback was provided. The self-directed group requested feedback after relatively good (low error) trials (consistent with previous findings; e.g., Chiviacowsky & Wulf, 2002, 2005), whereas the peer-directed group showed no dependency of feedback provision on timing error (providing feedback after both good and bad trials). Both groups performed similarly across acquisition and in retention, suggesting that although peer-control might lead to differences in strategies regarding when to give feedback, these differences do not appear to be to the detriment (or benefit) of learning.

Considering that individuals in charge of guiding others' practice are often experienced (e.g., coaches, senior teammates), there has also been some effort to determine whether prior experience with a task influences how a peer provides feedback (McRae, 2015). Only experienced peers provided feedback to their partner after relatively good trials, consistent with how they requested feedback for themselves under self-directed conditions (see also Chiviacowsky & Wulf, 2002, 2005). Learners directed by experienced versus inexperienced peer groups did not differ with respect to errors in retention, despite these differences in strategies (and the amount of feedback provided was also similar across the two groups).

In general, learners have expressed agreement with the choices of peers, and although there have not been retention benefits associated with receiving peer-directed practice, generally measures of engagement, motivation, and trust are supportive of the idea that this active observation method can have some benefits for the non-acting partner (Karlinsky & Hodges, 2014; McRae, 2015; McRae et al., 2015). More research is required to assess how peers of varying experience differ in how they arrange practice and provide feedback and instruction, as this sharing of information among practice partners is easy to encourage and likely an efficient way to facilitate learning.

1.3 Outline of experimental design

In this dissertation, I detail five experiments that were designed to investigate the impact of a partner on learners' skill acquisition and perceptions of the practice experience, to gain a better understanding of the factors that influence motor learning. As all data collection was conducted by females, participant eligibility was restricted to females for all studies, to limit any potential within-dyad (e.g., Carli, 1989; Harskamp, Ding, & Suhre, 2008) or participant-experimenter gender-related effects (for a review, see Rumenik, Capasso, & Hendrick, 1977).

Larger joint action effects have been observed for same-sex pairs compared with opposite-sex pairs (Mussi, Marino, & Riggio, 2015; van der Weiden, Aarts, Prikken, & van Haren, 2016), potentially related to in-group/out-group categorization processes (van der Weiden et al., 2016).

All experiments took place over a minimum of two consecutive days. Day 1 of each study comprised the "acquisition" phase, during which participants practiced one or more novel tasks. Day 2 was conducted approximately 24-hours later and consisted of retention and/or transfer tests. Retention tests measure how well a practiced task is retained, dissociated from the conditions under which it was practiced, while transfer tests measure how well the activities practiced transfer to novel variations of the task (Schmidt & Lee, 2011). Learning was inferred based on performance during this delayed testing phase.

1.4 Research plan

The overall aim of my thesis is to study how motor learning is affected by the behaviours of a co-learner. The main purpose of each study is presented below, followed by a brief summary of each experiment.

- Experiment 1: Evaluating how the timing of peer observation (either during or between physical practice trials) impacts performance and learning of a balance-related task
- Experiment 2: Evaluating between-person contextual interference through matched versus mismatched practice of golf putting skills
- Experiment 3: Evaluating the effectiveness of self- versus peer-scheduled practice on motor learning of keystroke sequences
- Experiment 4: Evaluating the impact of a partner's practice schedule on self-directed practice behaviours and motor learning outcomes in a contextual interference paradigm

- Experiment 5: Evaluating the impact of a partner's error on self-directed practice behaviours and motor learning outcomes in a contextual interference paradigm

These five studies were designed to extend the breadth and depth of the dyad learning literature by introducing novel forms of dyad practice and providing the first tests of dyadic multi-skill learning. Previous research has shown that dyad practice can be an effective and efficient means of learning when practice is undertaken in alternation with a partner, or when using a part-task interlocked protocol, where partners perform complementary task components simultaneously. However, it remains unknown how observing a peer's practice during one's own practice of a whole skill would impact the immediate performance of a co-learner as well as perceptions of practice and learning. Thus, in Experiment 1, I introduced a novel form of paired practice where both learners physically practiced a whole task and observed one another at the same time. This "concurrent" dyad practice was compared with alternating dyad practice and individual physical practice of a balance-related, platform-stabilizing task. I expected that concurrent practice would be more interfering for co-learners than alternating practice, in terms of performance in acquisition. However, there was reason to expect that this increased interference/challenge would promote enhanced retention for the concurrent group. Both paired groups were expected to outperform the individual group in retention testing. What was shown, however, was that although concurrent action observation impacted online action execution (such that partners tended to show coupled movements), and it was perceived as more interfering than practicing in alternation, these differences did not impact outcome error during practice. While dyad practice was associated with higher ratings of effort than individual practice, all groups improved and showed similar immediate and delayed retention irrespective of whether

practice was alone or in pairs. There was, however, significant between-subject variability in these data and our sample size was relatively low, such that replication and extension with larger sample sizes will be necessary to make strong conclusions about the absence of learning-related benefits. For now, these data provided evidence that a partner's concurrent practice influences one's own performance, but not to the detriment (or benefit) of learning. Thus, both alternating and concurrent forms of dyad practice are viable means of enhancing the efficiency, albeit not necessarily the effectiveness, of motor learning.

In Experiment 2, I provided the first test of paired practice under conditions where learners alternated practice of multiple skills. This multi-skill learning paradigm allowed me to study the effects of between-person contextual interference (CI) and whether matching or mismatching a partner's practice is better for learning. Matching might be beneficial in terms of observing and receiving feedback about the same skills to be practised, whereas mismatching should promote CI, aiding longer-term learning. Participants alternated practice of two golf-putting skills in pairs, practising the same skill (matched group) or a different skill (mismatched group) to their partner on consecutive trials. In a control group, only one partner practised. Alternating practice did not benefit learning compared to pure physical practice and the matched and mismatched groups did not differ in outcomes. There was, however, evidence that partners were adapting their actions based on the shots of their partner, with ~70% of actions being defined by between-person compensation (i.e., putting less far if a partner overshot or putting farther if a partner undershot). Although we had no evidence that principles of CI extended to "between-person" CI in this dyad learning study, we did show that partners were influencing performance, if not ultimately at a benefit (or cost) to overall learning.

In Experiment 3, I introduced a novel way partners can be used to bring contextual interference into a learner's practice: peer-directed practice. This method was compared to self-directed practice to assess whether the benefits of self-directed practice are related to the customized nature of the practice schedule, or the more general opportunity to make practice-related decisions. Within each pair, one person physically practiced three keystroke sequences, each with a different timing goal. I anticipated that both self- and peer-directed learners would experience performance-dependent practice scheduling, but that those in the latter group would do so without also experiencing the putatively more motivating and cognitively effortful opportunity to make their own task-switching decisions. If this opportunity for decision-making is important for learning, the self-directed learners would be expected to show enhanced retention compared to peer-directed learners. Both peer-schedulers and self-schedulers showed performance-dependent practice, making decisions to switch to a new sequence based on timing error. However, peer-schedulers generally chose for their partners to switch between skills more than self-schedulers, although this was not related to retention for either group. Importantly, self-scheduled learners did not differ in retention from peer-scheduled learners. Peer-scheduled practice was rated as more motivating and enjoyable than self-scheduled practice. In view of the lack of difference in retention and the positive ratings of peer-scheduled practice, I concluded that it is the adaptive nature of practice that is important for learning and that peer-directed practice is an effective alternative practice method to self-directed practice.

Experiment 3 showed that both self-directed learners and peer-schedulers made decisions based on the learner's current level of performance. It is unknown, however, how self-controlled practice decisions might be modulated in dyad practice, where observing a peer's practice could impact how learners make choices about their own. Thus, in Experiment 4, I was interested in

whether and how exposure to a partner's (blocked, random, or self-directed) practice schedule would impact self-directed practice choices and learning of three keystroke timing tasks. Partners alternated turns after 9-trial blocks of practice to promote the saliency of the partner's assigned practice schedule. If partners are influenced by each other in terms of how they practice, then people paired with a random-schedule partner would be expected to bring more switching into their practice than those paired with a blocked-schedule partner. Self-directed learners showed both own error-dependent practice (switching tasks following better performance) and partner-dependent practice, with random-schedule partners promoting an increase in task-switching frequency. A partner's schedule also impacted learning. Random practice resulted in better timing accuracy than blocked practice for both partners in an immediate and delayed retention test. These data provided evidence that self-directed practice behaviours and learning outcomes can be modulated by a partner's practice schedule.

In Experiment 5, I continued to study self-controlled task-switching schedules of keystroke timing tasks, but this time partners alternated turns with a blocked- or random-schedule partner on consecutive trials. This trial-to-trial turn-taking allowed me to determine whether switching between tasks was primarily based on one's own performance or that of their partner, as well as to replicate effects from Experiment 4 regarding the influence of a partner's schedule on task-switching frequency. Like previous studies with individuals directing their own practice, I expected to see performance-contingent task-switching with reference to learners' own previous trial (i.e., repeating tasks following higher error). Individuals might also decide to repeat a partner's task if it was performed errorfully, akin to their self-referenced strategy, or they might choose to repeat a partner's task if it was performed well, instead choosing to imitate successful demonstrations. As with Experiment 4, practice with a blocked partner facilitated

timing accuracy in acquisition but a random partner promoted task-switching and enhanced retention. Learners showed both “own-error-” and “partner-error-” dependent switching strategies during practice, choosing to repeat the same sequence on their next turn when they performed poorly or when their partner performed well. These results provided further evidence that learners are influenced by their partners, at least in terms of their behaviours and practice choices.

Overall, the behavioural data across five studies showed that learners’ motor performance and practice-related decisions are influenced by a partner’s practice, but that these immediate effects do not robustly impact longer-term learning compared to physical practice alone. These findings are the result of innovative approaches to the study of dyad learning, including unique tests of concurrent dyad practice (Experiment 1) and peer-directed practice (Experiment 3), as well as novel investigations into alternating dyad practice when co-learners practice multiple skills (Experiments 2, 4, and 5). These experiments are further detailed in the following chapters, where I discuss theoretical and experimental rationale for the studies and relate the findings to concepts concerning the efficiency and effectiveness of dyad practice, as well as to empirical and applied issues of motor learning more broadly.

Chapter 2: Experiment 1

Turn-taking and concurrent dyad practice aid efficiency but not effectiveness of motor learning in a balance-related task

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2.1 Introduction

People often learn motor skills in a social setting with a similarly skilled co-learner. One of the advantages of practicing in a group is that you can watch your partner and learn from their performance. However, there may also be disadvantages associated with practicing alongside someone else, where observing another person's movements could interfere with your own. Here we compared individual practice to two forms of dyad (paired) practice, where partners either took turns physically practicing and observing one another, or physically practiced and observed one another concurrently. Our aim was to provide insight into how the timing of peer observation impacts immediate performance and longer-term learning, as well as learners' subjective perceptions of such dyad practice experiences.

2.1.1 Observation within dyad practice

2.1.1.1 Alternating practice

How should co-learners practice together to maximize training effectiveness and efficiency? Here we refer to the effectiveness of dyad practice as increased task performance in retention and/or transfer relative to individual conditions and to the efficiency of dyad practice as decreased trainer time and resources compared to practice alone. In the latter case, efficiency would be gained if two individuals were trained in the same amount of time typically devoted to one (Shebilske, Jordan, Goettl, & Paulus, 1998). Research has shown that such potential benefits are achieved through a turn-taking form of paired practice, wherein partners alternate between physical and observational practice (e.g., Granados & Wulf, 2007; Shea, Wright, Wulf, & Whitacre, 2000; Shea, Wulf, & Whitacre, 1999). Here, efficiency is gained as one partner is able to practice during the rest periods for the other. Moreover, the opportunity to observe the partner during rest also affords efficiency and this activity is thought to enhance error detection and

correction processes, as well as potentially encourage greater engagement and attention than that afforded by practice alone (see Hodges & Ste-Marie, 2013; Lewthwaite & Wulf, 2012; Shea et al., 1999, 2000; Shebilske, Jordan, Arthur, & Regian, 1993; Shebilske et al., 1998; Shebilske, Regian, Arthur, & Jordan, 1992). Interspersing physical practice with demonstrations is thought to compensate for some of the costs of pure physical or observational practice, with this type of mixed practice supporting both robust retention of the practiced skill(s) (typically promoted by physical practice) as well as more generalizable transfer to novel versions of the practiced tasks (typically promoted by observational practice; see Hodges & Ste-Marie, 2013; Ong & Hodges, 2012).

The benefits of turn-taking practice for retention and transfer have been illustrated by Wulf, Shea and colleagues (Granados & Wulf, 2007; Shea et al., 1999, 2000). For example, alternating practice of a computerized, key-press tracking task with a partner was more effective than pure physical practice alone, which in turn was more effective than pure observational practice (Shea et al., 2000). This pattern of results highlights potential additive benefits of practicing with a partner, particularly when observation can help to determine the task goal and relevant strategies (e.g., Granados & Wulf, 2007; Shebilske et al., 1992). Moreover, dyadic alternating practice of a stabiliometer balance task was more effective than individual physical practice, but also more than dyadic practice that was blocked (whereby partners switched roles half way through, Shea et al., 1999). As the amount of physical and observational practice was matched between the two dyad groups, it seems that the interactive nature of turn-taking underpinned the success of paired practice. While partners could also discuss the balance task in this study (Shea et al., 1999), the interspersed observation of a co-learner was later shown to be more important than discussion in a follow-up experiment using a different (cup-stacking) task

(Granados & Wulf, 2007; see also Hebert, 2018, for a replication and extension of this paradigm).

In addition to the potential informational benefits of interleaved physical and observational practice, dyad practice might also impact the learning experience through a variety of other pathways. For instance, well studied social facilitation effects may play a role in aiding dyad practice, whereby the presence of an observer impacts cognitive and/or motor performance. In studies of social facilitation, an observer or audience typically enhances the performance of simple/well-learned tasks and interferes with that of more complex tasks (e.g., Zajonc, 1965; for a review and meta-analysis, see Strauss, 2002 and Bond & Titus, 1983, respectively). Importantly, the efficacy of alternating practice has been shown to be more than the product of social facilitation effects, as previous studies have controlled for such contributions by comparing multiple forms of dyad practice (e.g., Granados & Wulf, 2007; Shea et al., 1999, 2000).

2.1.1.2 Concurrent practice

There are many situations where learners do not practice skills in a temporally independent fashion (i.e., taking turns), but rather practice concurrently with fellow learners (e.g., in yoga, dance, or team-based sports). To our knowledge, concurrent practice of the same motor skill has not yet been studied, despite many real-world contexts where individuals physically practice independently in the presence of co-learners and the potential efficiency associated with this method of practice.

Concurrent practice has been studied in a task-sharing context (e.g., Arthur, Day, Bennett, McNelly, & Jordan, 1997; Day, Paulus, Arthur, & Fein, 2003; Shebilske et al., 1992, 1993, 1998). Using the militaristic video-game “Space Fortress”, Shebilske and colleagues

showed that "part-task" training with a partner (what they referred to as an 'active-interlocked-modeling' (AIM) protocol) led to skill retention that did not differ from that acquired from training on the whole-task alone (e.g., Shebilske et al., 1992). In effect, the shared practice protocol afforded a 100% increase in training efficiency, as two people could be trained in the time it took to train one. However, Space Fortress is a unique task that affords a clear distribution of responsibilities and complementary part-practice with a partner, whereas this is not the case for most motor and particularly sport skills (see also Shea et al., 1999). In the current study, we provide a first look at concurrent practice of a whole task by having both members of a pair simultaneously serve as actors and observers during practice of a novel motor skill. This scenario raises the question of how the observed actions of a partner immediately influence online action production and what this might mean for longer-term motor learning.

2.1.2 Concurrent observation and execution of action

2.1.2.1 Motor contagion during action observation

A growing body of research provides evidence for interactions between observed and executed actions, such that observing movements can result in 'motor contagion', which can facilitate or interfere with simultaneous action execution (for reviews, see Blakemore & Frith, 2005; Schütz-Bosbach & Prinz, 2007; Sebanz & Shiffrar, 2007). It has been shown that even when observers are not required to perform a motor task (other than to remain standing), action observation can elicit behavioural responses (e.g., Sebanz & Shiffrar, 2007; Tia, Paizis, Mourey, & Pozzo, 2012; Tia et al., 2011). Viewing actors in states of imbalance disturbed passive observers' own postural stability and different postural reactions were elicited based on the orientation of the actor (e.g., Sebanz & Shiffrar, 2007; Tia et al., 2012). Of particular relevance to the current study, observing an actor in disequilibrium face-on (i.e., third-person perspective)

gave rise to 'imitative' movements, such that the observer showed a propensity for 'mirrored' movements (Sebanz & Shiffrar, 2007). Such observation-induced movements have been referred to as 'ideomotor movements', due to the link between the idea and the action (e.g., De Maeght & Prinz, 2004; Häberle, Schütz-Bosbach, Laboissière, & Prinz, 2008; Knuf, Aschersleben, & Prinz, 2001). This terminology is derived from the ideomotor theory (James, 1890) and later extensions of this theory which have provided a framework for conceptualizing links between the imagination, perception, and production of action (e.g., Greenwald, 1970, 1972; Hommel, Müssler, Aschersleben, & Prinz, 2001; Prinz, 1997).

2.1.2.2 Motor contagion during action production

There is also compelling evidence that engaging in action observation during simultaneous action execution modulates online motor performance of intentional movements. This is thought to be due to the activation of overlapping neural structures involved in both action-related activities (e.g., Kilner, Paulignan, & Blakemore, 2003). Observation of biological movements, incongruous to the to-be-enacted movements, had an interference effect on action (Kilner et al., 2003). When participants made arm movements while observing an actor or a robot making congruent or incongruent movements, participants' movement variance (indexing interference in motor control) was increased only when observing the human incongruent movements (Kilner et al., 2003). Subsequent studies have since elucidated that a critical feature eliciting this interference is the velocity profile of biological motion (e.g., Bouquet, Gaurier, Shipley, Toussaint, & Blandin, 2007; Kilner, Hamilton, & Blakemore, 2007), that motor contagion is greater when viewing goal-directed compared to non-goal-directed actions (Bouquet, Shipley, Capa, & Marshall, 2011), and that some contagion effects may at least in part be due to the execution of incongruent eye movements (Constable et al., 2016).

While there are clearly interactions between concurrent observation and execution of actions, much remains to be learned about the conditions (and boundaries) of such contagion mechanisms. For instance, such research has been largely limited to the manipulations' immediate effects on action production and perception, with little empirical attention devoted to the learning process and any potential lasting effects on perceptual-motor processes (for exceptions, see Celnik et al., 2006; Stefan, Classen, Celnik, & Cohen, 2008; Weeks, Hall, & Anderson, 1996). Additionally, motor contagion paradigms have involved a unidirectional relationship between an experimenter or videoed model as the actor and a participant. Here we extend this line of research to study the bidirectional influence of a partner's practice on concurrent, independent motor performance, where there is the potential for partner-related interference during practice. Practice conditions that place higher demands on learners' information processing capabilities (e.g., through increased interference) have been shown to hinder acquisition performance but benefit learning compared to less challenging conditions (see Guadagnoli & Lee, 2004; Lee, Swinnen, & Serrien, 1994; Schmidt & Bjork, 1992). Social practice conditions might also engender increased effort related to being observed (e.g., Strauss, 2002), as well as potentially creating an element of competition when the relative success of a co-actor's performance is easily observed (e.g., Rhea, Landers, Alvar, & Arent, 2003).

2.1.3 Study Aims

We implemented a unique dyad-practice paradigm, whereby co-learners engaged in concurrent physical and observational practice of a stabiliometer balance task. We compared this to turn-taking and individual (physical practice only) forms of practice (see Shea et al., 1999). Our aim was to determine if and how observing a partner's practice, and particularly the timing of this peer observation, influences practice behaviours, learning outcomes, and subjective

perceptions of the practice experience (see also Day et al., 2003 for a comparison of AIM and turn-taking protocols). Based on previous findings (Shea et al., 1999), we anticipated that turn-taking would enhance learning compared to individual practice, potentially because of opportunities to engage in additional problem-solving activities via observation of a learning model (e.g., Lee et al., 1994).

In addition to processes related to information processing, it may be that practice and learning are moderated by factors related to motivational factors associated with practice in pairs, including increased intrinsic motivation towards the task (e.g., Lewthwaite & Wulf, 2012), and/or increased effort, as a function of being observed (e.g., Strauss, 2002). Therefore, in addition to studying outcome and learning effects associated with dyad practice, we were also interested in impacts of shared practice on markers, or regulators of motivation. According to Self Determination Theory (SDT), intrinsic motivation is theorized to occur when three fundamental psychological needs for autonomy, competence, and relatedness are satisfied (e.g., Ryan & Deci, 2000, 2017). We were particularly interested in how practising in pairs, versus alone, would impact factors related to intrinsic motivation towards the task, including interest/enjoyment, perceived competence and choice, pressure/tension and effort. There is reason to think that these factors may also differentially drive learning in alternating versus concurrent conditions, such that factors related to pressure/tension and effort would be higher when practising concurrently, versus in alternation, because of the immediate feedback provided by a partner in online conditions. Therefore, we used the Intrinsic Motivation Inventory (IMI; Deci & Ryan, n.d.) to assess motivational variables related to the various forms of practice, within the SDT framework. We also formulated our own customized questions to probe

perceptions of the dyad practice experience with respect to potential interference or benefits from learning together as well as perceptions of competition.

To study the effects of a partner on practice (and potentially retention), we also measured online motor performance. Concurrent observation of a partner could potentially interfere with online movements or provide a stabilizing role. We know that incongruent movements lead to interference in the form of increased action variability (e.g., Kilner et al., 2003), particularly in balance-based tasks (e.g., Sebanz & Shiffrar, 2007; Tia et al., 2011, 2012). As such, concurrent practice with a partner might be expected to degrade performance during practice as compared to alternating or alone conditions. However, it may be that watching someone perform similar movements has a stabilizing role, where concurrent practice would be associated with greater coupling between partners' movements (see Eaves, Hodges, & Williams, 2008; Sebanz & Shiffrar, 2007). With respect to long-term learning, there is evidence that conditions of practice that serve to challenge the individual, resulting in more cognitively effortful practice, can promote better long-term learning as measured in retention tests (e.g., Lee et al., 1994; Schmidt & Bjork, 1992). Concurrent practice with a partner might be considered a more challenging condition than practice alone, due to the potential for interference from the partner's movements.

2.2 Methods

2.2.1 Participants and Groups

Thirty-six females (*M* = 23.2 yr, *SD* = 4.6) volunteered to participate individually and were reimbursed for their time ($10.25/hr). Participants had normal or corrected-to-normal vision, no injury to the lower back or limbs, and limited balance training (gymnasts and dancers were not eligible to participate, nor were individuals with experience using balance training devices). Participants were pseudo-randomly assigned based on availability to practice

individually (Alone group) or to practice in pairs, with pairs randomly assigned to the Turn-taking or Concurrent group (n = 12/group). The study was conducted in accordance with the ethical guidelines of the university and all participants provided informed consent.

2.2.2 Task and Apparatus

The experimental task was based on Shea et al. (1999) and involved learning to balance under individual or paired conditions on one of two stabiliometer platforms (Lafayette Instruments, IN). The task was to keep the stabiliometer platform in a horizontal/balanced position for as long as possible during each trial. The front edges of the two stabiliometers were positioned 0.5 m apart. Two black rectangles (45 cm × 30 cm) were marked on each platform to indicate where the feet should be positioned (one foot on either side of the midline, in the centre of each rectangle). The displacement of each platform was measured by the addition of a potentiometer, which detected angular deviation (sampled at 100 Hz). A Toshiba laptop was used to control trial initiation and termination and record the potentiometer data via customized LABview programming.

2.2.3 Procedure

The study was conducted over two consecutive days, with Day 1 completed individually or in pairs and Day 2 completed alone. Within each pair, one partner was randomly assigned to be Partner 1 and the other to be Partner 2, which dictated which stabiliometer was used during practice (all participants used the same stabiliometer for test phases). For control purposes, the Alone group participants were also assigned to be Partner 1 or Partner 2 in alternation.

On Day 1, the platforms were raised 18 cm from their base, such that their maximum possible deviation to either side of horizontal was 21°. For the paired groups, Day 1 began with a brief period during which the experimenter initiated some exchange with the participants (who

did not know one another), before running participants through the familiarization and pretest phases individually. When one partner was being tested the other waited outside and responded to questionnaires regarding their handedness (Oldfield, 1971) and previous balance training experiences. During task familiarization, the experimenter explained to participants how to position themselves when on the stabiliometer. They were instructed to keep the platform as level as possible, to keep their arms crossed while balancing, and to visually focus on a fixation cross that was attached to a wall at chest level. No other instructions were given. After a brief warmup, participants completed one, 60-s pretest trial. This and all subsequent trials started with the participant holding onto a safety bar and bringing the platform into the horizontal position. On the experimenter's 'go', data collection was initiated and the participant brought their arms into the crossed position to begin balancing. Participants were told their 'percent time-on-target' (%TOT) score for this pretest trial and familiarized with the meaning of this measure. All participants used the same stabiliometer for familiarization and all test phases, and across all groups, the Partner 1s used this same platform for practice, while the Partner 2s used the second platform.

Participants received twelve, 60-s practice trials either individually (Alone group) or together (Turn-taking and Concurrent groups). In the Concurrent group, partners each stood on one of the platforms facing one another and attempted to balance simultaneously. In the Turn-taking group, partners alternated trials physically practicing and observing their partner. When observing, participants sat in a chair facing their partner's platform, to mimic the observation perspective of the Concurrent pairs. During physical practice trials, participants were asked to focus on a cross that was attached at approximately chest level to the wall (Alone and Turn-taking groups) or to their partner's body (Concurrent group). Participants rested for ~60 s at the

end of each trial. Participants were asked not to discuss the task at any point during the study. To encourage participants to attend to their partner, dyad group participants were asked to write down an estimate of their partner's %TOT. Participants also estimated their own %TOT after their own trials.[1] No feedback about %TOT was provided (see Shea et al., 1999).

After the acquisition session, participants responded to the Intrinsic Motivation Inventory (IMI) consisting of the interest/enjoyment (7 items), perceived choice (5 items), perceived competence (5 items), pressure/tension (5 items), and effort subscales (5 items; Deci & Ryan, n.d.). The wording of the IMI items was customized to the task, such that the original references to the "task" were modified to specify the "balancing task" (e.g., "Doing the balancing task was fun"). Dyad groups additionally responded to an adapted version of the perceived competence subscale (5 items), probing their perceptions of their partner's competence. These adaptations consisted of changing references to oneself within the items (e.g., "I am", "I did", "my performance") to references to the partner (e.g., "my partner is", "my partner did", "my partner's performance"). For example, the competence item became "I think my partner is pretty good at this balancing task". Participants in the dyad groups additionally rated the truthfulness of the following three items, to provide insight into how helpful and interfering they found watching their partner, as well as their desire to be more accurate than their partner: 1) "Watching my partner helped my own performance/learning", 2) "Watching my partner interfered with my own performance/learning", 3) "I wanted to be more accurate than my partner". Responses to all questionnaire items were based on a 7-point Likert-type scale, where 1 = *not at all true* and 7 =

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[1] Analysis of the estimation data (i.e., absolute difference between actual percent time-on-target (%TOT) and estimated %TOT) did not yield any group or estimation-type (Own, Partner) effects. Participants were just as good at estimating their own error as they were for their partner. We did not expect this to depend on group.

very true. Upon completing the questionnaires, participants individually performed an immediate retention test (post-test) consisting of three, 60-s trials. Partners completed this test in the same order as for the pretest.

Participants returned alone the next day for delayed testing. The platform was raised such that its maximum possible deviation to either side of the horizontal was 27°. This transfer test consisted of six, 60-s trials. We decided to increase the difficulty of this test (i.e., increase height), to better probe how well individuals had learnt to balance and compensate for errors, given our predictions that practice with a partner would aid error detection and correction processes. At the end of testing, participants were debriefed and compensated for their time.

2.2.4 Measures and Analysis

2.2.4.1 Error

For each 60-s trial, each platform's potentiometer data was transformed into degrees out of balance, and the average deviation of the platform was calculated as root-mean-square error (RMSE; e.g., Shea et al., 1999; Wulf, Lewthwaite, & Hooyman, 2013).[2] Because of our specific predictions, instead of testing for the overall group effect and then running post hoc tests, we ran two orthogonal, preplanned contrasts comparing; 1) the individual group to the two paired groups combined and 2) the two paired groups to each other. RMSE data were analyzed for each phase and submitted to separate Group (Alone vs. Pairs or Turn-taking vs. Concurrent) × Trial ANOVAs, with repeated measures (RM) on the last factor.[3]

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[2] We also analyzed %TOT; the results mirrored those of RMSE and were omitted for brevity.

[3] Pilot testing revealed that despite being set at the same height and having the same maximum deviation, one stabiliometer was associated with lower error than the other. We labelled this 'easier' platform as stabiliometer 2 (which was an older, wooden platform compared to the newer, metal version). All Partner 2s used stabiliometer 2 for acquisition, while all participants completed testing phases on stabiliometer 1. As anticipated, the Partner 2s had lower error across acquisition (M = 4.7°, SD = 1.7°) than the Partner 1s (M = 6.7°, SD = 2.3°), $F(1, 30) = 12.93$, p =

2.2.4.2 Coupling

For each acquisition trial, the data were first filtered with a fourth-order low-pass Butterworth filter (cut-off frequency = 8 Hz). The velocity profiles of the Concurrent partners' platforms were compared (based on the 100 Hz sampling), and the overall percentage of the trial during which the platforms moved the same direction in space was computed. This was also done for the Turn-taking pairs and Alone group 'pairs' to provide a reference of the 'coupling' that occurred between partners by chance. These data were submitted to a 3 Group × 12 Trial RM ANOVA. We again ran preplanned contrasts on these data, although this time our primary interest was the comparison between the Concurrent group and the two other groups, which did not practice at the same time as a partner.

2.2.4.3 Questionnaires

Participants' mean score of all the items in each multi-item subscale of the IMI (interest/enjoyment, perceived choice, perceived competence, pressure/tension, effort) and the adapted partner-related competence scale was calculated and first submitted to an omnibus MANOVA analysis, with group as the between variable. Preplanned contrasts based on univariate ANOVAs were used for follow-up analyses comparing the paired groups to the individual group and then the two paired groups to each other. For the dyad groups, an additional 2 Group (Turn-taking, Concurrent) × 2 Competence (Own, Partner) RM ANOVA was conducted. For the current sample, Cronbach's alpha values were good for the interest/enjoyment ($\alpha = 0.91$), perceived competence ($\alpha = 0.87$), and pressure/tension ($\alpha = 0.83$) subscales;

.001, $\eta_p^2 = .30$. However, as there was no Partner × Trial interaction in acquisition, $F(6.00, 180.11) = 1.19$, $p = .31$, 1 – ß = .46, nor any partner-related effects or interactions in any other phase of the study when all participants used stabiliometer 1, we attributed this partner-effect to the more stable platform and collapsed across partners in all reported analyses and results.

however, the values were weak to acceptable for the adapted partner-related competence (α = 0.78), perceived choice (α = 0.57), and effort (α = 0.69) subscales, and therefore the results of these scales should be interpreted with caution. Responses to the three dyad practice experience questions were analyzed using separate univariate ANOVAs.

Greenhouse-Geisser corrections were applied to the degrees of freedom for violations to sphericity. Significant effects were followed up with Tukey's HSD procedures where relevant (all *p*s < .05 reported). Partial eta squared values (η_p^2) and Cohen's *d* are reported as measures of effect size for all significant effects, and power values (1 – ß) are given for non-statistically significant effects where $F > 1$.

2.3 Results

2.3.1 Measures of balance performance

2.3.1.1 Error

The groups' mean RMSE across the study is presented in Figure 2. There were no group-related differences in the pretest (Alone vs. Pairs contrast, $p = .97$; Concurrent vs. Turn-taking contrast, $p = .81$). Groups improved across practice, $F(6.04, 199.46) = 25.30$, $p < .001$, $\eta_p^2 = .43$, confirmed by a linear trend component to the trial effect ($p < .001$). Surprisingly, performance in acquisition did not vary as a function of group (Alone vs. Pairs contrast, $p = .83$; Concurrent vs. Turn-taking contrast, $p = .34$). A similar pattern was observed in the individual, immediate retention test (post-test) and in delayed transfer testing on the higher platform (i.e., improvements across trials but no group differences evidenced for any of the contrasts).

2.3.1.2 Coupling

Analysis of the movement coupling between pairs (i.e., the percent of each acquisition trial where partners moved in the same direction in space) revealed that the Concurrent pairs

displayed significantly more coupling (M = 53.9%, SD = 4.8%) than the Turn-taking (M = 50.7%, SD = 4.6%) and Alone pairs (M = 51.0%, SD = 3.8%), although these differences were small. Statistical differences were confirmed based on preplanned contrasts comparing the Concurrent group to the mean of the two non-concurrent groups (M = 50.9%, SD = 4.2%), $p < .001$, $d = 0.68$ (the latter two groups did not differ from each other, $p = .72$). Examples of the Concurrent pairs' performance and movement coupling during acquisition are presented in Figure 3 (depicting each pair's final acquisition trial). Although there was no main effect of trial ($F < 1$), there was a Group × Trial interaction, $F(22, 165) = 1.92$, $p = .011$, $\eta_p^2 = .20$. Only after the first trial did the Concurrent pairs generally show greater coupling than their non-concurrent counterparts.

Figure 2. Root-mean-square error (RMSE) in degrees of the Alone, Concurrent, and Turn-taking groups across the study.

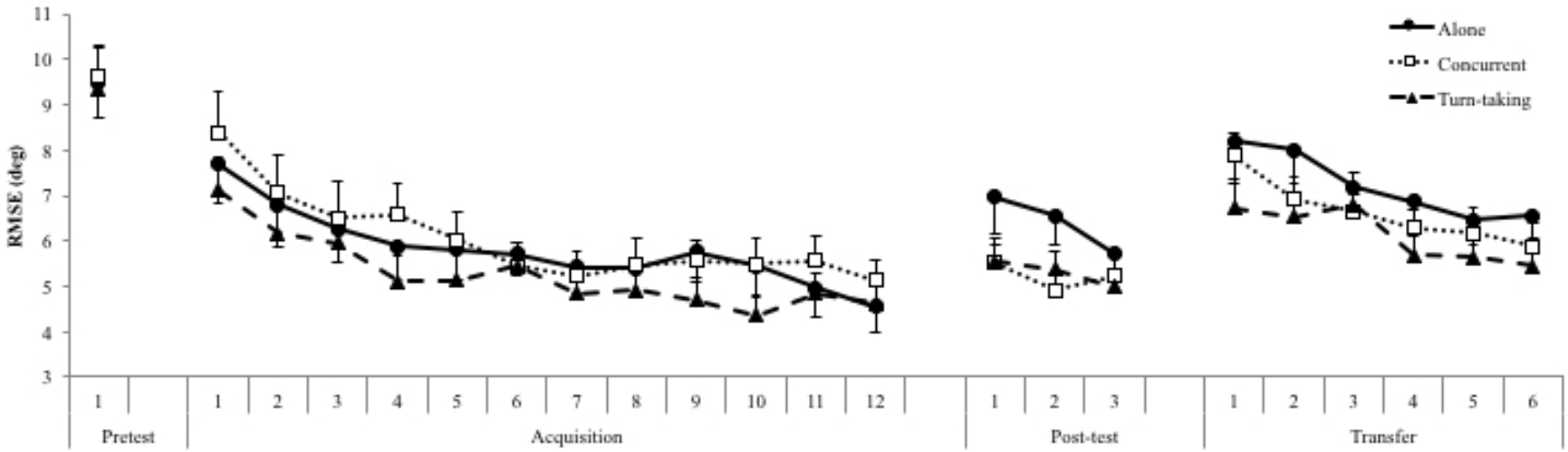
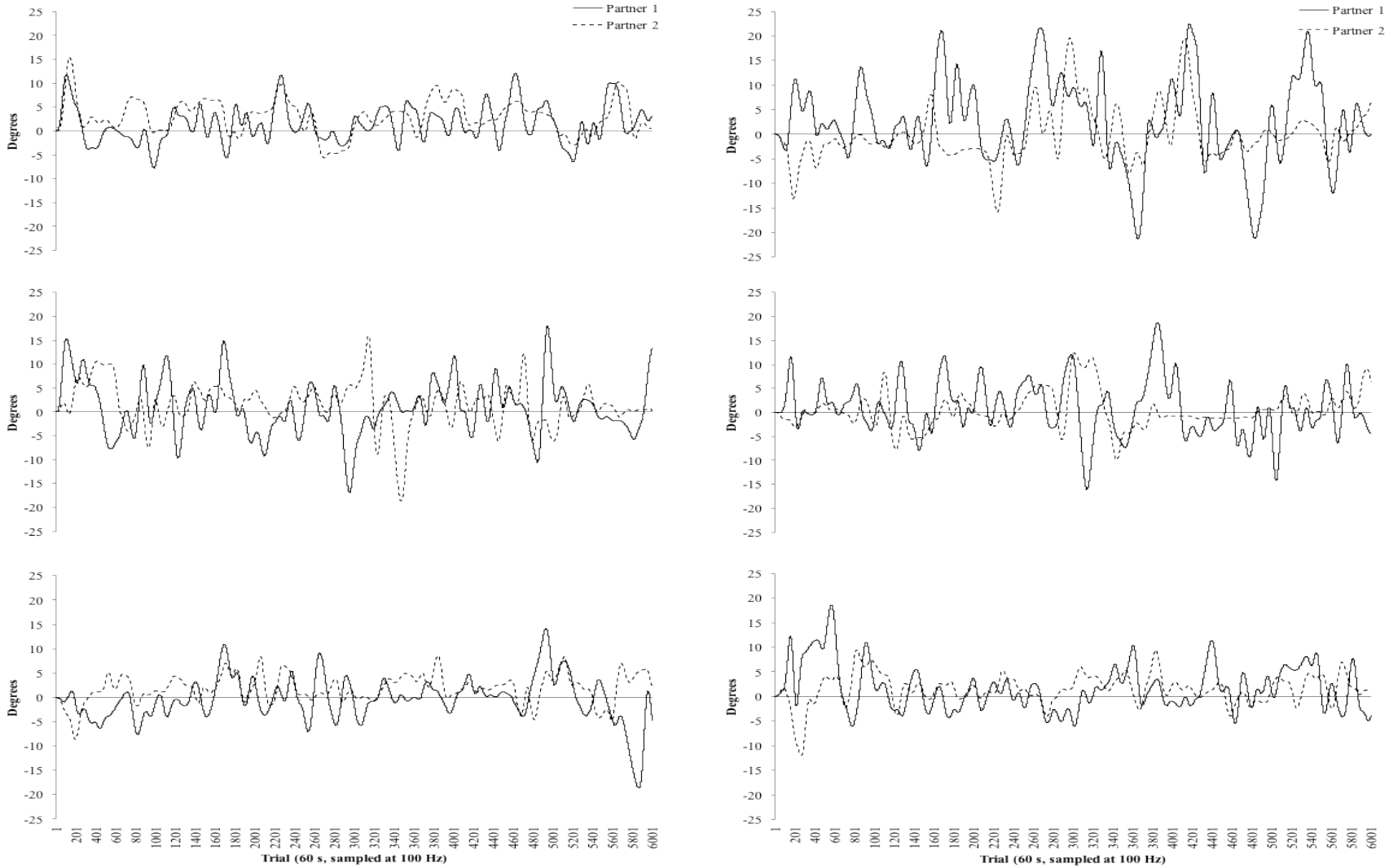


Figure 3. Displacement data for the last acquisition trial for pairs 1 to 3 (left side, top-bottom) and pairs 4 to 6 (right side, top-bottom) in the Concurrent group, with % coupling being 59%, 57%, 60%, 61%, 58%, and 54%, respectively.



2.3.2 Measures of affective experiences

2.3.2.1 Intrinsic Motivation Inventory

Responses to the IMI are presented in Table 2. The preplanned contrast between the individual and pair groups revealed that practice with a partner was associated with higher ratings of effort (M = 6.15, SD = 0.56) than practice alone (M = 5.33, SD = 0.85), p = .001, d = 1.15. There were no significant differences in learners' perceptions of interest/enjoyment (p = .47), competence (p = .28), choice (p = .74), or pressure/tension (p = .15). When comparing the two dyad groups, perceptions of practice did not vary as a function of concurrent versus turn-taking practice (all ps > .32). Dyad participants rated their partner's competence as higher than their own, $F(1, 22) = 9.89$, $p = .005$, $\eta_p^2 = .31$, but this did not depend on group ($F < 1$).

2.3.2.2 Paired practice experience

Observing a partner's practice was perceived as relatively helpful by both the Concurrent (M = 4.4, SD = 2.2) and Turn-taking groups (M = 4.8, SD = 0.8), $F < 1$. As anticipated, concurrent partners rated watching their partner as more interfering for their own performance (M = 3.7, SD = 2.1) than turn-taking partners (M = 2.1, SD = 1.0), $F(1, 22) = 5.75$, $p = .025$, $\eta_p^2 = .21$. There was no difference between the Concurrent (M = 5.3, SD = 1.9) and Turn-taking (M = 5.8, SD = 1.5) groups' desire to be more accurate than their partner ($F < 1$).

Table 2. Mean ratings (and *SD*s) for the Intrinsic Motivation Inventory subscales and customized partner-related competence subscale.

Group	Interest/ enjoyment	Competence		Choice	Pressure/ tension	Effort
		Own	Partner			
Alone	4.5 (1.0)	3.9 (1.1)	N/A	5.7 (0.8)	3.0 (0.9)	5.3 (0.9)
Concurrent	4.5 (1.1)	4.2 (1.2)	5.1 (0.8)	5.6 (1.9)	3.4 (1.3)	6.1 (0.4)
Turn-taking	5.0 (1.1)	4.5 (1.0)	5.4 (0.9)	5.5 (0.9)	3.9 (1.7)	6.2 (0.7)

Note. Scales ranged from 1–7, with higher scores representing more of the relevant construct.

2.4 Discussion

The challenge of how best to incorporate observation into the motor learning process is of long-standing interest to researchers and practitioners alike. Dyad practice is a method that achieves this aim, whilst being high in external validity and efficiency and potentially promoting other positive benefits associated with this more social type of practice. In previous studies, both dyadic alternating and concurrent part-practice were shown to modulate motor performance and learning compared to practice alone (e.g., Granados & Wulf, 2007; Shea et al., 1999, 2000; Shebilske et al., 1992). To date, concurrent practice of a whole skill for "independent" learners has not been studied, even though this method is efficient and allows study of how the presence of a partner influences performance online in addition to longer-term learning.

Physically practicing at the same time as another learner was expected to interfere with performance (e.g., Kilner et al., 2003). While concurrent practice was indeed perceived as more interfering than turn-taking practice, this perceived interference did not manifest as greater error during practice. Of note, the dyad groups' similar performance might still reflect different

underpinning mechanisms and pathways to learning (e.g., turn-taking practice might be more strategy-mediated whilst concurrent practice might be more effort-mediated). However, there were no differences between the groups with respect to the IMI.

Concurrent observation of a partner did however impact acquisition in another manner; co-learners' movements showed more coupling, showing a greater (though small) tendency to move in a directionally compatible way. This extends previous research showing that viewing unstable postures elicits bodily sway in the observer (e.g., Tia et al., 2011, 2012), and that these observation-induced movements are imitative (mirror-like) in nature when face-to-face with the actor (Sebanz & Shiffrar, 2007). It is likely that if learners, or at least one co-learner, was more errorful during practice, this would have promoted increased error within concurrent practice. In future studies it will be important to manipulate the skill level (and error) of the partner to test this hypothesis. Additionally, because the concurrent situation elicited coupled behaviour, the observed and executed movements were not strictly incongruent (*cf.* Kilner et al., 2003) and likely diminished interference-related effects. As such, we would be less likely to find differences in retention as a result of challenge or practice-related interference (*cf.* Guadagnoli & Lee, 2004).

Observing an actor from the front (i.e., facing) has been shown to elicit greater postural sway than that elicited from the side (Tia et al., 2012). Observing an unstable actor from the front has also been shown to promote imitative movements as opposed to compensatory movements that are elicited when viewing an actor from behind (i.e., a first-person perspective; Sebanz & Shiffrar, 2007). In the current study, individuals in the Concurrent group were always facing each other and as such, it was not possible to make conclusions about how perspective moderates the coupling between the partners. Such differing perspectives would provide additional insight

into when and why individuals' motor performance is susceptible to the behaviours of others (and what this might mean for learning).

Contrary to a similar study using this stabiliometer task and dyad practice, we did not show any evidence that alternating practice was beneficial to performance or learning compared to pure physical practice alone (*cf.* Shea et al., 1999). The major difference between our protocol and that of Shea and colleagues' was that we did not allow partners to discuss the task, and all participants were told at the start of practice how to stand (i.e., arms crossed, looking straight ahead). This suggests that these procedural details (i.e., more opportunity to learn how to stand from watching and to discuss why) were responsible for the positive benefits in this earlier study and not necessarily turn-taking per se. However, in subsequent work, albeit with a different task, observation and not dialogue underpinned the benefits of alternating practice (Granados & Wulf, 2007). It was also shown that it was this alternating schedule that was responsible for benefits of observation, rather than a period of watching followed by all physical practice, or vice versa (Shea et al., 1999). However, the benefits may also have been related to the rest between trials (which was not controlled between Shea et al.'s two dyad groups), rather than observation per se, given that distributed practice or spacing effects impact memory processes (Donovan & Radosevich, 1999; Lee & Genovese, 1988). Additionally, neither the previous dyad practice studies (e.g., Granados & Wulf, 2007; Shea et al., 1999), nor our own, controlled for mental practice during rest intervals (for example, by providing a distraction task during breaks). Some participants may have engaged in more practice-related activities than others, such that more control over this interval may be necessary in future work.

Despite the lack of group differences with respect to dyad practice in our study, it is important to point out that these methods did not incur costs and hence there are potential

efficiencies to be gained from this type of practice compared to practice alone. Two people could be trained in the same amount of time as that of an individual learner. In our study, as in other alternating dyad practice paradigms (e.g., Granados & Wulf, 2007; Shea et al., 1999), each member of the dyad received the same amount of physical practice as the individual learners (with breaks interspersed into the individual practice protocol to match the duration of the dyad training protocol). While there is evidence that physical practice of task components can be reduced when engaging in concurrent, part-practice with a partner (e.g., Shebilske et al., 1992), we did not test whether practice with a partner can compensate for and potentially replace some physical practice of the whole skill. This would be particularly important for training settings where there are high physiological demands, training-related expenses, or limited training time. While we chose to match the durations of practice and rest between individual and dyad groups, it will also be important to compare dyad practice to individual practice conditions where learners can self-pace their rest intervals and/or self-direct their amount of practice (e.g., Post, Fairbrother, & Barros, 2011). This would allow further testing of potential efficiency benefits to be gained from dyad practice when individual practice time is less controlled and potentially more representative of practical settings.

Given our interest in the impact of partners on the learning process more generally and the potential social benefits to be gained from practicing in pairs, we also asked participants to respond to the Intrinsic Motivation Inventory (Deci & Ryan, n.d.). Participants rated their practice experience positively, but contrary to predictions, paired practice was not associated with higher perceptions of interest/enjoyment compared to individual practice (*cf*. Lewthwaite & Wulf, 2012). It is possible that the subjective experience of paired practice would have been enhanced had co-learners been given increased opportunity to interact and potentially contribute

to one another's learning experience more explicitly (e.g., encouragement during or between practice trials, discussion during inter-trial breaks; e.g., Granados & Wulf, 2007; Shea et al., 1999). It is important to note, though, that the IMI scales we used did not necessarily capture the range of motives along the SDM continuum (see Ryan & Deci, 2000, 2017). Indeed, a number of external factors likely mediate motivation in lab-based settings (e.g., financial compensation, being observed by an experimenter and in some cases a peer), that were not picked up by our measures. It is also relevant to note that in our study and presumably others involving dyad practice (e.g., Granados & Wulf, 2007; Shea et al., 1999), an experimenter was present within all conditions. As such, any potential social facilitation effects would have been moderated by the presence of the experimenter potentially masking differences between individual and dyad practice conditions that might exist in less supervised, real-world settings.

Dyad practice was, as hypothesized, associated with higher effort. Being in a pair can of course encourage competition and social comparisons that are not necessarily positive and hence there is a need to assess potential learning effects against these general perceptions of the learning environment. It may be that perceptions of competence moderate any potential benefits from seeing and interacting with a partner. Indeed, there was a tendency to perceive a partner's competence as higher than one's own which may have impacted potential benefits. Comparative feedback about others' motor skill performance (e.g., a peer or group average) has been shown to impact one's own competency beliefs as well as learning (e.g., Wulf, Chiviacowsky, & Lewthwaite, 2010). Whereas the majority of research evaluating social-comparative feedback effects has been based on virtual or experimenter-provided comparisons (e.g., Wulf et al., 2010, 2013), personal perceptions of a veridical partner are potentially more meaningful to a learner. Interestingly, the opportunity for social comparisons within an ecologically valid social setting

(weight training class) enhanced performance in competitive contexts compared to non-competitive contexts (Rhea et al., 2003). Further research is required to delineate when and why genuine comparative feedback impacts not only immediate performance but also longer-term behavioural outcomes.

In summary, we have provided the first evidence that concurrent dyad practice of a whole skill is as effective as individual and turn-taking practice, despite the potential for interference brought about by perception-action links. Partners that practiced concurrently showed susceptibility to the movements of their partner, but not to the detriment of their learning (nor to their benefit). This study adds to the growing literature showing that practicing with a partner is an effective and efficient means of practice, which can be undertaken in various forms with positive outcomes (see also Karlinsky & Hodges, 2014 and McRae, Patterson, & Hansen, 2015 for peer-directed practice; and Shebilske et al., 1992 for 'interlocked' practice). However, our results also suggest that while dyad training protocols typically enhance the efficiency of practice, they do not always result in superior learning compared to practice alone and may be perceived as more effortful and interfering, and encourage comparative judgments that may not be favourable for learning. Considering the ecological validity of peer-based practice settings, where learners frequently observe and potentially compare themselves to their peers (e.g., physical education classes, physical activity classes, or team sports), it will be important to continue such inquiries into when and why such conditions support learning.

2.5 Bridging summary

In Experiment 1, we assessed the impacts of observing a partner during versus interspersed with physical practice. While concurrent action observation was perceived as more interfering than alternating practice and appeared to "interfere" with partners' motor

performance by promoting coupled movements, it did not impact error during practice nor how well the balance-related task was acquired. Because the concurrent situation elicited coupled behaviour, we did not achieve strict incongruency between the observed and executed movements, which likely diminished interference-related effects (e.g., Kilner et al., 2003). A more controlled way to manipulate incongruency in dyad practice would be to assign co-learners to perform and observe different skills. Thus, in Experiment 2, we ventured into multi-skill dyad practice, which allowed us to manipulate the congruency of the tasks co-learners observed and executed in turn, and thereby provide the first test of inter-person contextual interference.

Chapter 3: Experiment 2

Manipulations to between-person contextual interference through mismatched practice of golf putting skills

A version of this manuscript has been accepted for publication in *Human Movement Science:* Karlinsky, A., & Hodges, N. J. (accepted). Manipulations to practice organization of golf putting skills through interleaved matched or mismatched practice with a partner. *Human Movement Science.*

3.1 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 both individuals? Much has been written about practice organization for individuals when there is a need to acquire more than one skill, with evidence pointing towards variety in practice order to optimize learning (e.g., Kantak & Winstein, 2012; Lee, 2012). To date, principles of practice organization have rarely been tested in social settings, where variability is an inherent part of practice (as a result of watching and executing actions and taking turns). Here we limit our focus to practice in pairs, whereby turn-taking co-learners practice novel motor skills in either the same 'matched' schedule or different 'mismatched' schedules, alternating physical practice trials with rest and observation.

For practice of individual skills, there is evidence that turn-taking practice can enhance learning compared to practice alone (e.g., Granados & Wulf, 2007; Shea, Wright, Wulf, & Whitacre, 2000; Shea, Wulf, & Whitacre, 1999; *cf.* Karlinsky & Hodges, 2018b). Turn-taking allows individuals to observe a co-learner's practice in between their own physical practice attempts. These interspersed 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), which can immediately be enacted.

The research on turn-taking practice to-date has typically involved the learning of a single motor skill. Consequently, what co-learners observed has matched 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 paradigms, the outcome feedback from a partner's trial is directly 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 or scale the observer's own actions through experience of variability in a wide(r) range of parameters. For the learning of multiple skills, this parameter-based variability would be secondary to acquiring the actual skills.

The order, or schedule, in which learners practice different skills is known to be important for motor learning. This has been well-documented in terms of the "contextual interference" (CI) effect, first shown in a motor learning context by Shea and Morgan (1979). This effect captures the paradox whereby relatively "low interference" blocked practice schedules (wherein one skill is practiced repeatedly before switching to a different skill) yield better performance in practice, but relatively "high interference" random practice schedules (wherein skills are practiced unsystematically) are associated with better retention and transfer (for a review, see Lee, 2012).

While most CI-related research has focused on individual learners in isolated contexts, practice organization has also been considered within dyad-like conditions, when a learner has assumed either an actor or observer role (e.g., Blandin, Proteau, & Alain, 1994; Karlinsky & Hodges, 2014; Wright, Li, & Coady, 1997). Such paradigms have provided some evidence of CI effects following observational practice (e.g., Blandin et al., 1994; Wright et al., 1997), but they

have not addressed potential interactions between co-learners' practice schedules under conditions where both physical and observational practice are allowed.

A paradigm that has been used to investigate how between-trial events can moderate the CI effect is one where "correct" demonstrations were interspersed between an individual's physical practice trials (Lee, Wishart, Cunningham, & Carnahan, 1997; Simon & Bjork, 2002). In one study, participants followed a blocked or random 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) that either matched or mismatched the imminent task were presented before each physical practice trial to diminish or augment the degree of interference, respectively. Consistent with the typical CI effect, random groups performed worse in practice but more accurately in retention. Importantly, mismatched models also impaired acquisition and augmented retention for both blocked and random practice groups. Thus, interspersed demonstrations amplified the typical CI effect, providing further support for the implementation of practice conditions that promote cognitive effort and challenge individuals' information processing activities to enhance learning. Of note, Richardson (as cited in Guadagnoli & Lee, 2004) showed a different pattern of results when the keystroke sequences required more complex relative timing goals (as opposed to overall timing goals). 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.

Thus, 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). It is unclear, however, whether interspersed demonstrations impact multi-skill learning in dyads, when the modeled information is provided by a learning model and the relationship between what is practiced and what is observed varies. We have recently shown that multi-skill learning in pairs impacts self-controlled practice organization for a keypress, absolute-timing task, but the effects were more noticeable in behaviors rather than learning outcomes (Karlinsky & Hodges, 2018a). We do not know the potential impact of turn-taking when co-learners' practice schedules are experimenter-controlled to either provide matched or mismatched models. 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 et al., 1999; Simon & Bjork, 2002). From a theoretical perspective, testing if and how partners impact the learning of co-learners helps to understand when and why contextual interference works, and whether motor learning effects and principles derived from the study of individuals generalize to multi-person learning.

In the current study, we modulated the CI within a learner's physical practice schedule by manipulating the interleaved practice of a partner. A golf putting task was used with two different putters requiring standing or sitting putting. Partners either matched the putter of their partner or used a different putter. In the former case, between-person CI would be low, 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 feedback to aid with scaling of the upcoming skill. We expected to see evidence of between-person scaling in the matched condition, where over or undershooting behaviors in the partner would be "compensated" for in

the partner by opposite patterns of errors. In the latter case, where partners perform and observe different skills in alternation, between-person CI would be high. As such, practice would be more cognitively effortful, potentially aiding long-term retention. However, the information gained from watching should be less, given that a partner's feedback would not be relevant to the current skill. This should show up in between-trial behaviors 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 would be more effective than pure physical practice (e.g., Granados & Wulf, 2007; Shea et al., 1999, 2000).

To gain insight into learners' subjective experience of the practice conditions, participants completed the Intrinsic Motivation Inventory (Deci & Ryan, n.d.), to answer questions about interest/enjoyment, competence, choice, and pressure/tension. Because there is some suggestion that mixing physical and observational practice has a motivational benefit (e.g., Schmidt & Lee, 2014; Shea et al., 1999), we hypothesized that the alternating pairs would rate their 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). Participants additionally responded to customized questions probing their perceptions of the practice experience.

3.2 Methods

3.2.1 Participants and Groups

Seventy-six right-handed females (*M* = 22.1 yr, *SD* = 5.3 yr) volunteered to participate and were paid $10/hr. All participants had normal or corrected-to-normal vision, no known neurological or motor disorders, limited golf putting experience, and provided informed consent. 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 each pair, partners were randomly assigned to be Partner 1 (P1) or Partner 2 (P2). In the control group, the physical practice participants ("Actors") followed the same schedule as P2s.

3.2.2 Task and Apparatus

Participants had to putt a golf ball so that it stopped in the center 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 (Figure 4A) and the miniature putter was used seated, facing the target (Figure 4B). 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.

Figure 4. Photographs of the (A) standard golf putter (used standing), and (B) miniature golf putter (used seated).

3.2.3 Materials

We used two questionnaires. The Task Evaluation Questionnaire is a 22-item version of the Intrinsic Motivation Inventory designed to assess interest and attitudes toward an experimental task (Deci & Ryan, n.d.). It 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 practice experience questionnaire which included three questions which were rated on a similar, 7-point Likert scale for truthfulness: 1) Watching my partner helped my own performance/learning, 2) Watching my partner interfered with my own performance/learning, and 3) I wanted to be more accurate than my partner.

3.2.4 Procedure

The study was conducted over three days, with days 1 and 2 partially completed in pairs and "day 3" (one week later) completed alone (see Table 3). 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 so that it landed as close as possible to the target center. Instructions on how to hold the standard and miniature putters were provided and the experimenter demonstrated the general putting technique without hitting a ball.

Following a short familiarization phase (3 attempts/putter with order counterbalanced), participants completed a pretest without feedback (wearing occlusion glasses and ear protectors). Feedback was removed to prevent acquisition in the pretest. 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 customized golf experience and handedness (Oldfield, 1971) questionnaires in another room.

In acquisition, the matched and mismatched groups were instructed to watch one another's practice but not to communicate about the putting tasks. Partners alternated turns, with P1 putting on trial 1 and P2 using the same putter (matched group) or different putter (mismatched group) on trial 2 etc. Although partners alternated turns, participants followed a semi-blocked practice schedule, using their assigned putter for six trials before switching to the other putter. Outcome feedback was given on every trial and partners always stood in a designated location near the target when watching (so they could see both the putting action from the side and the ball's trajectory).

In the control group, “Observers” watched their “Actor” partner practice from the same observation location as the alternating pairs. Observers were 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 alternating groups. Importantly, we used the same putter schedule for all P2s (including the Actor controls) and only manipulated the P1’s schedule according to group. As such, our main comparisons between the groups concerned the P2s who only differed in terms of between-partner interference but not within-person contextual interference.

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. Each 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 (Deci & Ryan, 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 the three custom questions about their practice experience. Participants were then fully debriefed and compensated for their time.

Table 3. 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).

3.2.5 Measures and Analysis

3.2.5.1 Error

The distance of the ball to the target center 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 an 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 alternating practice dyad groups. Secondary analyses were conducted to compare alternating dyad practice to physical practice (i.e., Actors). 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. Separate analyses were conducted for pretest, Acquisition day 1 and day 2, and Retention day 2 and day 3. The practice data were grouped into 6-trial blocks (one putter/block); hence, an additional RM factor of Block was included in this analysis. In retention, an additional RM factor of feedback was included.

In secondary analyses, comparisons were also made across the P2s only (i.e., matched, mismatched, and Actors), and across the Observers and Actors. The same tests as above were used, with the group factor reflecting these additional comparisons.

3.2.5.2 Behaviors

We also studied two types of compensatory (or corrective) behaviors exhibited in practice, which give an indication of performance-based adjustments based on one's own error (self-referenced) or a partner's error (partner-referenced). These behaviors were determined based on whether an overshoot or undershoot of the target in the y-dimension resulted in compensation (i.e., putt less far or putt farther) on the subsequent trial for that individual or for their partner. We determined the percentage of acquisition trials where the matched and mismatched pairs "compensated" for their own preceding error, as well as relative to the partner's preceding trial. These data were analyzed in a 2 Group × 2 Partner × 2 Day × 2 Reference (self, partner) mixed ANOVA, with RM on the last two factors. As with the error data, we also ran a secondary analysis comparing just the P2s' self-referenced compensation data as compared to the Actors, in a 3 Group × 2 Day mixed ANOVA.

3.2.5.3 Questionnaires

Participants' average score for each subscale of the Task Evaluation Questionnaire was submitted to separate ANOVAs, to first test for differences between the matched and mismatched alternating groups and second to compare the Actors to the matched and mismatched P2s. For the current sample, Cronbach's alpha values were good for the interest/enjoyment ($\alpha = .93$), perceived competence ($\alpha = .86$), and pressure/tension ($\alpha = .86$) subscales; however, reliability for the perceived choice subscale was weak ($\alpha = .69$), so we do

not report these data. Similar analyses were used for each of the three paired practice experience questions.

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 $F > 1.5$.

3.3 Results

3.3.1 Error

3.3.1.1 Pretest

The matched and mismatched groups' mean RE and BVE in testing are presented in Figure 5. In the pretest, there were no significant differences in RE ($F = 1.1$; Figure 5A & B). There was no Partner main effect, $F(1, 44) = 2.41$, $p = .13$, 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. Importantly, the P2s did not look different across groups (for P2 pretest scores, see Figure 6). BVE (Figure 5C and D) showed the same pattern of results (no differences between groups or partners, *F*s < 1; Group × Partner: $F(1, 44) = 1.67$, $p = .20$, 1 – ß = .24; see Figure 6C & D). Participants performed better using the standard putter than the miniature putter for RE, $F(1, 44) = 11.12$, $p = .002$, $\eta_p^2 = .20$, and BVE, $F(1, 44) = 5.16$, $p = .028$, $\eta_p^2 = .11$. Additional analyses of the P2s only (matched, mismatched, and Actor controls) showed no between-group differences in RE ($F = 1.4$) or BVE, $F(2, 33) = 1.73$, $p = .19$, 1 – ß = .34 (see Figure 6). Similarly, as illustrated in Figure 7,

comparisons of the Actors with the Observers showed no differences in RE, $F(1, 22) = 2.06$, $p = .17$, $1 - ß = .28$, or BVE ($F < 1$).

Figure 5. Error in testing as a function of matched versus mismatched alternating practice in pairs. Top panel: Mean radial error (and *SE* bars) for the (A) standard putter, and (B) miniature putter. Bottom panel: Mean bivariate variable error (and *SE* bars) for the (C) standard putter, and (D) miniature putter. FB = feedback.

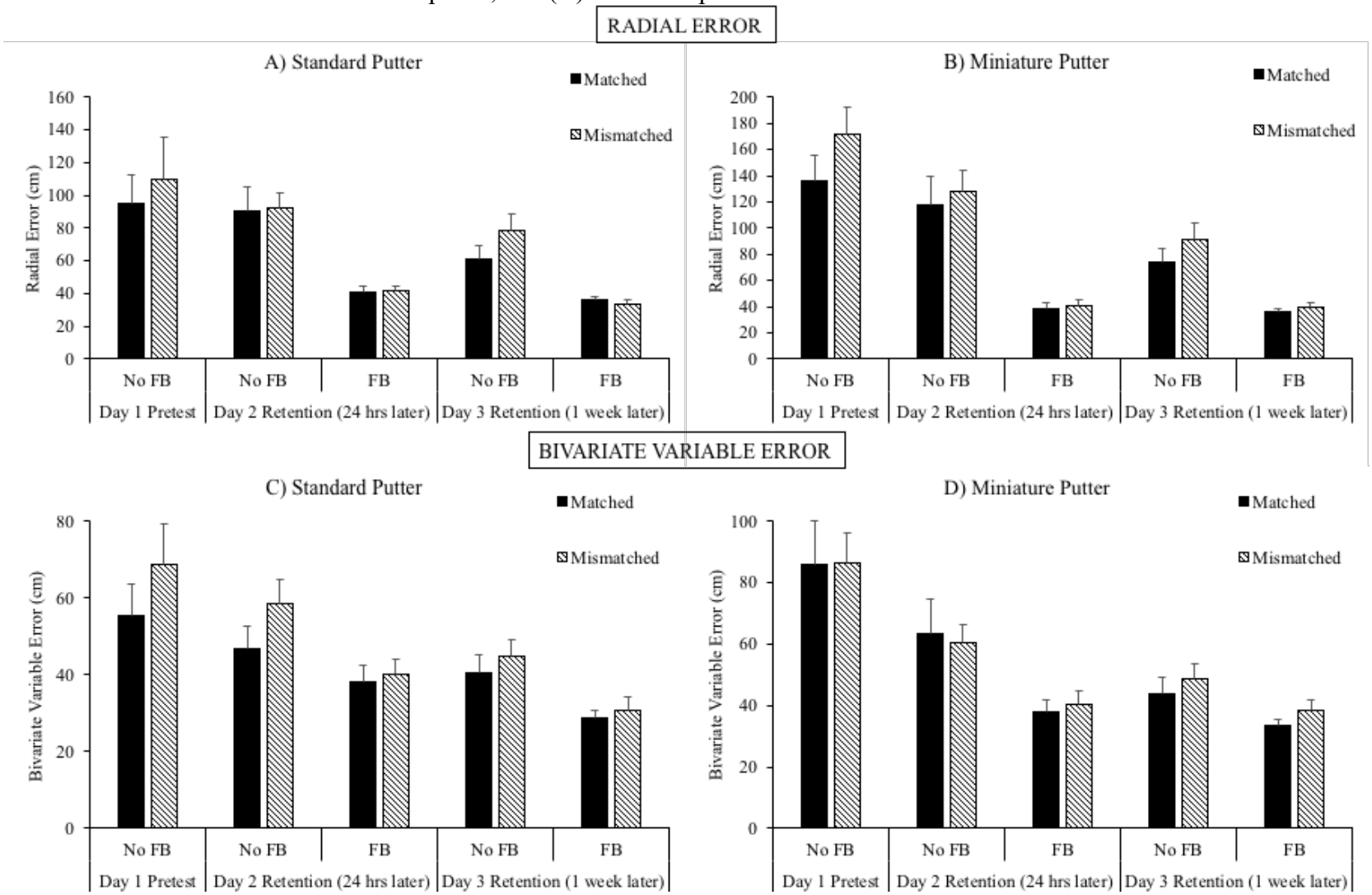


Figure 6. Error in testing for all Partner 2s (P2s), as a function of pure physical practice (Actors) or matched versus mismatched alternating practice. Top panel: Mean radial error (and *SE* bars) for the (A) standard putter, and (B) miniature putter. Bottom panel: Mean bivariate variable error (and *SE* bars) for the (C) standard putter, and (D) miniature putter. FB = feedback.

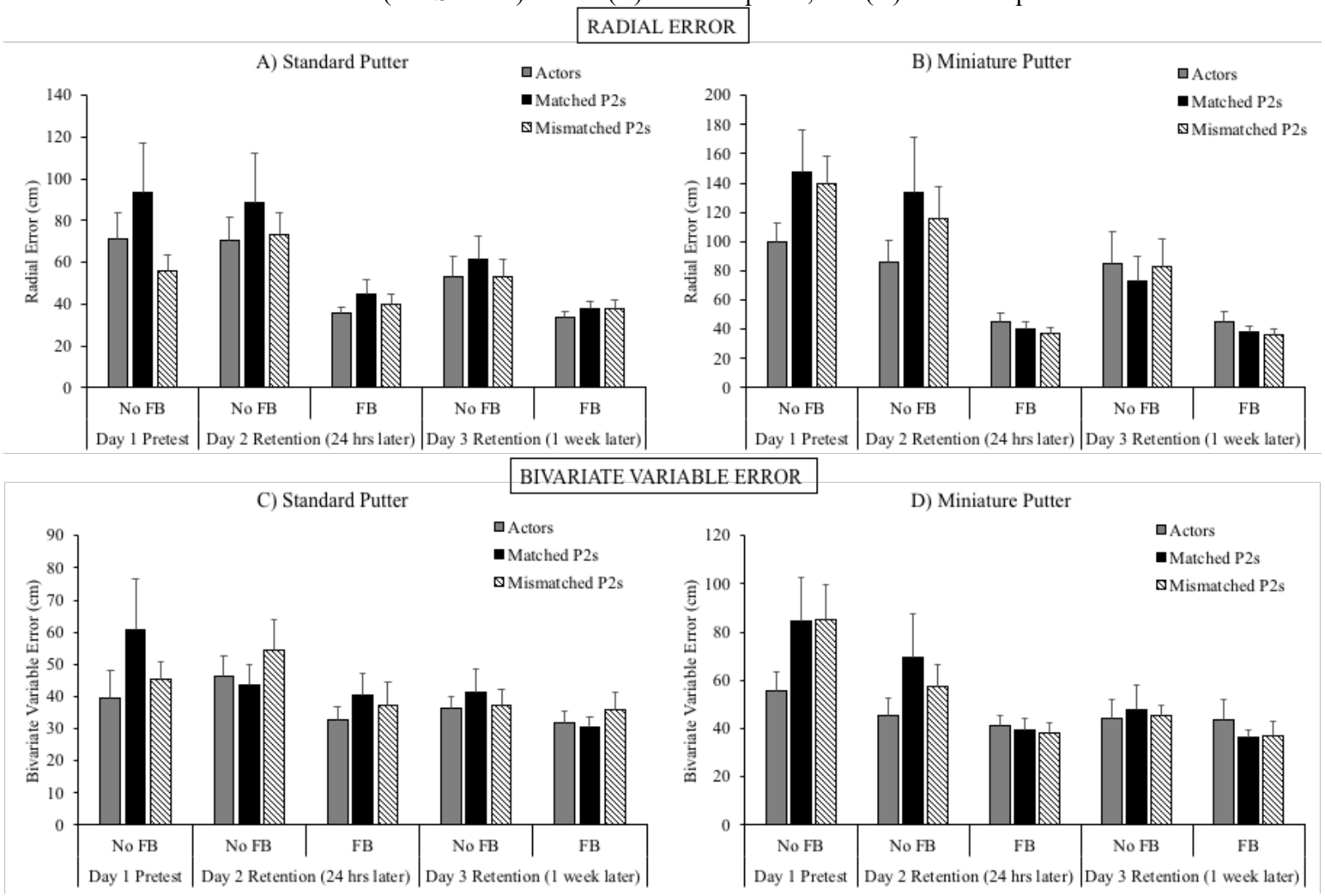
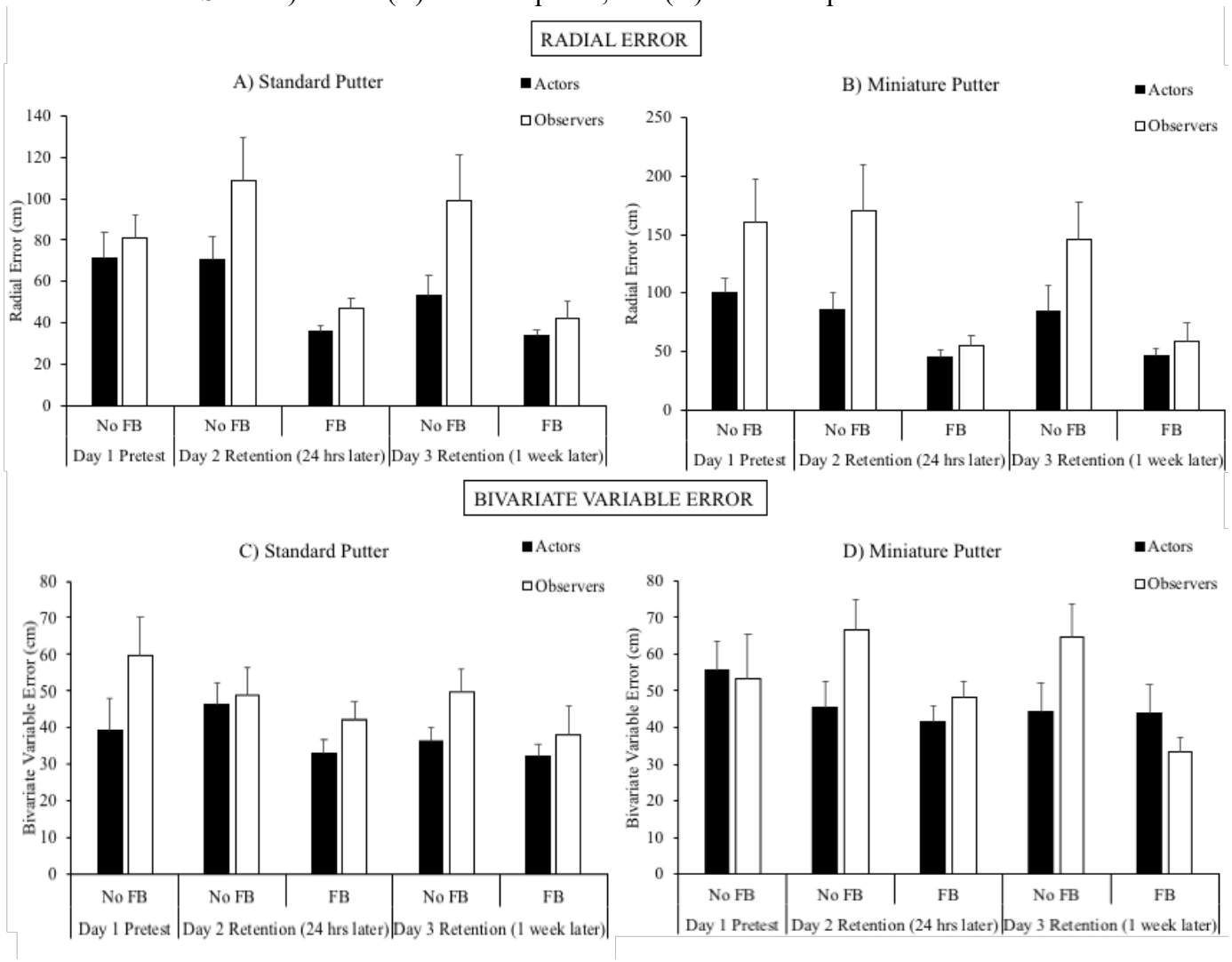


Figure 7. Error in testing as a function of physical practice (Actors) versus observational practice (Observers). Top panel: Mean radial error (and *SE* bars) for the (A) standard putter, and (B) miniature putter. Bottom panel: Mean bivariate variable error (and *SE* bars) for the (C) standard putter, and (D) miniature putter. FB = feedback.



3.3.1.2 Acquisition and retention

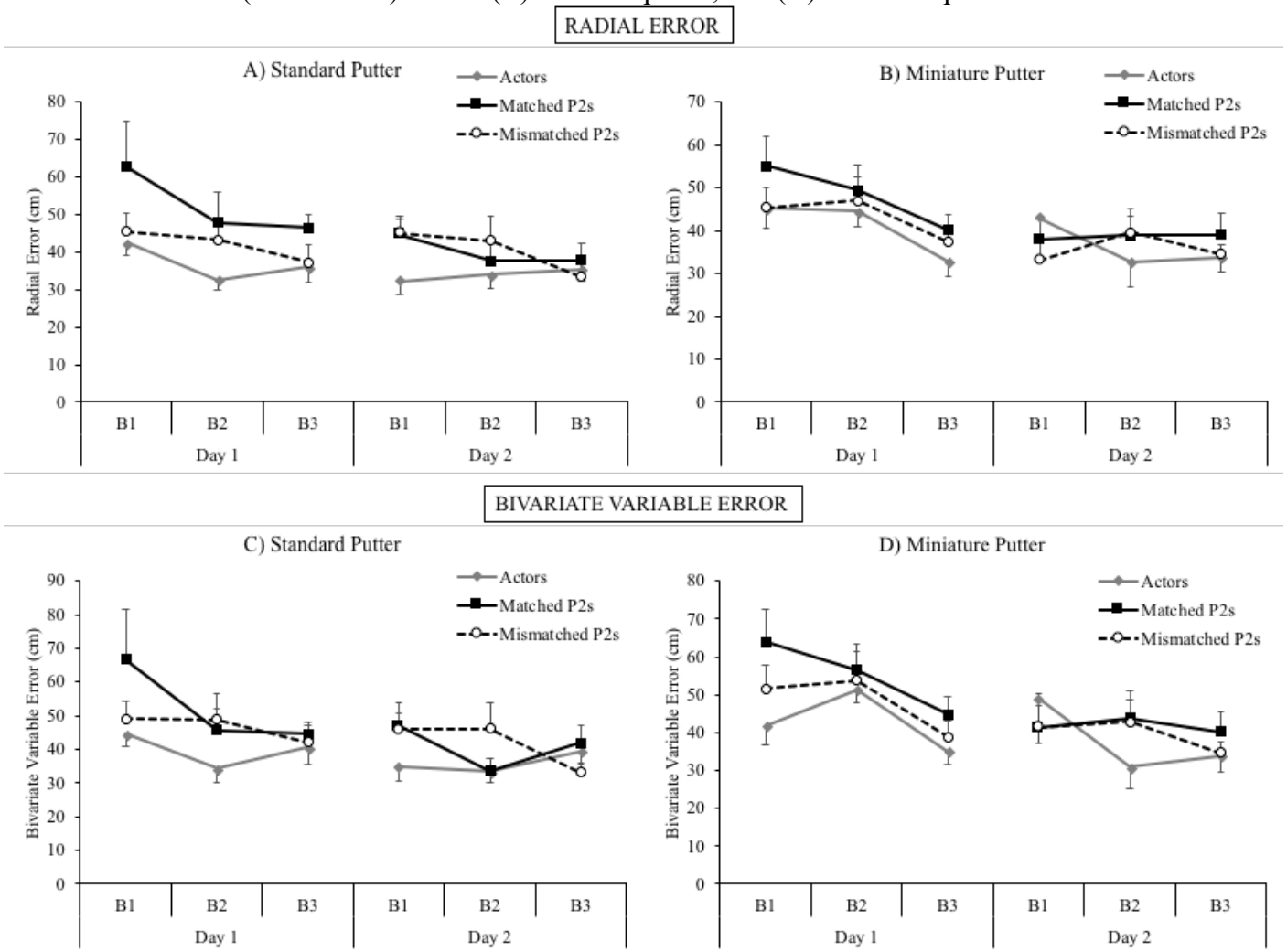
There were no differences between the matched and mismatched groups on either day of practice (*F*s < 1). There were also no partner-related effects on the first day of practice, with respect to putting accuracy ($F < 1$) or variability, $F(1, 44) = 2.69$, $p = .18$, $1 - ß = .36$, nor any Group × Partner interactions. Participants improved their accuracy and consistency across day 1, confirmed by linear contrasts for block effects (*p*s = .001). These improvements did not differ based on group or partner. During day 2 acquisition, both groups improved their accuracy (but not consistency) with practice, confirmed by a linear trend component to the block effect ($p = .004$). There were no group- or partner-related effects or interactions.

There were also no differences in retention, as illustrated in Figure 5. On day 2, there were no group-related effects for RE (*F*s < 1; Group × Partner: $F(1, 44) = 1.52$, $p = .22$, $1 - ß = .23$) or BVE (*F*s < 1). Having vision and outcome feedback aided accuracy, $F(1, 44) = 44.44$, $p < .001$, $\eta_p^2 = .50$, and consistency, $F(1, 44) = 13.92$, $p = .001$, $\eta_p^2 = .24$, and participants were again more accurate with the standard compared to the miniature putter, $F(1, 44) = 8.49$, $p = .006$, $\eta_p^2 = .16$ (especially in the no-feedback test as confirmed by an interaction, $F(1, 44) = 11.12$, $p = .002$, $\eta_p^2 = .20$). Variability did not differ across putters, $F(1, 44) = 1.72$, $p = .20$, $1 - ß = .25$. 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 P2s, comparing across the Actors and the two alternating dyad groups, also failed to show differences in acquisition (Figure 8) and retention (Figure 6). Only on day 1 of practice were the group-related *F* values >1.5, due to a trend in greater accuracy in the alone group (RE, $F(2, 33) = 2.91$, $p = .069$, $1 - ß = .53$; BVE, $F(2, 33) = 2.77$, $p = .077$, $1 - ß = .51$).

For illustration, we have compared the Actors to the Observers in Figure 7 across the pretest and retention tests. On day 2, the Actors tended to perform better than the Observers on the retention tests, in terms of lower RE, $F(1, 22) = 3.93$, $p = .060$, $1 - ß = .48$, and BVE, $F(1, 22) = 3.43$, $p = .077$, $1 - ß = .43$. These differences were less pronounced the following week, for both RE, $F(1, 22) = 2.74$, $p = .11$, $1 - ß = .35$, and BVE, $F(1, 22) = 1.38$, $p = .25$, $1 - ß = .20$. However, the Actors still tended to be more accurate than the Observers in the absence of feedback, $F(1, 22) = 3.85$, $p = .062$, $1 - ß = .47$, and were more consistent using the miniature putter, as shown by a Role × Putter interaction, $F(1, 22) = 5.07$, $p = .035$, $\eta_p^2 = .19$.

Figure 8. Error in acquisition for all Partner 2s (P2s), as a function of pure physical practice (Actors) or matched versus mismatched alternating practice. Top panel: Mean radial error (and *SE* bars) for the (A) standard putter, and (B) miniature putter. Bottom panel: Mean bivariate variable error (and *SE* bars) for the (C) standard putter, and (D) miniature putter. All blocks consisted of 6 trials.



3.3.2 Behaviors

As illustrated in Figure 9, all P2s tended to compensate for their own previous outcome errors (i.e., increasing or decreasing the distance of their putt if they previously undershot or overshot the target, respectively), with similar frequency on both days of practice, regardless of whether they practiced in alternation with a partner or alone (Fs < 1; Group × Day: $F(2, 33) = 1.21$, $p = .31$, $1 - ß = .25$).

Analysis of the matched and mismatched dyad groups additionally revealed that the tendency to display compensatory behaviors was also there at a between-person level. The frequency of such behaviors was not different when comparing the participant's own previous trial to their partner's previous trial ($F < 1$). Contrary to expectations, this pattern of compensation also did not differ as a function of group. There were no group-related effects or interactions (Group × Day × Reference: $F(1, 44) = 2.07$, $p = .16$, $1 - ß = .29$; all other Fs < 1). An effect of Partner revealed that the P2s generally compensated more frequently ($M = 74\%$, $SD = 7\%$), than the P1s ($M = 71\%$, $SD = 14\%$), $F(1, 44) = 6.34$, $p = .016$, $\eta_p^2 = .13$. There was a Partner × Reference interaction, $F(1, 44) = 6.43$, $p = .015$, $\eta_p^2 = .13$, but Tukey post-hoc tests showed that there were no significant differences between the means.

In light of the high frequency with which compensatory behaviors could be viewed as relevant towards both the participant's own and their partner's previous trial's y-dimension error, we looked at the percentage of trials which showed only partner-referenced compensation (collapsed across practice days). In Figure 10 we have illustrated the four various compensation scenarios along with % of trials which characterized these various scenarios. The vast majority of trials showed compensatory behaviors which could be classed as both self- and partner-referenced. The percentage of trials which showed only partner-referenced switching was small

(17%). When we just analyzed these trials as a function of Group (based on independent *t*-tests), there was no group effect, $t(46) = .73$, $p = .47$, $d = .21$.

Figure 9. Mean percentage of acquisition trials where Partners 2s (P2s) compensated for the y-dimension error of the preceding self- or partner-referenced trial.

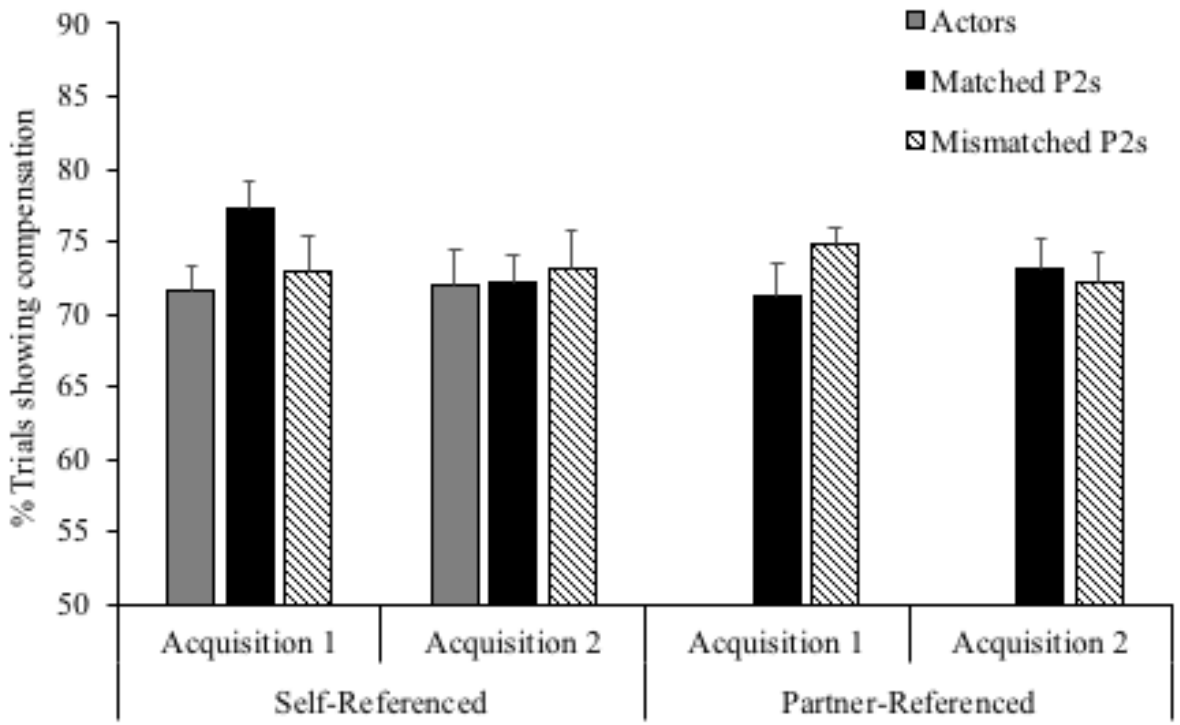
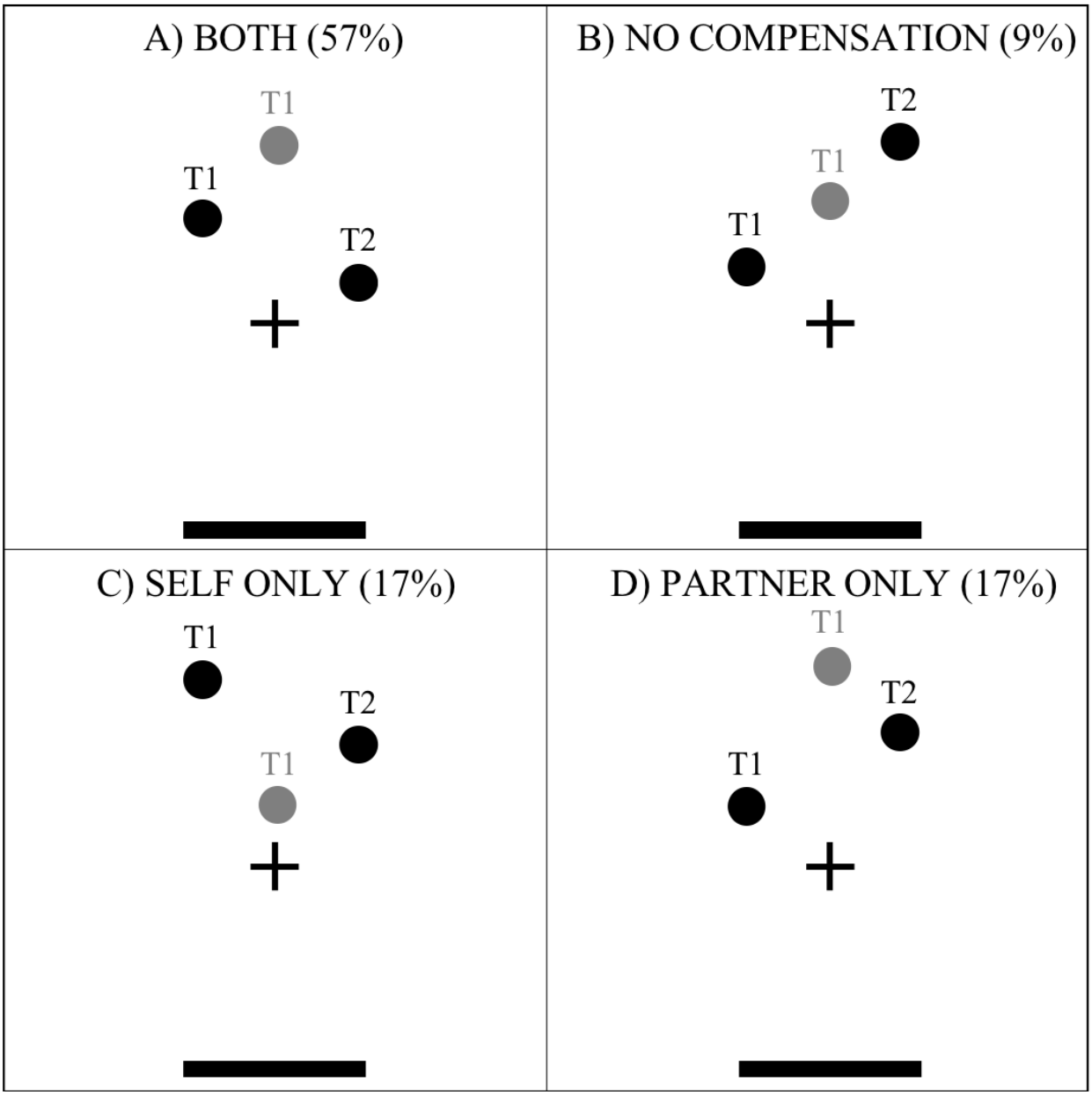


Figure 10. Schematic of potential corrective behaviors to errors in the y-dimension (i.e., overshooting or undershooting the target), along with percentage of acquisition trials showing each type of scenario (collapsed across groups, partners, and days). Examples pertain to the outcome of 'trial 2' ("T2") with reference to the individual's own previous trial (black "T1") or their partner's previous trial (gray "T1"). Outcomes could reflect A) compensation with respect to both their own and their partner's previous error, B) no compensatory behavior, C) compensation with respect to only their own previous error, or D) compensation with respect to only their partner's previous error.



3.3.3 Questionnaires

Mean responses to the Task Evaluation Questionnaire subscales are presented in Table 4. 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: $t(46) = 1.40$, $p = .17$, $d = .39$; competence: $t(46) = –.45$, $p = .66$, $d = .18$; pressure/tension: $t(46) = –.83$, $p = .41$, $d = .19$). There were also no significant differences between practicing in alternation for P2s versus physical practice only (all Fs < 1).

The paired practice experience questionnaire items and results are presented in Table 5. Regardless of whether schedules were matched or mismatched, watching a partner's practice was rated as moderately helpful, $t(46) = 1.36$, $p = .18$, $d = .39$, and not interfering, $t(46) = –1.45$, $p = .16$, $d = .42$, 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, $t(46) = .36$, $p = .72$, $d = .10$.

Table 4. Mean ratings (and *SD*s) for the Task Evaluation Questionnaire subscales.

Subscale	Matched	Mismatched	Actors
Interest/enjoyment	5.0 (1.3)	4.5 (1.3)	4.7 (1.3)
Competence	3.6 (1.2)	3.8 (1.0)	3.7 (1.0)
Pressure/tension	2.3 (1.0)	2.5 (1.1)	2.1 (1.1)

Note. Scales ranged from 1–7.

Table 5. Mean ratings (and *SD*s) for the paired practice experience questionnaire.

Item	Matched	Mismatched
1. Watching my partner helped my own performance/learning	4.3 (1.4)	2.7 (1.6)
2. Watching my partner interfered with my own performance/learning	2.3 (1.5)	3.0 (1.7)
3. I wanted to be more accurate than my partner	5.1 (1.3)	4.9 (1.5)

Note. Scales ranged from 1–7.

3.4 Discussion

Our primary aim was to assess if and how between-person schedules which do and do not promote interference (i.e., mismatched and matched, respectively) influence motor performance and learning, compared to non-turn-taking practice (where learners either physically practice or observe). In the turn-taking dyad groups, because between-person contextual interference (CI) would be higher for the mismatched group, this should aid retention, even though matched practice was expected to benefit immediate performance due to the benefits of observational practice of the to-be-enacted skill before each trial. However, counter to our hypotheses, the matched and mismatched alternating practice conditions yielded essentially the same outcomes, in terms of errors and perceptions of the practice experience. These findings are inconsistent with predictions regarding the benefits of mismatched practice, where physical practice is interspersed with practice-relevant, yet not immediately applicable cognitive activities (e.g., Simon & Bjork, 2002; Wulf & Shea, 2002).

There were a number of factors that differed between the current study and previous research, which likely contributed to the differences in results. For instance, here we used relatively complex golf putting skills, as opposed to relatively simple timing, keystroke tasks (Lee et al., 1997; Simon & Bjork, 2002). Because of the relative difficulty of putting compared to attaining timing goals on a keypad, we also used only two skills rather than the previous use of three different tasks. Of note, we controlled the amount of within-person CI to focus on the effects of between-person interference, but did not test the sensitivity of the golf putting tasks to traditional, within-person CI (although there is some evidence that CI effects arise when practice involves two different tasks (e.g., Simon, 2007) and golf putting skills (e.g., Guadagnoli, Holcomb, & Weber, 1999; Hwang, 2003)). This individual control condition would be important to include in future investigations of dyadic multi-skill learning, to further contextualize the effects (or lack thereof) of between-person CI manipulations. There is also unpublished evidence that more complex skills are not subject to the same between-trial interference effects as seen with these more simple skills (Richardson, 1997, as cited in Guadagnoli & Lee, 2004; see also Wulf & Shea, 2002). It may be that interference-enhancing paradigms (such as mismatched practice) 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; Lee & White, 1990).

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 model 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 paradigm brought too much interference into practice, hindering learning of this task.

Surprisingly, the mismatched group did not perceive their partner's interspersed practice to be more interfering than the matched group, nor did the matched group perceive watching their partner to be more helpful. As above, 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 (discussed further below). One way to test this idea is to pair learners who were each assigned to learn different skills. Whether or not the interspersion of 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. We are currently testing these ideas.

Combining observation and physical practice did not show any benefits for the alternating pairs compared to the Actor control group who only physically practiced the golf putting skills. This goes against some previous dyad learning literature, where alternating turns enhanced the learning of a single skill compared to practice alone (e.g., Granados & Wulf, 2007;

Shea et al., 1999, 2000; *cf.* 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, and we failed to show between-group differences, this suggests that previous benefits of dyad practice on learning may be related to these inter-trial or social facilitation factors. It is also possible that fully blocked schedules (as opposed to the semi-blocked schedules used in the present study) would better differentiate any impacts of matched and mismatched conditions compared to physical practice alone. Mismatched models have been proposed to be especially important for blocked practice, even though these same authors also showed benefits of mismatched practice for random practice schedules (Simon & Bjork, 2002).

Despite the fact that there were no outcome-related differences between groups, which would suggest that between-person (contextual) interference impacts learning, there was evidence that pairs were sharing in the practice experience. Looking at the alternating behaviors of the dyads, there was evidence of compensation, such that one's own versus a partner's overshooting (or undershooting) was equally likely to lead to a shorter (or longer) putt on the next turn. These findings are in line with joint action research showing that co-actors monitor each other's performance and adapt their behaviors in response to a partner's errors and/or task constraints, similarly to as if they were their own. For 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, Mars, Bekkering, & Coles, 2012; de Buijn, Miedl, & Bekkering, 2011) and 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 behaviors 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 behavioral adaptations will be important for better understanding the potential costs and benefits of shared learning conditions.

The control group also allowed us to assess the efficacy of physical versus observational practice for the golf putting skills. As anticipated, the Actors generally outperformed the Observers on the retention tests, consistent with previous findings that physical practice promotes superior retention as compared to observational practice (e.g., Blandin et al., 1994; Karlinsky & Hodges, 2014; Shea et al., 2000; Wright et al., 1997). The results also suggest, however, that observational practice was sufficient towards developing cognitive representations of these skills, particularly considering the similarity between the Actors' and Observers' performance on the final, with-feedback retention test. This is in line with research suggesting the benefits of observation are not fully appreciable until individuals have had the opportunity to receive feedback on their own performance (which the Observers received during the day 2 with-feedback retention test; e.g., Andrieux & Proteau, 2013; Deakin & Proteau, 2000). Of future interest is whether the benefits of observation within turn-taking conditions would be enhanced by more actively engaging learners during periods of observation. 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).

In summary, we studied multi-skill turn-taking practice, where partners practiced the same or different skills in alternation. Turn-taking practice did not enhance (or impede) learning compared to pure physical practice, nor did matched versus mismatched practice schedules differentially impact skill acquisition, indicating that principles of contextual interference (CI) do not necessarily extend to dyad learning conditions at the between-person level. 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), 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.

3.5 Bridging summary

In Experiment 2, we manipulated the degree of between-person contextual interference (CI) in practice via a partner's interspersed practice trials, which either matched or mismatched the peer's subsequent task. This manipulation did not modulate acquisition of the golf putting skills and there was no evidence that dyad, alternating practice was any better to repetitive, pure physical practice. However, we did have evidence that partners were adapting their behaviours based on their partner's, suggesting that giving individuals more choice over how to practice could potentially alert as to how learners are influenced by their partners in these dyad learning

conditions. There is substantial interest in how learners choose to organize their own practice, with research showing benefits associated with having control over practice decisions. These benefits are thought, at least in part, to be related to optimizing of decisions in relation to one's own current performance levels. However, there is reason to think that these decisions might also be influenced by how a partner schedules their practice. Moreover, there is speculation that having control over one's own practice, regardless of the choices made, also aids later learning (potentially through enhanced motivation related to higher perceived autonomy).

The purpose of these last three experiments was to further study dyad practice, under conditions where individuals have some choice about how to practice (Experiments 4 and 5), or when their partner is in charge of the practice decisions, what we term "peer-directed" practice (Experiment 3). In these studies we chose to use simpler keystroke sequence tasks, as these have been shown to be sensitive to the CI effect.

Chapter 4: Experiment 3

Evaluating the effectiveness of peer-scheduled practice on motor learning

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4.1 Introduction

In real-world situations, it is not uncommon for people to practice motor skills in pairs, with a similarly skilled partner. From a motor learning perspective, these peer-based practice conditions have received little empirical attention despite their relative popularity and potential informativeness as to the processes that best promote learning. In this study, we evaluated how partners schedule practice for their peers, the effectiveness of this scheduling, and the subjective response to these situations. We evaluated these questions against the backdrop of work concerning self-determined practice and questions as to whether the cognitive effort associated with choosing how to practice is a necessary component of self-directed practice benefits.

The limited research on peer-based (or dyad) motor learning has generally been concerned with whether paired practice conditions can improve the efficiency and effectiveness of learning compared with individual practice. Two forms of peer-based training protocols have been studied. In one form, partners share task demands. Following part-task practice with a partner on a video game, positive transfer to the whole task was subsequently shown (Shebilske, Regian, Arthur, & Jordan, 1992). In a second form, partners take turns (physically performing, then observing). This type of alternating schedule on a balance task resulted in better retention, when tested alone, compared with individual practice and blocked trials of physical practice followed by observation (Shea, Wulf, & Whitacre, 1999; see also Shea, Wright, Wulf, & Whitacre, 2000). A variety of mechanisms have been proposed to explain the potential beneficial effects of learning in pairs. In addition to observational benefits (e.g., Granados & Wulf, 2007), factors related to social facilitation (audience effects), competition, and positive self–other comparisons are thought to impact the motivation and effort devoted to learning (Shea et al., 1999).

Cognitive effort is thought to play an important role in motor learning, and many practice variables have been manipulated to provide a "cognitively effortful" practice context. Such contexts are designed to promote learning by challenging the information-processing capabilities of the learner (e.g., Guadagnoli & Lee, 2004; Lee, Swinnen, & Serrien, 1994). One approach to promoting effortful practice relates to the order in which learners practice motor skills. Certain practice schedules impose greater challenge on information-processing resources and enhance learning more than others. Schedules that promote "contextual interference" (CI) between practice attempts (i.e., a more random type of practice, in which skills are practiced in alternation) are generally better for learning than low-CI schedules (i.e., a more blocked type of practice, in which skills are practiced in repetition; for a review, see Lee, 2012). An exception to this effect has been noted when learners have been given control over their practice (for a review of self-directed practice, see Sanli, Patterson, Bray, & Lee, 2013).

Allowing learners to decide when to switch tasks has been shown to enhance learning, compared with experimenter-imposed schedules, even when the chosen schedules are relatively low in CI (e.g., Hodges, Edwards, Luttin, & Bowcock, 2011; Keetch & Lee, 2007; Wu & Magill, 2011). What these studies generally show is that some process or processes associated with choosing how to practice can not only compensate for potential disadvantages in retention that might be associated with low-CI practice but can also enhance learning relative to experimenter-imposed schedules.

Two major features distinguish self-scheduled from experimenter-scheduled practice, which could be driving the associated benefits. Requiring learners to make decisions about their practice adds a cognitive component not present in more prescriptive practice regimes (Sanli & Lee, 2013; Wu & Magill, 2011). The second feature is the individualized nature of self-directed

practice, whereby practice is adaptive and contingent upon a person's current performance (e.g., Chiviacowsky & Wulf, 2002). Illustrative examples of such performance-contingent practice have been provided by Keetch and Lee (2007) and Wu and Magill (2011). Self-scheduled groups made switching decisions based on their trial-to-trial performance, switching to a new task following relatively better performance irrespective of the amount of CI. These self-scheduled groups then performed as well or better than a high-CI or matched-schedule group in retention.

The two above-mentioned studies provide evidence that when learners are permitted to control their own task-switching schedules, they tailor their practice to their performance. However, in both of these studies, the performance-contingent nature of practice was not distinct from the more generally cognitively effortful practice promoted by the decision demands of self-scheduled practice. Thus, the present experiment was designed to dissociate the features of self-scheduled practice that might engender improved retention. We adopted a novel peer-scheduling paradigm whereby task switching was either directed or not directed by a peer. We controlled for the social factors inherent in paired-learning paradigms by testing all participants in pairs. In self- and peer-scheduled practice, we expected that the order in which the different tasks were practiced would be performance dependent. However, only actors in the self-scheduled group would make decisions about when to switch between tasks.

Thus, two groups of participants were tested in pairs during practice of 3 five-keystroke sequences and individually tested for retention the next day. During practice, one person was the Actor, who physically practiced. The Partner's role varied depending on group. In the self-scheduled group, the Partner passively observed while the Actor self-directed practice. In the peer-scheduled group, the Partner directed the Actor's practice. We anticipated that both self- and peer-scheduled Actors would experience performance-contingent practice (switching more

following relatively good, low-error trials) but that those in the latter group would do so in the absence of decision-making demands associated with when to switch between tasks. If this additional decision component associated with self-directed practice is important for learning, the self-scheduled Actors would be expected to do better in retention than the peer-scheduled Actors. There is suggestive evidence that having choice about how to practice positively impacts factors related to motivation to learn (for a review, see Lewthwaite & Wulf, 2012). As such, we expected that self-scheduled Actors would rate their practice experience more positively (with respect to enjoyment and motivation) than peer-scheduled Actors.

4.2 Methods

4.2.1 Participants

Forty-eight women (18–35 years of age) participated and were paired with a randomly assigned partner to be either the Actor or the Partner. Actors physically practiced while the responsibilities of the Partner varied depending on group. In the *self-scheduled* group, the Actor self-directed practice and the Partner passively observed. In the *peer-scheduled* group, the Partner directed the Actor's practice schedule. All participants self-reported as right-handed, provided informed consent, were naïve to the specific goals of the study, and were paid $10 per hour.

4.2.2 Task and Apparatus

The experimental task was adapted from Simon and Bjork (2001) and involved learning to execute three different five-keystroke sequences on a nine-digit computer keypad (Dell SK-8115), using only the right index finger. Each sequence consisted of a particular set of keys and a distinct overall movement time (MT) goal (1,000 ms, 1,300 ms, or 1,600 ms; see Hodges, Lohse, Wilson, Lim, & Mulligan, 2014, for sequence details). The goal MTs, their corresponding

sequence, and the feedback screens (containing information on keystroke and MT accuracy) were displayed on a 38.1 × 21.6–cm laptop computer screen (Dell XPS L702X) during the acquisition and retention phases via a customized E-Prime 2.0 program (Psychology Software Tools, Inc., Sharpsburg, PA).

4.2.3 Procedures

4.2.3.1 Acquisition

The Day 1 acquisition phase was completed in pairs. There was a brief period during which the experimenter initiated some exchange between the pair, and then the Actors were told they would physically practice keystroke sequences during acquisition while the Partner either passively observed (self-scheduled group) or scheduled the Actor's practice (peer-scheduled group). The Partner always sat adjacent to the Actor so that they could see the keypad and computer screen and easily communicate the sequence number. Participants were informed that the overall goal was for the Actor to do well on the Day 2 retention test. A prize of $10 was offered to each member of the pair whose Actor achieved the lowest error on this test within each experimental group. Partners were, however, instructed to watch the Actors' practice carefully as they would also be tested the following day.

In a familiarization phase, the Actor completed 5 successful trials of a sequence, which had a different goal MT to those of the experimental trials. Throughout the experiment, a successful trial required only that the correct keys be pressed in the correct order.

Each trial began with the presentation of a "home screen", displaying the three sequences. The sequence to be practiced would then be manually selected by the Actor (with no time constraints). In the self-scheduled group, the Actor would decide which sequence to practice and say the sequence's number aloud. Her Partner would then repeat this number, which

the Actor would then input on the keypad. In the peer-scheduled group, the Partner would determine which sequence the Actor should practice and say the number aloud; the Actor would then repeat and input this number to proceed. The participants did not otherwise communicate during practice.

Once the sequence number had been inputted, the corresponding goal MT would be displayed for 4 s. This screen was omitted following the first 3 successful trials of each sequence, yet images of the sequences and goal MTs were posted above the computer if reminders were needed. The selected sequence was then visually presented, and after 2 s a beep sounded, indicating that the Actor should enter the sequence (matching the goal MT). An "S" marked the start key of the sequence, with the order in which to press the keys identified by a black adjoining line. After five keys had been depressed, the sequence remained for a further 2 s before feedback was displayed. Participants were told (a) whether the keys were pressed in the "correct" or "incorrect" order, (b) MT in milliseconds, and (c) signed MT error in milliseconds. This information was shown for 5 s before the home screen appeared, marking the start of the next trial.

Actors were required to complete exactly 30 "correct" trials of each of the three sequences. Participants were not made aware of how many trials remained for each sequence. Thus, if a sequence was chosen that had already reached criterion, the participant was prompted to choose another sequence. If a trial was "incorrect" (i.e., wrong key or key pressed before the beep), the trial was recycled and hence available to be chosen again.

4.2.3.2 Retention

All participants individually completed an 18-trial, random-order retention test on Day 2 (6 trials per sequence). The home screen and goal MTs were not presented during this test. As in

practice, the image of the sequence remained on screen during the test trials, eliminating the need for explicit memory of the sequence. Only feedback about whether sequence execution had been "correct" or "incorrect" was displayed. If the sequence was incorrectly performed, the trial was recycled and repeated at the end.

Following retention, participants were asked two questions to ascertain their subjective experiences associated with the different types of practice. Ratings were made on 7-point Likert-type scales (1 = *not at all*, 7 = *very*). Actors were asked, "How motivated were you to learn these 3 sequences?" and "How much did you enjoy practicing these tasks yesterday?". Partners were asked, "How motivated were you for your partner to learn these 3 sequences?" and "How much did you enjoy watching/scheduling practice for these tasks yesterday?".

4.2.4 Analysis

4.2.4.1 Percentage error

MT was assessed from the start of the first keystroke to the fifth keystroke. Absolute error (AE) in MT was calculated for each sequence and converted to percent MT error based on the associated MT goal, to enable comparisons across the sequences' different MT goals.

Error data for acquisition and retention were analyzed separately and grouped into 9-trial blocks. The Actors' acquisition data were submitted to a two-way mixed analysis of variance (ANOVA) of group (self-scheduled, peer-scheduled) and block (1–10). Retention data were submitted to a three-way mixed ANOVA, with group and role (Actor, Partner) as between factors and block (1–2) as a within factor.

4.2.4.2 Switching characteristics

If task switching was based on performance, errors should be lower on switch trials (trial t) than on the preceding trial (trial t – 1). We identified trials in which the same sequence was

practiced in both “switch” and preceding “nonswitch” trials (Keetch & Lee, 2007). The Actors’ AE was used as the best estimate of the information that would have been used to assess performance. These data were submitted to a two-way mixed ANOVA of group and trial type (t, “switch”; t – 1, “nonswitch”). We also analyzed the frequency of switching between sequences. The number of switches per 9-trial block was analyzed using a two-way mixed ANOVA of group and block (1–10).

4.2.4.3 Debrief questions

Answers to the motivation and enjoyment questions were submitted to an ANOVA with group and role as factors.

For all analyses, Greenhouse-Geisser corrections were applied to the degrees of freedom for violations to sphericity. Significant effects and interactions were further examined using Tukey’s honestly significant difference procedure (all *p*s < .05 reported). Partial eta squared values (η_p^2) are reported as measures of effect size, and power values (1 – ß) are given for non–statistically significant effects where $F > 1$.

4.3 Results

4.3.1 Evidence for Performance-Contingent Switching

4.3.1.1 Switching strategies

There was a significant effect of trial type, $F(1, 22) = 17.09$, $p < .001$, $\eta_p^2 = .44$, such that the Actors’ AE was significantly lower on switch trials ($M = 75.59$ ms, $SD = 35.79$) than on nonswitch trials ($M = 116.85$ ms, $SD = 53.83$). As there were no other effects or interactions (*F*s < 1), we infer that both groups organized practice in a performance-contingent manner.

4.3.1.2 Switching frequency

The peer schedulers directed more switches between sequences per block (M = 3.94 switches, SD = 2.80) than the self-scheduled Actors chose for themselves (M = 2.18 switches, SD = 2.23), $F(1, 22) = 4.84$, $p = .039$, $\eta_p^2 = .18$. Sequence switching also covaried as a function of block, $F(4.92, 108.31) = 2.58$, $p = .031$, $\eta_p^2 = .11$, generally increasing until the middle of practice. However, there were significant differences only between Block 1 (M = 2.00 switches, SD = 2.13) and Block 6 (M = 3.88 switches, SD = 3.26) and between Block 1 and Block 9 (M = 3.67 switches, SD = 2.94). There was no interaction ($F < 1$).

Because of these unexpected differences in CI between the Actor groups, we performed additional analyses to determine whether switching frequency was related to retention error. We classified the Actors as high or low switchers relative to their within-group median number of total switch trials and reanalyzed the error data with this additional factor (see also Hodges et al., 2014). Although high switchers were more error prone during practice (M = 9.02%, SD = 3.54) than low switchers (M = 7.01%, SD = 3.42), $F(1, 20) = 5.11$, $p = .035$, $\eta_p^2 = .20$, there was no effect of switcher type in retention ($F < 1$). Moreover, there was no correlation between overall switch amount and retention when considered across all participants, $r(22) = -.02$, or within groups, self-scheduled, $r(10) = .05$; peer-scheduled, $r(10) = -.08$.

4.3.2 Peer-Scheduled Actors Acquired and Retained the Tasks as Well as Self-Scheduled Actors

4.3.2.1 Acquisition

Both groups of Actors improved across practice as shown in Figure 11, $F(4.86, 106.90) = 11.79$, $p < .001$, $\eta_p^2 = .35$. This was confirmed by a linear trend component to the block effect ($p < .001$). There were no group-related effects or interactions (Fs < 1).

4.3.2.2 Retention

The Actors performed with significantly less error (M = 14.33%, SD = 12.58) than their Partners in retention (M = 22.67%, SD = 20.26), $F(1, 44) = 6.42$, $p = .015$, $\eta_p^2 = .13$. However, there were no group-related effects (Fs < 1).

4.3.3 Peer-Scheduled Practice Was Perceived More Positively Than Self-Scheduled Practice

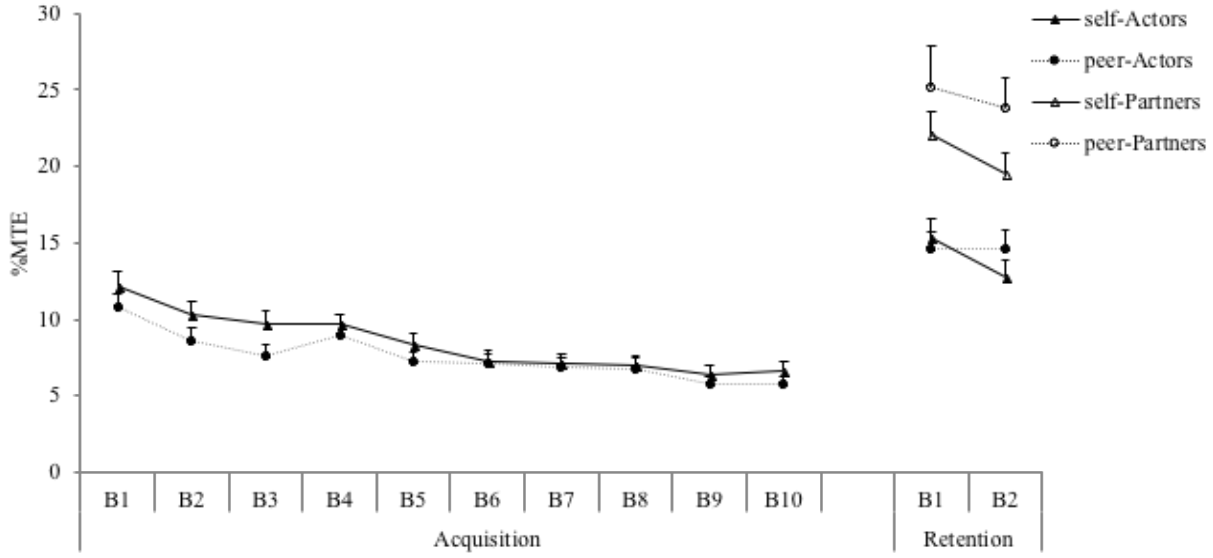
4.3.3.1 Motivation

Both Actors and their Partners in the peer-scheduled group were more motivated for the Actor to learn the sequences (M = 6.06, SD = 1.20) than in the self-scheduled group pairs (M = 5.19, SD = 1.09), $F(1, 44) = 6.71$, $p = .013$, $\eta_p^2 = .13$. There was no effect of role or Group × Role interaction (Fs < 1).

4.3.3.2 Enjoyment

The peer-scheduled group pairs (M = 5.08, SD = 1.01) also rated practice as more enjoyable than the self-scheduled group pairs (M = 4.08, SD = 1.59), $F(1, 44) = 8.32$, $p = .006$, $\eta_p^2 = .16$. The Actors enjoyed their role (M = 5.13, SD = 1.08) more than their Partners (M = 4.04, SD = 1.52), $F(1, 44) = 9.76$, $p = .003$, $\eta_p^2 = .18$, but there was no Group × Role interaction, $F(1, 44) = 2.83$, $p = .10$, $1 - ß = 0.38$.

Figure 11. Average percentage movement time error (and *SE* bars) as a function of 9-trial blocks for the acquisition and retention phases as a function of group (self-scheduled and peer-scheduled) and role (Actor and Partner).



4.4 Discussion

We evaluated practice organization in the context of paired practice, providing a first look at how partners schedule practice for their peers and the effectiveness of this peer scheduling in comparison with self-scheduled practice. In terms of overall error in acquisition and retention, self- and peer-scheduled Actors did not differ despite the fact that only the self-scheduled Actors made decisions about when to switch between tasks across trials. Both groups adopted practice schedules that were performance dependent, with switching following low error trials, but the peer schedulers chose practice schedules that were higher in CI (i.e., more switching) than those adopted by self-schedulers. Actors and Partners in the peer-scheduled group rated practice as more enjoyable and more motivating than the self-scheduled group pairs.

We assumed that peer schedulers would provide performance-contingent practice, given that the only cue for switching between tasks would be their partner's error, and for both Actors

and peer schedulers, their role was to maximize learning for the Actor. This was confirmed by the data. Absolute-timing errors were, on average, lower on switch trials than on the preceding trial for both groups. This shows that peers direct a partner's practice commensurate with how learners choose to organize their own practice (e.g., Hodges et al., 2014; Keetch & Lee, 2007; Wu & Magill, 2011).

In view of the fact that both groups of Actors practiced under conditions that supported performance-contingent task switching, we are now able to consider whether the additional requirement to choose when to switch across trials positively impacted motor learning. There were no significant differences between the self- and peer-scheduled Actors' MT error scores during practice or in retention. While the opportunity to self-control task switching has benefited learning (e.g., Hodges et al., 2011; Keetch & Lee, 2007; Wu & Magill, 2011), our results suggest that it is not the decision making per se that is responsible for self-directed practice benefits, but rather it is more likely to be the dynamic, adaptive nature of performance-contingent practice. The opportunity to receive individualized practice (be it self-, peer-, or algorithmically-determined) is proposed to benefit learning by systematically incorporating interference into acquisition while preserving the learners' opportunity to experience success (Simon, Lee, & Cullen, 2008).

We did find, however, that the self-scheduled Actors switched between sequences significantly less often than the peer-scheduled Actors. This unexpected difference between the groups might be viewed as a potential confound. However, the amount of CI was low in both groups (~one third of what would be experienced in a random condition), and importantly, it was not related to retention performance, neither when we compared across high and low switchers, nor when we looked individually via correlational analyses. This lack of a relationship between

switching frequency and retention has also been characteristic of other self-directed practice research (Keetch & Lee, 2007; Wu & Magill, 2011). This suggests that the quantity of switching (and hence CI) is relatively less important in comparison with the performance-contingent nature of practice (see also Simon et al., 2008). As to why there were differences between groups, it is possible that the self-scheduling Actors were also basing their judgments of performance error on intrinsically sensed error feedback, which was not available to the Partners, leading to higher standards for judgments of a "good" trial and hence less switching. It is also possible that self-scheduled Actors, who were also charged with executing correctly timed actions, opted to make their practice "easier" than peer schedulers, who only had to make decisions about what to practice. This potentially low frequency of switching for self-scheduled Actors is also supported by data from a previous study (Hodges et al., 2014), where self-scheduled actors switched, on average, 2.19 times per block (current study, $M = 2.18$ switches per block), with both being less than the peer schedulers ($M = 3.94$ switches per block).

In our groups the pairs were always the same gender, but we did not encourage any relationship to develop between the partners (beyond a brief introduction). Efforts to encourage interaction could have facilitated learning among the observer Partners, particularly considering the growing body of joint-action literature suggesting that the social relation between partners modulates motor performance effects (Eskenazi, van der Wel, & Sebanz, 2012). It is worth noting that general levels of enjoyment and motivation to learn (or for their partner to learn) were relatively high in both groups, despite a lack of interaction. However, the peer-scheduled group generally rated their practice as more motivating and enjoyable than the self-scheduled group, which might mean that the social aspect of merely sharing a task together is relatively more

important for creating a positive learning experience than control over one's practice and potentially enhanced feelings of autonomy (see Lewthwaite & Wulf, 2012).

In summary, we sought to disentangle two major features of self-directed practice schedules through the use of a dyad-learning paradigm. In doing so, we also contribute novel insight into the practice-scheduling behaviors of peers, the effectiveness of these schedules toward learning, and learners' subjective perceptions of such practice conditions. Peer-directed practice was similar to self-directed practice with respect to its performance-contingent nature, and it was equally effective in terms of learning outcomes. The lack of difference between the two groups leads to the suggestion that the enhanced learning previously associated with self-directed task switching is more contingent upon the performance-dependent nature of the schedule than the cognitive effort related to decision making per se. We do provide a caveat to this conclusion, based on the fact that the peer schedulers chose for their Actor partners to switch between tasks more frequently than Actors in control of their own practice and hence the amount of CI could have compensated for any decrease in effort associated with a release from decision making. However, the finding that the amount of CI, either across groups or within groups, showed no relationship to retention performance, probably because in both cases it was still relatively low, does speak against this alternative explanation for these data, adding more support for an adaptive, performance-dependent explanation. Clearly further work is required to draw stronger conclusions about these effects, comparing across experimenter-determined practice conditions and potentially constraining the amount of switching.

4.5 Bridging summary

In Experiments 2 and 3, we studied how a partner can bring contextual interference into a peer's practice via their own interspersed practice schedule or through their role as a peer-

scheduler, respectively. The findings from Experiment 3 were congruent with previous research showing that self-directed learners tend to switch tasks following relatively good (low error) performance. Moreover, the results showed that this performance-contingent strategy extends to novice observers charged with scheduling a peer's practice. Importantly, Experiment 3 also provided an index of the practice organization strategies self-directed learners tend to adopt when they are the only learner physically practicing. The purpose of Experiment 4 (and 5) was to assess whether and how exposure to a peer's practice potentially modulates self-directed practice choices, compared to those typically observed when practice is undertaken alone. We used an alternating practice paradigm to study how a co-learner's interspersed blocked, random, or self-directed practice schedule influences a partner's self-directed practice choices and the implications for longer-term retention.

Chapter 5: Experiment 4

Dyad practice impacts self-directed practice behaviors and motor learning outcomes in a contextual interference paradigm

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5.1 Introduction

The challenge of how to organize practice schedules to promote learning has captured the interest of researchers and practitioners alike for over 3 decades. However, these attentions have been largely devoted to learners in an isolated practice environment. Despite the ubiquity of social motor learning settings, little empirical attention has been paid to how to effectively organize multi-person practice, and the potential benefits and costs of multi-skill practice within dyad settings. This study provides a first look at the processes, outcomes, and affective perceptions associated with peer-based, multi-skill learning.

5.1.1 Dyadic Turn-taking Practice

Though the research is limited, turn-taking, dyadic practice can be an effective and efficient means of promoting learning (e.g., Granados & Wulf, 2007; Shea, Wright, Wulf, & Whitacre, 2000; Shea, Wulf, & Whitacre, 1999). This form of practice, where partners alternate between physical practice and observation, capitalizes on the inter-trial breaks that are generally required between attempts at a (complex) skill (e.g., Wulf & Shea, 2002). Rather than unfilled rest, learners can observe a co-learner. This is proposed to promote additional information processing activities which impact on subsequent practice and retention (e.g., Lee, Swinnen, & Serrien, 1994; Simon & Bjork, 2002; Wulf & Shea, 2002).

While the benefits of turn-taking have been attributed in part to the opportunity for observational learning (e.g., Granados & Wulf, 2007), this form of paired training seems to be more than simply the opportunity for both performance and observation. It is the interactive, dynamic nature of the alternating physical and observational practice that appears important to the success of paired practice. As an illustrative example, blocked physical practice on a balance task in the first half of practice, while a partner observed, before switching actor and observer

roles for the second half of practice, was not as good as turn-taking practice (Shea et al., 1999). Moreover, only turn-taking practice was better than individual practice (Shea et al., 1999). Despite this finding, research on turn-taking practice to date has been limited and constrained to the learning of a single skill (e.g., stabiliometer task, Shea et al., 1999; computer-based task, Shea et al., 2000; cup-stacking, Granados & Wulf, 2007). Because of these single-skill paradigms, it has been difficult to infer when and what aspects of a partner's practice impacts one's own, such as the choice to switch skills, which is only present in a multi-skill learning context. Here we aim to study how paired practice influences the effectiveness of multi-skill motor learning, when practice organization (i.e., task-switching schedule) is experimenter-determined for one partner only and the other partner gets to choose how to practice.

5.1.2 Multi-skill Practice Organization

There is considerable evidence that certain practice schedules enhance learning more than others, particularly in terms of the "contextual interference" (CI) effect. This effect refers to the finding that while blocked practice schedules (in which all trials of a particular skill are practiced one after another before switching to a different task, reducing "interference") are associated with better performance during practice, random schedules (in which skills are practiced unsystematically, promoting "interference") result in better retention and transfer (for a review, see Lee, 2012).

While evidence of CI effects within observational practice is mixed (e.g., Blandin, Proteau, & Alain, 1994; Lee & White, 1990), interspersing a learner's physical practice with demonstrations (that either augment or diminish the degree of CI within the practice schedule) significantly affects both immediate motor performance and learning. For example, adding blocked "demonstrations" of a keystroke-sequence timing task (i.e., a computerized run-through

of the upcoming sequence, such that each key was highlighted in the order and timing it should be pressed), to a random practice schedule, reversed the typical advantages of random practice in retention (Lee, Wishart, Cunningham, & Carnahan, 1997). This random + blocked demonstration group displayed the best performance in acquisition, compared with a typical random practice only group, showing that the demonstrations were effective in reducing immediate timing error. These results were partially replicated when the interspersed computerized demonstrations matched (vs. mismatched) the next trial of blocked and random practice schedules (Simon & Bjork, 2002). Mismatched demonstrations resulted in more errors in acquisition but more accurate retention than matched demonstrations. Although random groups continued to outperform blocked groups in retention, matching demonstrations impaired retention for both schedules. These findings are consistent with a cognitive effort framework of learning, which supports the implementation of practice conditions that impose greater demands on the learner's information processing activities to promote learning (in this case, random and mismatched practice; see Guadagnoli & Lee, 2004; Lee et al., 1994).

There is accumulating evidence showing that allowing learners to make decisions regarding their practice enhances learning compared with experimenter-determined or yoked practice conditions, wherein the latter, practice is matched to another group's schedule (for a review, see Sanli, Patterson, Bray, & Lee, 2013). Of particular relevance to the current study, giving learners control over when to switch tasks during motor skill acquisition has been shown to enhance learning compared with experimenter-imposed (including yoked) schedules, even when the amount of CI adopted is relatively low (e.g., Hodges, Edwards, Luttin, & Bowcock, 2011; Keetch & Lee, 2007). Making practice-related choices on a trial-by-trial basis enables learners to customize their practice to suit their needs (e.g., Carter, Carlsen, & Ste-Marie, 2014;

Chiviacowsky & Wulf, 2005). Self-directed learners appear to make decisions to change tasks in a performance-contingent manner, switching following better (e.g., faster, more accurate) performance (e.g., Hodges, Lohse, Wilson, Lim, & Mulligan, 2014; Keetch & Lee, 2007; Wu & Magill, 2011; see also Karlinsky & Hodges, 2014 for evidence of peer-directed performance-contingent practice).

Self-directing learners have also been shown to be influenced by their previous practice experience when making task-switching decisions (Hodges et al., 2011, 2014). For example, when participants self-directed practice following blocked or random practice of key-sequence tasks, those who were initially assigned to a random practice condition later chose to switch between tasks more frequently than those who had experienced blocked practice (Hodges et al., 2014). In the current study, we evaluate whether the practice organization self-directed learners experience vicariously (via observation of a partner's schedule) modulates the task-switching strategies they choose to adopt, compared with those typically seen when practicing alone. We were interested in whether performance-contingent practice is demonstrated less, or not seen, when partners of a self-scheduled learner adopt a high or low frequency switching schedule.

5.1.3 Study Aims

We adopted a novel dyad-practice paradigm, whereby multi-skill task-switching was either experimenter- or self-directed, to determine if and how a partner's practice schedule influences practice behaviors and learning outcomes. Three groups were tested in pairs during practice of 3 keypress timing tasks and individually tested for immediate and delayed retention. Partner 1s (P1s) either had a blocked, random, or self-directed schedule. All Partner 2s (P2s) self-directed their practice. Self-directed P1s did not observe their partner's practice, for control purposes. We anticipated that for these individuals, the order in which the different tasks were

practiced would be performance-dependent (switching more following relatively good trials, e.g., Hodges et al., 2014; Karlinsky & Hodges, 2014; Keetch & Lee, 2007; Wu & Magill, 2011). Self-directed learners' decision-making in other conditions was expected to be impacted by the partner, with switching decisions less or no longer reflective of their own trial-to-trial performance. We also predicted that in general, exposure to a partner's random practice would promote more switching than that seen by people with blocked practice partners (Hodges et al., 2014). It was not clear whether this would benefit overall learning, due to the finding that self-directed learning outcomes do not always correlate with switching frequency (e.g., Karlinsky & Hodges, 2014).

Considering recent suggestions that self-directed and dyad practice comprise motivational benefits (e.g., Lewthwaite & Wulf, 2012), participants responded to the Intrinsic Motivation Inventory (IMI; Ryan, 1982) to provide insight into the affective experiences associated with the different forms of practice. We hypothesized that self-directed learners would rate their practice experience more positively than participants following experimenter-determined schedules as a result of the autonomy associated with choosing how to practice.

5.2 Methods

5.2.1 Participants and Groups

Ninety-four right-handed females (*M* = 22.1 yr, *SD* = 3.9 yr) volunteered to participate individually and were paid $10.50/hr. Participants had normal or corrected-to-normal vision, were naïve to the specific goals of the study, and provided informed consent. Individuals were randomly paired, and within each pair partners were randomly assigned to be Partner 1 (P1) or Partner 2 (P2). Pairs were pseudo-randomly assigned to the blocked-self (*n* = 17 pairs), random-self (*n* = 15 pairs), or self-self (*n* = 15 pairs) groups, where 'self' refers to self-directed practice.

The paired group names reflect the practice (task-switching) schedule assigned to P1 followed by the practice schedule assigned to P2. Therefore, the 3 groups of pairs comprised 6 subgroups based on group and partner assignment (see Table 6 for list of groups and partner labels). Thus, the P1s in the blocked-self and random-self groups followed predetermined schedules, while all other participants self-directed their practice. The self-directed P1s could not watch or hear their partner's practice, to provide an index of self-directed practice, unmodulated by an observed partner's practice.

Table 6. A summary of the three pair groups and their subgroup labels for each partner (Partner 1 and 2).

Group name	*n* (pairs)	Partner 1 (P1)		Partner 2 (P2)	
		Order	Label	Order	Label
Blocked-Self	17	Blocked	Blocked-Self P1	Self	Blocked-Self P2
Random-Self	15	Random	Random-Self P1	Self	Random-Self P2
Self-Self	15	Self[1]	Self-Self P1	Self	Self-Self P2

Note. 'Order' refers to the practice (task-switching) schedule followed during the acquisition session. 'Self' means that practice was self-directed.
[1] The self-self P1s did not observe their partner's practice for control purposes.

5.2.2 Task and Apparatus

The experimental task was based on Lee et al. (1997) and Simon and Bjork (2001) and involved learning to execute 3 different, 5-keystroke sequences on a 9-digit computer keypad (Dell SK-8115), using only the right index finger. Each sequence consisted of a unique set of keys and a distinct overall movement time (MT) goal (900, 1200, or 1500 ms). An LG computer was used to control stimulus presentation and record participants' responses via customized E-

Prime 2.0 software (Psychology Software Tools, Inc., Sharpsburg, PA). Stimuli were presented on a monitor (ASUS HDMI 23 in.) set in the middle of a desk. During paired acquisition sessions, 2 chairs were placed side-by-side facing the monitor, with the keyboard fixed centrally on the desk. During individual testing, there was only one centrally located chair.

5.2.3 Procedures

The experiment was conducted over 2 consecutive days, with Day 1 completed in pairs and Day 2 completed alone. Day 1 began with a brief period during which the experimenter initiated some exchange with the participants (who did not know one another), before explaining the task. During task familiarization, participants were required to complete 5 successful trials of the same 5-keystroke sequence, which had a different pattern and goal MT to those of the experimental trials. Timing feedback was provided. Throughout the experiment, a successful trial required only that the correct keys be pressed in the correct order (regardless of MT accuracy). Participants then completed a 9-trial, random-order pretest on the 3, 5-keystroke sequences used throughout the remaining phases of the experiment (3 trials/sequence). The relevant goal MT was presented before each trial and the image of the sequence remained on screen during the trial, eliminating the need for memory of the sequence. Only feedback about whether sequence execution had been “correct” or “incorrect” was provided. If the sequence was incorrectly inputted, the trial was repeated at the end of the test. While one partner completed the familiarization and pretest, the other waited outside and responded to the Edinburgh Handedness Inventory (Oldfield, 1971). The order in which partners (i.e., P1 vs. P2) performed these phases was counterbalanced.

During acquisition, partners were seated next to each other with the monitor and keyboard centered between them so that they could each easily see both at all times. They were

instructed to watch one another's practice (except for the self-directed P1s who did not observe their partner's practice) but were asked not to communicate. They were told that the goal of the practice session was to learn to input the keystroke sequences in the goal MT, with the overall goal to do well (i.e., low error) when tested without MT feedback.

All participants were required to perform each sequence correctly exactly 24 times, such that the acquisition session consisted of 144 correct trials (72/partner). The blocked and random P1s followed experimenter-imposed practice schedules, where the blocked sequence order was counterbalanced within the group and the same sequence could not be repeated more than twice consecutively in the random schedule. All other participants decided for themselves which sequence to practice on a trial-to-trial basis. Partners practiced in a turn-taking fashion, starting with P1, and each partner completed a block of 9 trials before switching turns. We had chosen a block of 9 trials, rather than turn taking every trial in order that the P2 would be exposed to a particular schedule of practice before choosing their own schedule. The use of 9-trial blocks also maintained congruency with the other phases of the experiment (as each test comprised 9 trials) and afforded a consistent 3 trials/sequence in each random-order acquisition and test block. The self-directed control group (self-self P1s) wore earplugs, headphones playing white noise, and a blindfold during their partner's blocks. All pairs took a short break halfway through the acquisition session.

During self-directed blocks, each trial began with a "home screen", depicting the 3 sequences. The desired sequence was then manually selected (no time constraints). Participants were not made aware of how many trials remained for each sequence. If a sequence was chosen that had already reached criterion, the participant was prompted to choose again.

Once the sequence to be performed was identified, the goal MT was displayed for 3 s. This screen was only shown for the first 3 successful trials of each sequence (for each partner), but images of the sequences and goal MTs were posted above the monitor for participants to refer to in case they wanted a reminder. The sequence was then presented on screen and after 1 s a beep sounded, indicating the participant should enter the sequence whenever ready. An "S" marked the start key of the sequence, with the order in which to press the remaining keys identified by black adjoining lines. After 5 keys had been pressed, the sequence remained on screen for a further 3 s before feedback was displayed. This delay was expected to provide ample time for learners to engage in information processing activities surrounding error detection and estimation before receiving MT feedback, and hence not show feedback dependence when tested without knowledge of results (KR).

The feedback screen informed participants, (a) whether the keys were pressed in the "correct" or "incorrect" order, (b) total MT in ms (assessed from the start of the first keystroke to the fifth keystroke), and (c) signed MT error (constant error, CE) in ms. This information was shown for 4 s. After an inter-trial interval of 3 s, either the home screen or the next goal MT/sequence appeared. If a trial was "incorrect", the trial was recycled and was either, (a) repeated at the end of the relevant sequence's set of trials (blocked schedule), (b) repeated at the end of the acquisition trials (random schedule), or (c) available to be chosen again (self-directed schedule).

To encourage participants to attend to their partner's practice, MT feedback was temporarily withheld (without warning) on trial 12, 30, and 51 for each partner. On these trials,

both partners (except the self-directed P1 controls during their partner's turns) were prompted to independently record an estimation of either the just performed total MT or MT constant error.[4]

At the end of acquisition, participants responded to the interest/enjoyment, perceived competence, perceived choice, and effort subscales of the IMI (Ryan, 1982). The wording of the IMI items was customized to the task. Participants additionally responded to adapted versions of the perceived competence and perceived choice subscales, probing their perceptions of their partner's competence and choice over their practice. A 9-trial posttest, identical to the pretest (i.e., random schedule, no MT feedback) was then completed individually, while the partner waited outside (same counterbalanced order as for the pretest).

Participants returned alone the next day to complete 4 different, 9-trial delayed retention tests (each 3 trials/sequence), which varied in terms of the order in which the sequences were presented during the test (random vs. blocked sequence order) and whether or not MT-related KR was provided during the test. During the first 2 tests, participants did not receive KR, whereas they received KR during the last 2 tests. All participant groups completed all 4 delayed retention tests in the following order: (i) random-order/no-KR, (ii) blocked-order/no-KR, (iii) random-order/KR, and (iv) blocked-order/KR. During random-order tests, the goal MT was displayed on every trial, while during blocked-order tests, the goal MT was displayed on the first trial of each sequence's set of trials. Images of the sequences and goal MTs remained posted above the monitor if reminders were needed. At the end of testing, all participants responded to a customized questionnaire probing their perceptions of their practice experience and practice-

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[4] Analysis of the estimation data (i.e., actual error – estimated error) did not yield any group, partner (P1, P2) or estimation-type effects (Own, Partner). Participants were just as good at estimating their own error as they were for their partner. We did not expect this to depend on group.

scheduling decisions where appropriate (based on Wu & Magill, 2011). Responses to each question were based on a 7-point Likert-type scale, where 1 = *not at all true* and 7 = *very true*. Participants were then fully debriefed and compensated for their time.

5.2.4 Measures and Analysis

5.2.4.1 Self-directed switching behaviors

We evaluated 3 features of self-directed task-switching including switching strategy, switching frequency, and proportion matching. If task-switching was performance-contingent, absolute errors (AE) should be lower on "switch" trials than on the preceding trial ("switch – 1"). These AE data were analyzed in a 4 Subgroup (self-directed only) × 2 Trial-type (switch or switch – 1) mixed ANOVA, with repeated measures (RM). Switching frequency was also assessed and compared across the 4 self-directed Subgroups in a univariate ANOVA. A Pearson's *r* correlation was run to assess similarities in frequency of switching across the self-self pairs. One final way we measured the influence of a partner on self-directed practice was in terms of the proportion of sequences that were matched/block (i.e., P2s matched the P1s' sequences from the preceding block). The mean proportion of matched sequences was analyzed in a 3 Subgroup (P2s only) univariate ANOVA. Pearson's *r* correlations were used to assess relationships between these switching frequency and proportion of matched trials measures and retention test outcomes.

5.2.4.2 Percentage error

Absolute error (AE) in MT was calculated for each trial and converted to percent MT error (%MTE) based on the trial's associated MT goal, to normalize the size of the error to the goal of the sequence and enable comparisons across the 3 sequences within the same analysis block (Hodges et al., 2014; Karlinsky & Hodges, 2014; Simon & Bjork, 2001, 2002).

Specifically, for each trial, we divided the AE in MT by the relevant MT goal, and multiplied the result by 100%. For example, for an AE of 100 ms, the %MTE for each of the 3 sequences would be 11.1% (100/900 × 100), 8.3% (100/1200 × 100) and 6.7% MTE (100/1500 × 100). Variable error was also analyzed but the results mirrored those of %MTE and were thus omitted for brevity.

We separately analyzed the blocked-self versus random-self groups to capture any potential differences between experimenter- versus self-directed practice and the self-self P1s versus P2s, to assess the influence of partner observation. Error data for the pretest, acquisition, posttest, and delayed retention tests were analyzed in (sub)Group × Partner × Block RM ANOVAs.

5.2.4.3 Questionnaires

Participants' average score for each multi-item subscale of the IMI (interest/enjoyment, perceived choice, perceived competence, effort), and the adapted partner-related choice and competence scales was calculated and submitted to separate Group × Partner ANOVAs. Similar analyses were used for the post-practice experience measures.

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). Cohen's **f** values are reported as measures of effect size (where $\mathbf{f}^2 \times 100$ gives an indication of % variance accounted for), such that .25 – .5 is considered a medium to large effect, and power values (1 – ß) are given for non-statistically significant effects where $F > 1$. Any correlations > .30, considered a medium effect size (Cohen, 1992), are reported, along with the associated *p* value.

5.3 Results

Incorrect trials (i.e., wrong key pressed or key pressed before the beep) constituted 1.9% of all trials and were eliminated from analysis. Trials on which the MT error was greater than 1000 ms were considered errors and removed from further analysis (~0.05% of all correct trials). One participant (blocked-self P1) was excluded from analyses as she did not return for Day 2. Based on high errors in the no-KR retention test (>2.5 *SD*s of group's mean), 4 participants, across 4 different subgroups, were excluded from statistical analysis (1 blocked-self P1, 1 random-self P1, 1 self-self P1, and 1 blocked-self P2). In all cases, the data of the excluded participant's partner were maintained. The excluded P1s' practice schedule data were also still used in analyses related to their partner's self-directed switching behaviors (i.e., proportion match analysis and self-self switching frequency correlation). The final *n*s (in brackets) for the 6 partner subgroups within the 3 groups of pairs were as follows; blocked (15) – self (16); random (14) – self (15); and self (14) – self (15).

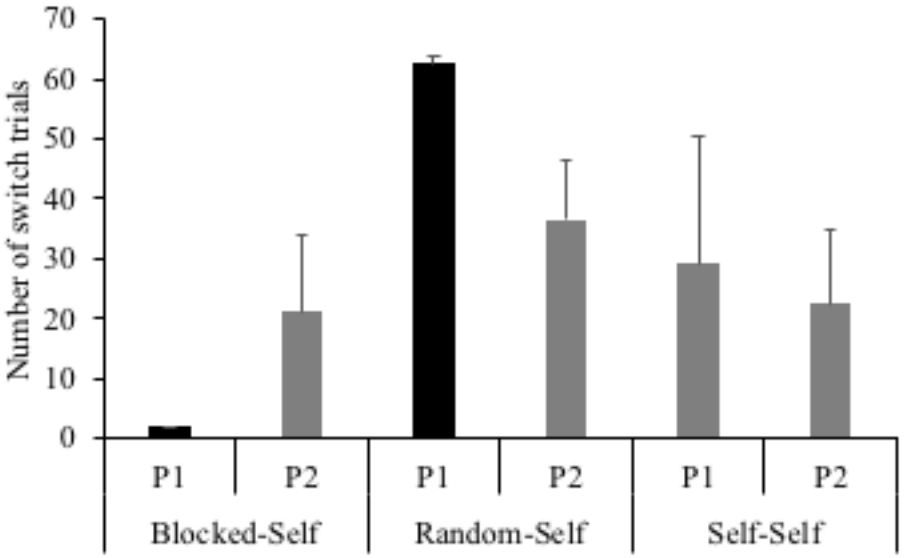
5.3.1 Performance-dependent and Partner-dependent Behaviors

In general, self-directed learners organized practice in a performance-contingent fashion based on findings that timing errors (i.e., AE) were significantly lower on switch trials (M = 93.7 ms, SD = 38.4 ms) than on the preceding trial (switch – 1; M = 113.0 ms, SD = 37.3 ms), irrespective of subgroup. This was confirmed by a significant and large effect of trial-type, $F(1, 56) = 26.85$, $p < .001$, **f** = .96, but no effect of subgroup ($F < 1$), nor a Subgroup × Trial-type interaction, $F(3, 56) = 1.20$, $p = .32$, 1 – ß = .31.

Switching frequency was shown to be dependent on subgroup, as illustrated in Figure 12 and evidenced by a main effect when comparing across the 4 self-directed subgroups, $F(3, 56) = 3.57$, $p = .02$, **f** = .44. Post-hoc analysis showed that practice with a random partner led to more

frequent task-switching (random-self P2s; ~36 trials) than practice with a blocked partner (blocked-self P2s; ~21 trials). There was a tendency for the random-self P2s to switch more than P2s watching self-directed practice (self-self P2s; ~22 trials), $p = .052$. There were no other significant differences (self-self P1s; ~29 trials).

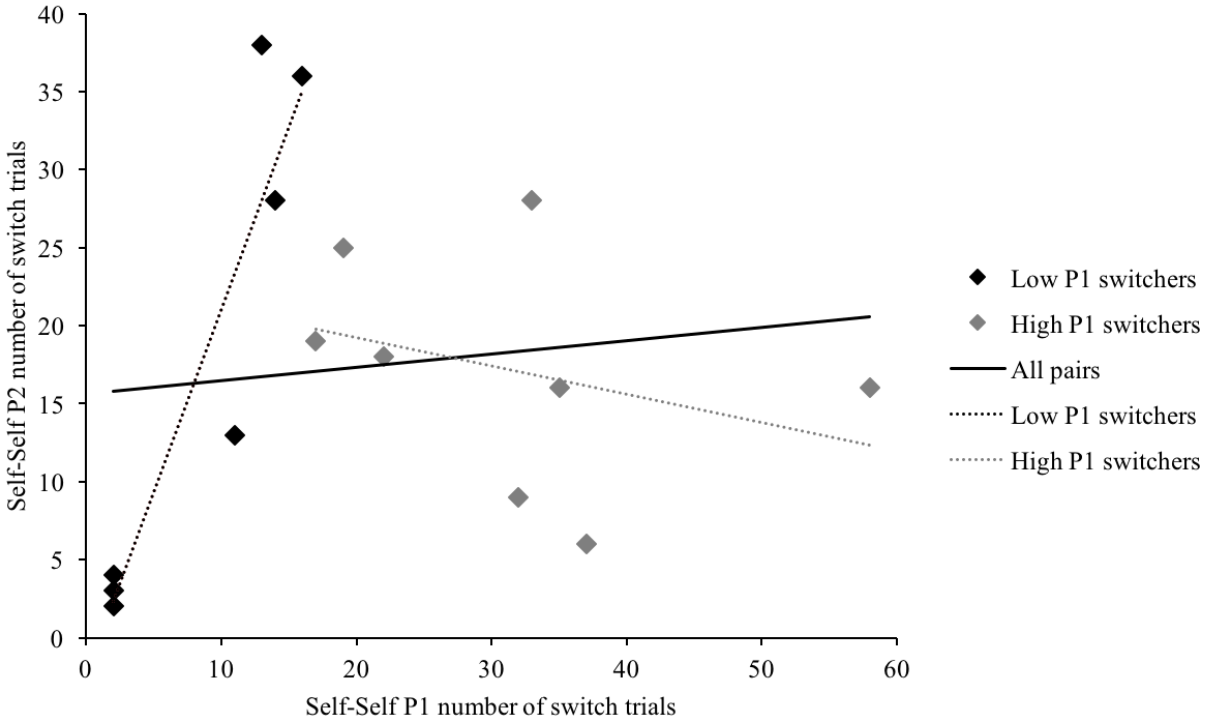
Figure 12. Mean number of trials (and *SD* bars) where participants switched to a different sequence over the course of the 72-trial acquisition session. Black bars = experimenter-determined task-switching. Gray bars = self-directed task-switching.



We also correlated switching frequency among the self-directed pairs who could choose what to practice. As can be seen in Figure 13, there was not a significant relation across the pairs ($r(15) = .12$, $p = .68$), but there appeared to be 2 relationships dependent on whether the P1s adopted a low (black symbols) or high (gray symbols) switching frequency. This was confirmed when we performed a median split analysis based on P1 'low-switchers' ($M = 8.57$ switches, $SD = 6.32$) and 'high-switchers' ($M = 31.63$ switches, $SD = 13.11$). For the low-switchers, P2s matched the switching frequency of their partners, $r(7) = 0.93$, $p = .003$, but this was not the case

for the P1s who switched frequently, where a trend toward a negative relation was shown, $r(8) = -0.32$, $p = .44$ (i.e., more switching by P1s, less switching by P2s).

Figure 13. Scatter plot showing the self-self partners' switching frequency and linear trend lines.



A final analysis designed to give an indication of sequence matching among the self-directed P2s showed evidence that learners paired with blocked-schedule partners matched the content of their partner's preceding practice block (blocked-self P2s; $M = 41\%$, $SD = 16\%$) significantly less than those paired with random-schedule partners (random-self P2s; $M = 81\%$, $SD = 6\%$) and self-directed partners (self-self P2s; $M = 72\%$, $SD = 18\%$). This was confirmed by a large, main effect of subgroup, $F(2, 43) = 32.93$, $p < .001$, $\mathbf{f} = 1.24$ and post-hoc Tukeys comparing the blocked-self P2s to the other P2 subgroups (ps $< .001$).

5.3.2 Outcome Effects in Acquisition and Retention

5.3.2.1 Random and blocked groups

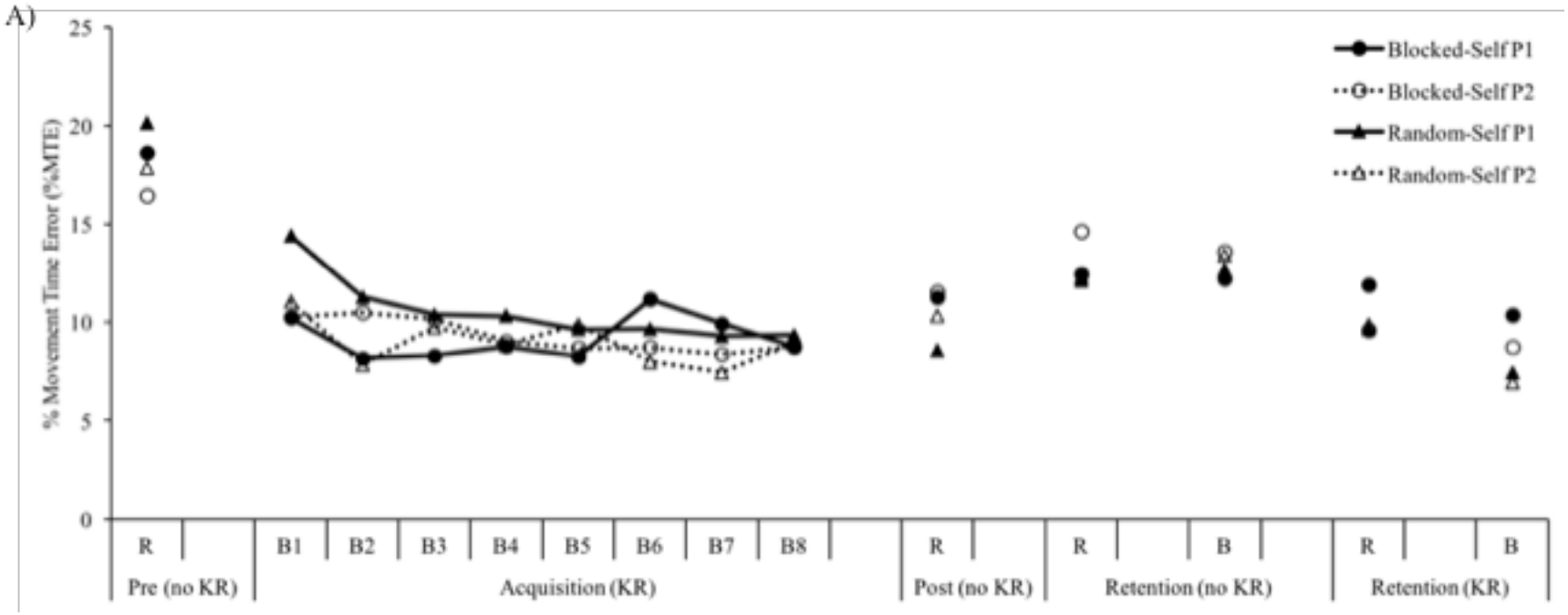
The blocked-self and random-self groups' mean %MTE across all testing phases is presented in Figure 14A. There were no group- or partner-related differences in the pretest (Fs < 1). Participants improved across practice, $F(5.03, 281.76) = 3.90$, $p = .002$, **f** = .27, confirmed by a linear trend component to the block effect ($p = .005$). Surprisingly, performance in acquisition did not vary as a function of group or partner (Fs < 1), nor were there any significant Group × Partner interactions (3-way: $F(5.03, 281.76) = 1.58$, $p = .17$, 1 – ß = .55).

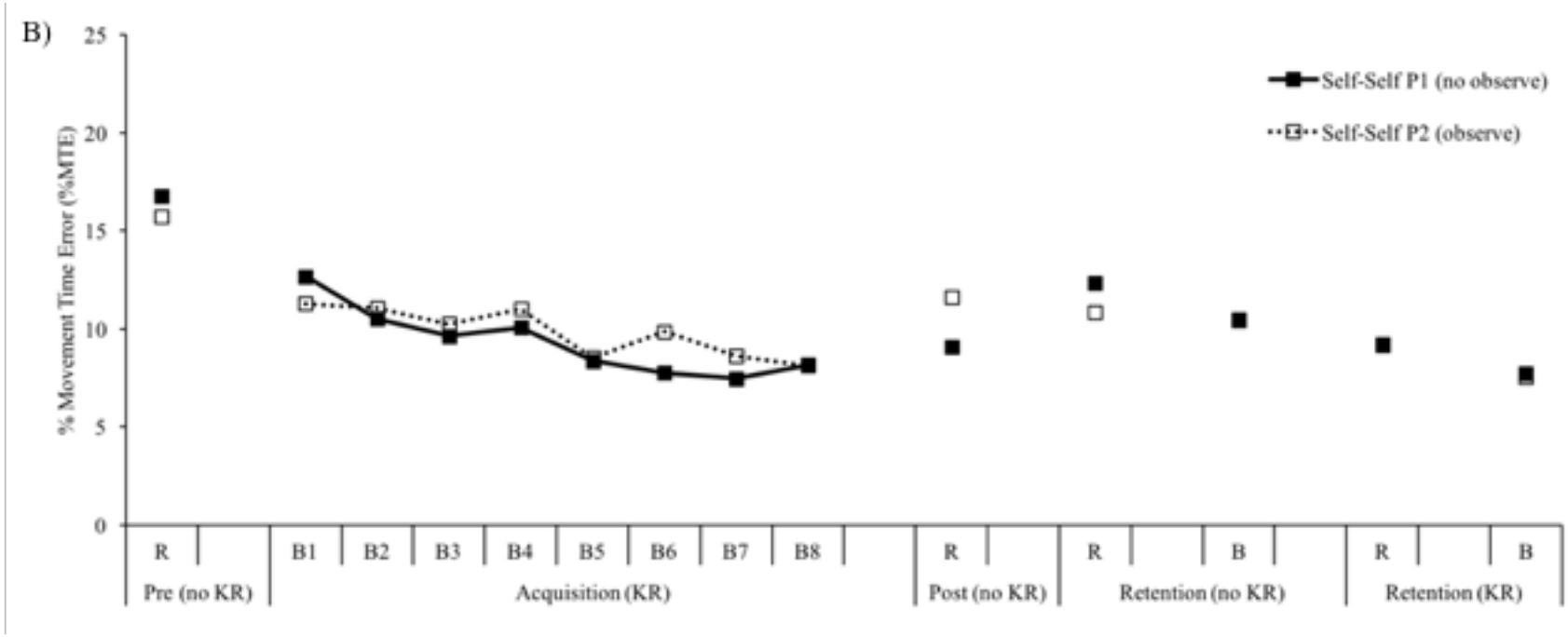
In immediate retention (posttest), there was a typical, medium-sized CI effect, whereby the random-self group had significantly less error (M = 9.48%, SD = 2.75%) than the blocked-self group (M = 11.44%, SD = 4.15%), $F(1, 56) = 4.70$, $p = .034$, **f** = .29. There were no partner-related differences. On Day 2, out of the 4 delayed retention tests, only the blocked-order/KR retention test yielded any group differences, whereby the random-self group (M = 7.19%, SD = 2.69%) had lower error than the blocked-self group (M = 9.55%, SD = 5.22%), $F(1, 56) = 4.76$, $p = .03$, **f** = .29. Again, there were no partner-related effects (Fs < 1).

5.3.2.2 Self-directed group

Mean %MTE for both partners in the self-self group is presented in Figure 14B. There were no partner differences in the pretest (F < 1). Both partners improved across practice, evidenced by a main effect of block, $F(7, 189) = 7.19$, $p < .001$, **f** = .60, which had a significant linear trend component ($p < .001$). There were no partner-related effects (Fs < 1). Although there was a trend for the P1 (no-observe) partners to have less error in the posttest, this was not significant, $F(1, 27) = 2.33$, $p = .15$, 1 – ß = .30. The delayed retention tests did not yield any partner-related differences (Fs < 1).

Figure 14 (A and B). Percentage movement time error (%MTE) as a function of experimenter- or self-directed practice within (A) the blocked-self and random-self pairs or within (B) the self-self pairs that either did not observe (P1) or did observe (P2) their partner's practice. All blocks consisted of 9 trials. KR = knowledge of MT results, R = random sequence order, B = blocked sequence order.




B)
% Movement Time Error (%MTE)
25
20
15
10
5
0
Self-Self P1 (no observe)
Self-Self P2 (observe)
R
B1
B2
B3
B4
B5
B6
B7
B8
R
R
B
R
B
Pre (no KR)
Acquisition (KR)
Post (no KR)
Retention (no KR)
Retention (KR)

5.3.3 Relations between Behaviors and Outcomes

Across the self-directed subgroups, frequency of task-switching was negatively related with %MTE during the immediate posttest, although this was only statistically significant for the random-self P2s ($r(15) = –.64$, $p = .01$). More frequent switching was generally related to lower error (blocked-self P2s, $r(16) = –.48$, $p = .06$; self-self P2s, $r(15) = –.43$, $p = .11$). The self-directed, P1 controls did not show any relation between switching frequency and %MTE (self-self P1s, $r(14) = –.01$, $p = .99$). Relationships between task-switching and %MTE did not persist in the delayed retention tests, with the exception of random-self P2s in the blocked-order/KR test, $r(15) = –.57$, $p = .03$ and a trend for the self-self P2s in the random-order/no-KR test, $r(15) = –.33$, $p = .23$ (all other rs < .30).

We were also interested in whether sequence matching moderated retention error, as shown in previous research involving interspersed demonstrations of matched or mismatched sequences (Lee et al., 1997; Simon & Bjork, 2002). For the blocked-self group, matching a partner's practice was related to higher error (%MTE) for both members of the pair in the immediate posttest; blocked-self P1s, $r(15) = .53$, $p = .04$; blocked-self P2s, $r(16) = .57$, $p = .02$. In contrast, there were negative correlations between matching and %MTE in the posttest within the random-self pairs, although these were not significant; random-self P1s, $r(14) = –.35$, $p = .21$; random-self P2s, $r(15) = –.42$, $p = .12$. These relationships were not observed for the self-directed P2s in delayed retention (all rs < .30). However, similar relationships between matching and error tended to persist for the P1s across the delayed retention tests, such that being matched was related to higher error for blocked-self P1s (blocked-order/no-KR test: $r(15) = .35$, $p = .20$; blocked-order/KR test: $r(15) = .39$, $p = .16$), but lower error for random-schedule P1s (random-

order/no-KR test: $r(14) = -.34$, $p = .23$; blocked-order/no-KR test: $r(14) = -.53$, $p = .049$; random-order/KR test: $r(14) = -.54$, $p = .048$; all other rs < .30).

5.3.4 Measures of Affective Experiences Related to Motivation

5.3.4.1 Intrinsic motivation inventory

The data were analyzed separately for the self-self group and the CI-paired groups (random-self; blocked-self), but the data for all groups are presented in Table 7. Observing a partner (or not) did not impact the self-directed practice experiences for the self-self pairs. They generally scored medium to high on all measures and there were no partner-related effects (interest/enjoyment: $F(1, 27) = 1.34$, $p = .26$, $1 - ß = .20$; effort: $F(1, 27) = 2.07$, $p = .16$, $1 - ß = .28$; all other scales, Fs < 1).

For the CI-paired groups, the blocked-self P1s had higher perceived self-competence than the random-self P1s, but this was not the case for the partners who self-directed practice. Rather the random-self P2s had higher self-competence ratings than the blocked-self P2s. This was evidenced by a significant Group × Partner interaction, $F(1, 56) = 7.95$, $p = .007$, $\mathbf{f} = .38$. Perceptions of partners' competency were generally high and consistent across the 4 CI paired subgroups (i.e., no group- or partner-related effects, Fs < 1).

Not surprisingly, the self-directed P2s perceived greater choice over their own practice than P1s, as evidenced by a partner-effect, $F(1, 56) = 5.75$, $p = .02$, $\mathbf{f} = .32$. There were no group-related effects. Congruently, the P1s perceived their self-directing partner as having greater choice over their practice, $F(1, 56) = 10.01$, $p = .003$, $\mathbf{f} = .42$. Again, there were no group-related effects (Fs < 1). There were no group- or partner-related effects with respect to interest/enjoyment toward practicing the tasks or perceptions of effort (Fs ranged from < 1–1.74).

Table 7. Mean ratings (and *SD*s) for the Intrinsic Motivation Inventory subscales and customized partner-related subscales.

Measure	Blocked-Self		Random-Self		Self-Self	
	P1	P2	P1	P2	P1	P2
Interest/enjoyment	3.9 (1.4)	3.9 (1.3)	3.9 (1.9)	4.0 (1.4)	4.7 (1.4)	4.1 (1.4)
Competence						
Own	4.9 (0.8)	4.0 (1.0)	4.0 (0.9)	4.7 (1.1)	4.6 (0.9)	4.3 (1.2)
Partner	4.9 (0.9)	4.8 (0.9)	5.0 (1.0)	5.0 (0.8)	N/A	5.1 (1.0)
Choice						
Own	4.8 (0.8)	5.3 (0.7)	4.5 (1.0)	5.1 (0.8)	5.3 (0.9)	5.4 (0.9)
Partner	5.0 (0.5)	4.5 (1.1)	5.0 (0.8)	4.2 (1.0)	5.1 (1.0)	5.0 (0.9)
Effort	5.2 (0.8)	5.2 (1.2)	5.0 (1.4)	5.7 (1.0)	5.3 (1.1)	5.8 (0.9)

Note. The self-self P1s did not provide ratings regarding their perceptions of their partner's competence at the keystroke sequence task, as they did not observe their partner's practice. Note that P1s for the other groups did not have choice over their practice schedule. Scales ranged from 1–7.

5.3.4.2 Paired practice experience questionnaire

The questionnaire's items and results are presented in Table 8. While ratings were generally high, observing a partner's practice was perceived as more helpful by self-directed learners than by their partners (in the CI-paired groups), $F(1, 56) = 4.96$, $p = .03$, **f** = .29. Moreover, the blocked-self and random-self P1s rated watching their partner as more interfering for their own performance than their self-directed P2 counterparts $F(1, 56) = 5.87$, $p = .019$, **f** = .32. For both these items there were no group differences.

With respect to competition and the desire to be more accurate than their partner, there were no group or partner differences. When asked whether they would have preferred to practice alone, participants' ratings were generally low, but the blocked-self group cited a greater preference ($M = 3.96$, $SD = 2.13$) than the random-self group ($M = 2.79$, $SD = 1.61$), $F(1, 56) = 5.82$, $p = .02$, **f** = .31 (Group × Partner: $F(1, 56) = 2.22$, $p = .14$, $1 - ß = .31$).

Table 8. Mean ratings (and *SD*s) to customized paired practice experience questionnaire.

	Blocked-Self		Random-Self		Self-Self	
	P1	P2	P1	P2	P1	P2
1. Watching my partner helped my own performance	4.5 (1.3)	5.6 (1.0)	5.1 (1.4)	5.5 (1.3)	3.6 (1.3)[1]	4.5 (1.6)
2. Watching my partner interfered with my own performance	2.7 (1.4)	2.3 (1.3)	3.4 (1.6)	2.1 (1.2)	2.9 (1.2)[2]	2.9 (1.5)
3. I wanted to be more accurate than my partner	5.0 (1.6)	4.4 (1.3)	5.3 (1.9)	4.9 (1.9)	4.8 (1.5)	4.7 (1.4)
4. I would have preferred to practice alone	4.6 (2.1)	3.3 (2.0)	2.7 (1.8)	2.9 (1.5)	3.7 (1.5)	3.4 (2.0)

Note. All questions were responded to using a 7-point Likert-type scale, where 1 = *not at all true* and 7 = *very true*.
[1] As the self-self P1s did not observe their partner, they responded to the question, "Being watched by my partner helped my own performance".
[2] As the self-self P1s did not observe their partner, they responded to the question, "Being watched by my partner interfered with my own performance".

5.3.4.3 Self-directed practice questionnaire

When the P2s were asked whether their practice decisions were influenced by their partner's schedule, the random-self P2s (M = 1.60, SD = .83) perceived this to be less true (ps < .05) than the blocked-self P2s (M = 3.69, SD = 2.06) and self-self P2s (M = 3.80, SD = 2.51), $F(2, 43) = 6.19$, $p = .004$, **f** = .54. Consistent with the task-switching strategy observed in the behavioral data (i.e., lower error on switch than pre-switch trials), self-directed learners near unanimously reported repeating the same sequence following relatively poor performance (87%) and the majority reported choosing to practice a different sequence following relatively good performance (57%; see Table 9). However, this latter finding was primarily driven by the random-self P2 and self-self P1 subgroups (66%). If given the opportunity to redo the practice session, most reported they would not change anything (53%) or that they would switch sequences less often (37%). Overall, the questionnaire results demonstrate learners' desire to repeat poorly performed sequences and their opinion that less switching would be better for learning (see also Hodges et al., 2014; Karlinsky & Hodges, 2014; Simon & Bjork, 2001; Wu & Magill, 2011).

Table 9. Frequency of responses to questions assessing self-directed practice choices.

	Blocked-Self	Random-Self	Self-Self	
	P2	P2	P1	P2
1. If I did well on a trial practicing a certain pattern, I would choose...				
A) The same pattern again	7	3	3	5
B) A different pattern	8	10	9	7
C) Random	0	1	0	1
D) Other	1	1	2	2
2. If I did poorly on a trial practicing a certain pattern, I would choose...				
A) The same pattern again	13	14	11	14
B) A different pattern	2	0	1	0
C) Random	0	0	0	0
D) Other	1	1	2	1
3. If I could redo the practice session, I would...				
A) Switch patterns more often	0	2	2	1
B) Switch patterns less often	6	6	4	6
C) Not change anything	10	6	8	8
D) Other	0	1	0	0

5.4 Discussion

We assessed if and how vicarious practice experiences related to a partner's practice schedule impacted self-directed task-switching and motor learning. In doing so, we applied a novel dyad-learning paradigm, which also provides insights into practice scheduling behaviors (and perceptions) within social contexts. Self-directed practice was similar across groups in its performance-contingent nature, but it was modulated by a partner's schedule with respect to the frequency of task-switching and matching versus mismatching of practice content.

In terms of practice choices, a partner's random practice promoted task-switching, leading self-directed learners to incorporate greater CI into their practice. Our paradigm also afforded consideration of an as yet unexamined aspect of self-directed multi-skill practice scheduling, that is, how learners choose to match or mismatch observed behaviors. The random-self P2s who were paired with a random-schedule partner seemed to compensate for the high switching they observed by matching the content of their partner's practice across blocks, potentially to achieve a more reasonable or "optimal" level of challenge (see Guadagnoli & Lee, 2004). For this random-self P2 subgroup, more matching was related to lower error in the posttest. In contrast, when observing a blocked partner, self-directed learners (blocked-self P2s) showed less matching of content ($p = .06$). This seemed to have modulated outcome-related effects, as less matching for this group (that was already observing an "easy" schedule of practice) was associated with lower error in the posttest (see also Lee et al., 1997; Simon & Bjork, 2002). That self-directed learners could select for themselves when to emulate may have contributed to these subgroups perceiving a partner's practice as more helpful and less interfering than the predetermined-schedule subgroups. These findings are consistent with a cognitive effort framework of learning, which supports the implementation of practice conditions

that impose demands on the learner's information processing activities to promote learning (without exceeding a skill level-appropriate load; Guadagnoli & Lee, 2004). Importantly, despite the impact of a partner on aspects of practice scheduling, self-directed learners did not sacrifice the performance-contingent nature of their task-switching.

We predicted that a self-directed group that did not view another's practice (i.e., self-self P1s) would adopt an error-dependent scheduling strategy, choosing to switch tasks following relatively good performance (e.g., Hodges et al., 2014; Karlinsky & Hodges, 2014; Wu & Magill, 2011), which was confirmed. For the other self-directed groups that watched a partner, we also saw a similar error-dependent strategy, where timing errors were, on average, lower on switch trials than on the preceding trial (and the no-observation, control P1s were not different in the relative differences in error across switch and pre-switch trials). Therefore, this type of (own) performance-contingent practice withstands external influences presented by social learning settings, even though behaviors appear to be moderated by a partner. It is likely that a relative increase in the amount of switching for learners observing a random-schedule partner is still moderated by the individual's own performance error.

One of the potential issues with the current design was that practice was alternated across blocks rather than trial-to-trial. This was done so that matched pairs would be privy to the practice schedule of their partner immediately (i.e., blocked or random). However, alternating practice on a trial-to-trial basis would allow consideration of whether self-directing learners prefer demonstrations to serve a modeling and/or feedback function (where demonstrations would match the learner's to-be performed or just performed skill, respectively), as well as whether decisions to match or mismatch are related to the success of the partner (i.e., error).

In terms of overall error, the random-self group showed less error on the immediate, no-KR posttest, as well as in the blocked-order/KR retention test, compared with the blocked-self group (although they did not differ on the other tests of retention). These posttest differences were achieved without the typically seen degradation of performance in acquisition as a function of random practice. We infer that self-directed learners showed a small benefit from observing random-schedule practice, in line with previous research showing that interspersed demonstrations that augment the CI within practice is beneficial for learning (Simon & Bjork, 2002). The lack of partner-related effects or interactions in these group-level outcomes provides evidence that self-directed learning does not depend on achieving the high levels of CI obtained via random practice (e.g., Keetch & Lee, 2007; see also Karlinsky & Hodges, 2014). However, observing random practice promoted task-switching, and for this subgroup (random-self P2s), greater switching was associated with lower error in post-testing and the blocked-order/KR delayed retention test. Whether these task-switching levels uniquely related to these learning effects, or whether there were additive benefits of task-switching and observed random practice remains to be disentangled. There is evidence that pure observational practice of a random (vs. blocked) practice schedule is sufficient to bring about CI-related effects in retention (Blandin et al., 1994; Wright, Li, & Coady, 1997; yet see Lee & White, 1990).

Because we were interested in how partners influence each other in a learning paradigm, we also asked questions to probe the perceptions and strategies guiding their performance. Contrary to predictions of some authors, self-directed practice was not associated with higher perceptions of interest/enjoyment compared with predetermined conditions (*cf.* Lewthwaite & Wulf, 2012; Sanli et al., 2013), even though autonomy (choice) perceptions were higher in the self-directed subgroups. This is consistent with recent findings that self-controlled practice

enhanced learning over yoked practice in the absence of augmented motivation (Ste-Marie, Vertes, Law, & Rymal, 2013). However, it is possible that the current dyad context promoted interest/enjoyment among the predetermined-schedule learners, due to enhanced 'relatedness' (e.g., Lewthwaite & Wulf, 2012).

While all groups perceived their competency as fairly high, the relatively high perceptions of competence associated with blocked rather than random practice, are commensurate with other CI-related work (Simon & Bjork, 2001, 2002). The ease of practice is often seen as an indication of overall learning, despite outcome data showing this not to be the case. Also worth noting, is that all groups rated their partner's competency as high, which should have given good reason for copying (switching or matching).

Dyad practice provides the opportunity for social comparisons. Researchers have demonstrated that relative (comparative) feedback about another (either a peer or average) influences one's competency beliefs and potentially motor skill learning (e.g., Wulf, Chiviacowsky, & Lewthwaite, 2010). Comparative effects have usually been based on virtual or experimenter-provided comparisons, but a real partner is potentially a better model for assessing comparative information, as we typically judge ourselves in reference to how people in our immediate environment are doing (Stanne, Johnson, & Johnson, 1999). Indeed, all our groups reported some desire to be more accurate than their partner, despite the fact that they were not being evaluated in reference to each other.

The data from this investigation, though informative as to when and what types of influence a partner can have on another's practice, also yielded some unexpected effects, or lack of effects, with respect to behavioral outcomes. The first is the lack of difference between the self-directed partners that did or did not observe their partner. Although we had evidence with

the blocked-self and random-self groups that observation affected practice and outcomes, here this was not the case. This may be related to the large variability between individuals as to how they practiced and difficulties in observing matched behaviors when only a few showed very blocked or high random schedules. Indeed, we saw stronger relationships between partners' switching frequencies when we separately assessed the relatively low-switching P1s (and partners) from the relatively high-switching P1s (and partners). Whether self-directing partners would be more or less likely to adopt similar task-switching habits when both members of the pair can observe one another remains to be tested. Further insight into how learners choose to practice and potential task-sharing between pairs could also be gained by removing the constraint that all sequences be practiced equally.

Another issue concerns CI-related effects. Although the random-self group showed less error than the blocked-self group in the posttest and blocked-order/KR delayed retention test, we did not see group differences in practice or in the no-KR delayed retention tests. As the CI effect is relatively robust in such simple tasks as these (and has been shown previously using these same keystroke sequences and goal MTs; Hodges et al., 2014; Simon & Bjork, 2001, 2002), it is possible that the addition of a partner's 'demonstrations' modulated the strength of the typical CI effect. However, there were other factors that differed from a typical task design including rest between blocks during a partner's turns, trials without feedback in practice, and pretesting. All of these factors could potentially dilute group differences due to CI, as these first 2 factors would serve to reduce the ease of blocked practice compared with random, whereas the latter might decrease the negative impact of random practice. Indeed, we made these changes to facilitate performance in the no-KR retention tests due to findings from a previous study where practice with feedback did not transfer well to conditions without (Karlinsky & Hodges, 2014).

In summary, we have provided evidence showing that when people learn in pairs their practice behaviors are influenced by their partners, but not in such a way that it impacts the strategies they typically adopt when performing alone. In this task, as with others, this was evidenced by performance-contingent practice, involving increased switching on lower versus higher error trials. In addition, self-directed learners showed some susceptibility to the practice behaviors of a co-learner, especially when the latter exhibited particularly salient blocked or random practice schedules. We anticipate that such investigations of learning in a social context will be important for guiding or affirming learning principles that have been based on an individual context. Given potential efficiencies from such shared practice contexts as well as their commonality in many motor learning settings, this will be an important line of inquiry to pursue toward enhancing not only theoretical discourse, but also applied motor learning.

5.5 Bridging summary

Experiment 4 established that self-directed learners are sensitive to the practice schedule of a partner, especially when they exhibit particularly low- or high-CI patterns of task-switching. Partners alternated turns after blocks of 9 trials to emphasize the blocked nature or random nature of the practice schedule. However, this design did not allow consideration of if and how self-directed learners choose to immediately match or mismatch a partner's practice and how such decisions potentially relate to the partner's success (error). Therefore, in Experiment 5, we planned a study replication with some differences in design features to further study the impact of a co-learner on self-directed practice. By allowing partners to alternate on consecutive trials, we could assess how a partner's practice schedule and their performance impacts the subsequent practice decisions, performance, and ultimately skill acquisition of self-directed learners.

Chapter 6: Experiment 5

Self-controlled practice schedules are modulated by a partner's practice in dyad learning

6.1 Introduction

Giving learners some control over their physical practice contributes to the learning of a wide variety of motor skills (for reviews on self-controlled practice, see Sanli, Patterson, Bray, & Lee, 2013; Wulf & Lewthwaite, 2016). Most of this research has focused on individual learners in isolated settings. There may be unique advantages or disadvantages associated with self-controlled practice in more social settings, where observing another person's practice could influence how learners make decisions about their own practice. Here we studied self-controlled practice within individual and dyad learning contexts, when individuals have control over their practice (task-switching) schedule. Partners took turns physically practicing and observing one another practice three different skills. Our primary aim was to determine how taking turns with a partner influences one's own practice decisions in reference to the partner's task-switching schedule and performance (error). A secondary aim was to study how such paired practice influences motor learning, with respect to behavioural outcomes as well as subjective perceptions of these self-controlled and dyad experiences.

Physical practice schedules often require periods of rest between attempts at a task. Not only have rest intervals been shown to be beneficial for motor learning (e.g., Baddeley & Longman, 1978; for reviews, see Donovan & Radosevich, 1999; Lee & Genovese, 1988), but there is suggestion that these rest intervals can be optimized if filled with periods of observation (for a review, see Ong & Hodges, 2012). Dyad practice, where partners alternate between physical practice and observation of one another, is one way to make efficient use of rest intervals as well as to promote learning of a single skill compared to practice alone (e.g., Granados & Wulf, 2007; Shea, Wright, Wulf, & Whitacre, 2000; Shea, Wulf, & Whitacre, 1999). However, the question of whether filling this rest interval detracts from potential rest

benefits is somewhat unclear. Little is known regarding the potential benefits or costs of practicing *multiple* skills in alternation with a partner. There may be reason to suspect that inter-trial intervals are more important for practice of multiple skills, where evidence supporting the importance of time to engage in inter-trial processing activities for motor learning has been underscored (e.g., Wright et al., 2016). Whether observation of a partner interferes or enhances processing activities that take place during these rest intervals is not yet known.

6.1.1 Multi-skill Practice Organization

Understanding how observing a co-learner's practice can modulate one's own practice choices is particularly important given the significant impact practice structure has on the learning of motor skills. This has frequently been shown in terms of the "contextual interference" (CI) effect (for a review, see Wright et al., 2016). This effect epitomizes the relationship between practice organization and motor learning via comparisons of blocked and random practice schedules. In blocked practice, skills are practiced repetitively and information processing "interference" experienced in practice is low. In random practice, skills are practiced unsystematically, increasing information processing demands and resulting in high between-trial interference. Blocked schedules typically facilitate performance in practice but degrade retention, while random schedules typically hinder performance in practice but enhance learning.

In social settings, observing a partner's practice to "fill" the inter-trial rest intervals could modulate the degree of CI within an individual's practice schedule. There is evidence that interspersing physical practice trials with demonstrations modulates the degree of CI experienced, affecting both immediate motor performance and longer-term learning (Lee, Wishart, Cunningham, & Carnahan, 1997; Simon & Bjork, 2002). Lee and colleagues (1997) provided computer-based models of performance before each trial of a random practice schedule,

with the goal of reducing individuals' need for planning operations by providing the "solution" to the task. Performance in practice was facilitated by these demonstrations that "matched" the upcoming skill, but retention was impaired compared to typical blocked and random practice. In an extension of this work, demonstrations that "mismatched" the next to-be-practiced skill degraded performance but improved retention for both blocked and random schedules (Simon & Bjork, 2002). These studies highlight the importance of how inter-trial intervals are filled within multi-skill practice schedules, and the potential for interspersed periods of observation to significantly impact the effectiveness of physical practice.

We recently tested this hypothesis about between-person interference in a paired learning context, where participants practiced two golf-putting skills in alternation (Karlinsky & Hodges, in review). Partners performed the same skill ("matched" group) or different skills ("mismatched" group) on consecutive trials and were compared to a group who physically practised the skills in the same order as one of the partners, without alternating turns (a partner just watched to control for any effects of an observing peer). On an individual level, all matched and mismatched partners experienced the same degree of CI, and practised in a semi-blocked schedule of practice (switching to a new putter every six trials). However, between-person CI would be higher for the mismatched group than the matched group. In terms of overall error, there were no differences between these groups in practice or in retention and the alternating practice groups did not show less error than the non-alternating, physical practice only group. There was, however, evidence that partners were adapting their actions based on the shots of their partner, with ~70% of actions being defined by between-person compensation (i.e., putting less far if a partner overshot or putting farther if a partner undershot). Therefore, although we had no evidence that principles of CI extend to "between-person" CI in this dyad learning study, we

did show that partners were influencing performance, if not ultimately at a benefit (or cost) to overall learning.

6.1.2 Self-controlled Practice Organization

A method which has revealed both how learners choose to practice as well as being an effective training tool is that of self-controlled practice. When learners are given control over when to switch between tasks, self-controlled practice schedules have been shown to yield learning benefits not seen when these schedules are imposed (i.e., for blocked or yoked practice schedules; e.g., Keetch & Lee, 2007; Wu & Magill, 2011). Such findings have led to speculations that giving learners control over their practice conditions, (a) satisfies their psychological need for autonomy, which in turn enhances investment in practice and motivation to learn (see Lewthwaite & Wulf, 2012; Wulf & Lewthwaite, 2016); and (b) provides the opportunity for customized, performance-dependent practice (e.g., Carter, Carlsen, & Ste-Marie, 2014; Chiviacowsky & Wulf, 2002, 2005), with more evidence supporting the latter interpretation.

Self-controlled practice organization has typically been evaluated in terms of the frequency and timing of individuals' decisions to switch between skills. While individuals have exhibited large variability in how often they choose to switch between tasks, they tend to adopt relatively low-CI practice schedules overall (choosing to switch on approximately one third of acquisition trials or less; e.g., Hodges, Edwards, Luttin, & Bowcock, 2011; Karlinsky & Hodges, 2014, 2018a; Wu & Magill, 2011). As above, despite the relatively low amounts of CI, the benefits of self-controlled practice have been attributed in part to the customized timing of task switches, with individuals generally choosing to switch to a different task following better (e.g., lower error, faster) performance (Hodges, Lohse, Wilson, Lim, & Mulligan, 2014; Karlinsky &

Hodges, 2014, 2018a; Keetch & Lee, 2007; Wu & Magill, 2011). This performance-contingent task-switching strategy has also been shown to be effective when learners are directed by a peer (Karlinsky & Hodges, 2014) or algorithm (Simon, Lee, & Cullen, 2008).

We recently considered how a partner's practice influenced self-controlled practice choices during dyad practice, when partners switched turns after every 9-trial block (Karlinsky & Hodges, 2018a). Blocks of trials were used to ensure the practice schedules were salient, as we manipulated whether the partner followed a blocked, random, or self-controlled schedule. Learners who practiced with a random-schedule partner chose to switch between tasks more frequently than those who practiced with a blocked-schedule or self-controlled partner. These learners also chose to match the content of their partner's preceding practice block (i.e., perform the same tasks as observed) to a greater extent than those paired with a blocked-schedule or self-controlled partner. However, this block-to-block form of turn-taking obscured the effects of observing a partner's turn on immediate practice behaviours and performance. Our aim in the current study was to assess how the practice of a partner in a turn-taking setting impacted the subsequent practice decisions, performance, and ultimately learning of their partner.

A growing body of research provides evidence that individuals monitor a partner's performance and adapt their own behaviours in response to observed errors in a peer (e.g., de Bruijn, Mars, Bekkering, & Coles, 2012; de Bruijn, Miedl, & Bekkering, 2011; *cf.* Picton, Saunders, & Jentzsch, 2012). Such partner-related behavioural adjustments tend to be more prevalent in cooperative as opposed to competitive performance contexts, suggesting that this is not an automatic process (e.g., de Bruijn, Miedl, & Bekkering, 2008; de Bruijn et al., 2012). However, adjusting one's own behaviour in response to a peer's performance can be detrimental to success at the task, suggesting that at least part of this behaviour might be done without

awareness or at least awareness of the consequences of partner-related adaptations (e.g., de Bruijn et al., 2008). Of particular relevance to the present multi-skill study, the observed task need not be the same as the to-be-performed task for observed errors to affect subsequent performance (Wang, Pan, Tan, Liu, & Chen, 2016). Of current interest is how such monitoring of a partner's accuracy might impact learners' choices about what to practice.

6.1.3 Study Aims and Hypotheses

A dyad-practice paradigm was used, where partners alternated turns on every trial. A self-controlled practice participant was paired with a partner following either an imposed blocked or random task-switching schedule. We were primarily interested in whether and how a partner's practice influenced self-controlled practice, in terms of task-switching frequency and strategy and ultimately learning outcomes. An additional group of individuals self-controlled their practice alone, for control purposes. Of interest was how learners would choose to repeat or switch tasks with reference to their own previous trial, and/or with respect to their partner's preceding trial by "matching" or "mismatching" what they observed. We anticipated that self-controlled learners exposed to random practice would exhibit more frequent task-switching than those exposed to blocked practice or who practiced alone (Hodges et al., 2014; Karlinsky & Hodges, 2018a). While more frequent (self-referenced) task-switching is typically better for learning, self-controlled switching frequency does not always correlate with learning outcomes (e.g., Karlinsky & Hodges, 2014; Keetch & Lee, 2007; Wu & Magill, 2011). It is possible that matching an observed task would facilitate performance but impair learning, due to the ease in copying a previously observed action (e.g., Lee et al., 1997; Simon & Bjork, 2002). However, while matching a partner in a turn-taking schedule has been shown to enhance single-skill learning (e.g., Granados & Wulf, 2007; Shea et al., 1999, 2000; *cf.* Karlinsky & Hodges, 2018b),

it did not modulate multi-skill learning compared to mismatched practice, when matching was experimenter-imposed (Karlinsky & Hodges, in review).

We anticipated that self-controlled learners who practiced alone would adopt a performance-contingent task-switching strategy, choosing to practice a different task following relatively good, low error trials (e.g., Hodges et al., 2014; Karlinsky & Hodges, 2014, 2018a; Keetch & Lee, 2007; Wu & Magill, 2011). In dyad contexts, task-switching was expected to become less self-referenced, potentially as a function of the partner's task choices and/or performance as a reference against which to guide their own behaviour (e.g., de Bruijn et al., 2011, 2012). If task-switching was dependent on the partner's performance and responded to in the same manner as one's own, we would expect individuals to switch tasks (i.e., mismatch a partner's preceding trial) following a partner's relatively good trials, and to repeat (i.e., match) the observed task following a partner's relatively poor performance.

We were also interested in learners' subjective perceptions of the practice conditions, towards building a more comprehensive understanding of the mechanisms underpinning dyad and self-controlled practice (see also Karlinsky & Hodges, 2018a). Participants responded to subscales of the Intrinsic Motivation Inventory (IMI; Deci & Ryan, n.d.), a questionnaire designed to assess participants' subjective experiences in relation to an activity. We hypothesized that dyad practice would be rated more positively than individual practice, as mixed physical and observational practice has been posited to have positive effects on motivation (e.g., Schmidt & Lee, 2014; Shea et al., 1999). We also anticipated that self-controlled practice would be more enjoyable than experimenter-imposed practice, given the motivational benefits thought to be associated with autonomy-supportive practice conditions (e.g., Lewthwaite & Wulf, 2012; Wulf & Lewthwaite, 2016). Customized questionnaires were

also used to probe learners' perceptions of the dyad and self-controlled practice conditions, including strategies driving choices during practice (see also Karlinsky & Hodges, 2018a; Wu & Magill, 2011).

6.2 Methods

6.2.1 Participants and Groups

Eighty right-handed females (*M* = 21.0 years, *SD* = 3.7 years) volunteered to participate individually and were assigned to practice alone or in pairs across 5 groups (*n* = 16/group). Partners did not know one another in advance and were paired based on availability. Pairs were assigned in alternation to form four "dyad" groups. They were first assigned to either a blocked or random schedule and to be either Partner 1 (predetermined schedule) or Partner 2 (self-controlled schedule). This led to four subgroups (blocked, random, blocked-self, and random-self). A fifth control group practiced alone and controlled their own practice schedule (self-alone group). Participants had normal or corrected-to-normal vision, were naïve to the specific goals of the study, and provided informed consent. Participants were reimbursed for their time (~$10/hr).

6.2.2 Task and Apparatus

The experimental task was based on Lee et al. (1997) and Simon and Bjork (2001) and involved learning to execute three different, 5-keystroke sequences on a 9-digit computer keypad (Dell SD-8115) using only the right index finger. Each sequence consisted of a unique set of keys and a distinct overall movement time (MT) goal (900, 1200, or 1500 ms). An LG computer and custom E-Prime 2.0 program (Psychology Software Tools, Inc., Sharpsburg, PA) were used to control stimuli presentation and record responses. Stimuli were presented on a monitor (ASUS HDMI 23 in.) set in the middle of a desk. During paired acquisition sessions, two chairs were

placed side-by-side facing the monitor, with the keyboard fixed centrally on the desk. During individual sessions, there was only one centrally located chair.

6.2.3 Materials

6.2.3.1 Intrinsic Motivation Inventory

The Intrinsic Motivation Inventory (IMI) is a multidimensional tool designed for laboratory use, which measures participants' subjective, task-specific experience (Deci & Ryan, n.d.). Participants responded to the following subscales of the IMI: interest/enjoyment (7 items), perceived choice (5 items), perceived competence (5 items), pressure/tension (5 items), and effort (5 items). The wording of the items was customized to the study, such that original references to the "task" were modified to specify the "keystroke task" (e.g., "I put a lot of effort into this keystroke task"). Pairs additionally responded to adapted versions of the perceived choice and perceived competence subscales (5 items/subscale), probing perceptions of their partner's choice over how they practiced and their partner's competence at the tasks, respectively (Karlinsky & Hodges, 2018a). These adaptations consisted solely of changing references to oneself within the items (e.g., "I am", "I did", "my performance") to references to the partner (e.g., "my partner is", "my partner did", "my partner's performance"). Participants rated the truthfulness of all subscale items using a 7-point Likert-type scale, with the anchors of 1 = *not at all true* and 7 = *very true*. Some items were reverse coded, so that for all subscales a larger score reflected a larger subjective experience of the relevant construct.

6.2.3.2 Dyad practice experience questionnaire

A set of four questions was used to probe pairs' perceptions of the dyad practice experience. Participants rated the truthfulness of the items using the 7-point scale described above.

6.2.3.3 Self-directed practice experience questionnaire

We asked the self-controlled learners who practiced in pairs two questions to assess how practice choices were influenced by their partner's schedule (i.e., whether the partner repeated the same pattern often or switched between different patterns often) and performance (i.e., whether the partner did well or poorly). Participants rated the truthfulness of these items using the same 7-point scale described above. A further five multiple choice questions were used to assess practice-scheduling preferences, based on the success of their own and their partner's performance (based on Wu & Magill, 2011).

6.2.4 Procedures

The study was conducted over two consecutive days. Day 1 was completed alone (group 5) or in pairs (groups 1–4) and began with the experimenter initiating some exchange with the participants before explaining the task. During a familiarization phase, participants were required to complete five successful trials of a 5-keystroke sequence, which had a different keystroke pattern and goal MT to those of the experimental trials. The sequence's goal MT was displayed at the start of each trial for 3 s. The sequence was then presented on screen and after 1 s a beep sounded, indicating the participant should enter the sequence when ready. An "S" marked the start key of the sequence, and black adjoining lines identified the order in which to press the keys. The image of the sequence remained on screen during the trial, eliminating the need for explicit memory of the pattern. After five keys had been pressed, the sequence remained on screen for a further 3 s before feedback was displayed. The feedback screen was shown for 4 s and informed participants, (a) whether the keys were pressed in the "correct" or "incorrect" order, (b) total MT in ms, and (c) signed MT error (constant error) in ms. Throughout the experiment, a successful trial required only that the correct keys be pressed in the correct order.

Participants then completed a 9-trial, random-order pretest on the three sequences used throughout the remainder of the experiment (3 trials/sequence). Although participants were aware of the goal MTs, only feedback about whether sequence execution was correct was provided. If a sequence was incorrectly executed (wrong key or key pressed too early), the trial was repeated at the end. Partner 1 always completed the familiarization and pretest first, while Partner 2 waited outside the room and completed the Edinburgh Handedness Inventory (Oldfield, 1971).

During paired acquisition sessions, partners sat next to one another with the monitor and keyboard centered between them so that they could each always see both. Partners' seating arrangement was counterbalanced. Partners were instructed to watch one another's practice but not to communicate. They were told that the goal of practice was to learn the goal MTs for the three sequences and that they would be tested without MT feedback.

All participants completed 24 correct trials of each sequence, such that the paired acquisition sessions consisted of 144 correct trials (72/partner). The individual group's trials were spaced to approximate the inter-trial breaks received by the dyad groups. Partners alternated turns starting with Partner 1s. The blocked sequence presentation order was counterbalanced within the group. The random sequence presentation order was the same for all participants but constrained not to repeat the same sequence more than twice consecutively. The self-control participants decided for themselves which sequence to practice at the start of each trial. If 24 trials for a sequence had been reached, the participant was prompted to choose again.

During acquisition, the MT goal was not presented after the first three correct trials of each sequence (for each partner), but images of the sequences and associated MT goals were posted above the monitor for reference. After the trial and feedback, there was an inter-trial

interval of 3 s after which an arrow pointing to the left or right was presented signaling whose turn was next (~2 s). If a trial was "incorrect", it remained the same partner's turn on the next trial. The incorrect trial was recycled and either, (a) repeated at the end of the relevant sequence's set of trials (blocked order), (b) repeated at the end of the acquisition trials (random order), or (c) available to be chosen again (self-control). After acquisition trials, participants independently responded to the Intrinsic Motivation Inventory (Deci & Ryan, n.d.).

Retention testing was conducted alone the next day. There were four blocks of 9-trial retention tests (3 trials/sequence). MT goals were not provided. The tests varied in terms of sequence presentation order (random vs. blocked) and whether MT feedback (knowledge of results, KR) was provided. The following order was used for all participants: (i) random-order/no-KR; (ii) blocked-order/no-KR; (iii) random-order/KR; and (iv) blocked-order/KR. At the end of retention, participants responded to customized questionnaires probing their perceptions of practice and practice-scheduling decisions where appropriate (based on Karlinsky & Hodges (2018a) and Wu & Magill (2011)). Participants were then debriefed and compensated for their time.

6.2.5 Measures and Analysis

6.2.5.1 Timing errors

Movement time (MT) was the time between the first and fifth keystrokes. For each trial, absolute error (AE) was calculated and converted to percent MT error (%MTE), based on the trial's associated MT goal. This normalized the size of the error to the sequence's timing goal and enabled comparisons across the three sequences. Variable error (VE) was also calculated based on the variance in the signed MT errors.

Data for the pretest and delayed retention tests were analyzed in separate 2 Schedule (blocked, random) × 2 Partner (predetermined Partner 1, self-controlled Partner 2) between-subject ANOVAs. We grouped the acquisition data into 9-trial blocks and analyzed these in a 2 Schedule × 2 Partner × 8 Block mixed ANOVA, with repeated measures (RM) on the last factor. A second analysis was also performed comparing the individual self-control group to the blocked-self and random-self groups. This two-phase analysis was repeated for all measures where relevant, comparing across the three self-control Groups, rather than Schedule and Partner.

6.2.5.2 Self-controlled practice behaviours

We evaluated how the person's own performance and the performance of their partner affected their practice decisions and errors. This was achieved three ways. First, we assessed task-switching frequency. This was the number of times self-controlled partners switched to practice a different sequence compared to their own previous trial (what we refer to as self-referenced switching) or compared to their partner's preceding trial (what we refer to as partner-referenced switching). These data were analyzed in a 2 Group (blocked-self, random-self) × 2 Reference (self, partner) mixed ANOVA. As a secondary analysis, we used a between-group ANOVA to compare the self-referenced task-switching frequencies of the self-controlled learners who practiced alone to those who practised in pairs. Pearson's *r* correlations were additionally calculated to assess relationships between the task-switching measures and retention test outcomes.

Second, we assessed self-controlled learners' task-switching strategy, based on self-referenced or partner-referenced errors. With this analysis we could test whether MT errors (i.e., AE) were on average higher on trials preceding a choice to repeat rather than switch sequences,

based on either their own schedule or in reference to their partner (what elsewhere we've referred to as partner-referenced matching or mismatching). AE was used as the best estimate of the information that participants would have used to assess performance and base these task-switching decisions (Wu & Magill, 2011). We first identified "switch" trials and recorded average AE based on all trials immediately preceding a switch to a different sequence. The "repeat" trials were chosen as the trial nearest to the switch trial but preceding it in time and one where the next trial was a repeat of the same sequence (Keetch & Lee, 2007). These data were analyzed in a 2 Group (blocked-self, random-self) × 2 Reference (self, partner) × 2 Trial-type (repeat, switch) mixed ANOVA, with RM on the last two factors. An additional 3 Group × 2 Trial-type RM ANOVA was used to compare the self-referenced task-switching strategies of the self-controlled groups.

A third analysis was conducted to assess whether the content of a partner's practice trial affected subsequent trial accuracy. That is, we assessed whether observing the same (matched) or different (mismatched) sequence before a practice attempt influenced timing error (i.e., %MTE) on the next trial. We included the imposed-schedule partners in this analysis as we would also expect their performance to be impacted by whether they observed a matched or mismatched demonstration of their upcoming task (Lee et al., 1997; Simon & Bjork, 2002). These data were submitted to a 2 Schedule × 2 Partner × 2 Demonstration-type (matched, mismatched) mixed ANOVA, with RM on the last factor.

6.2.5.3 Questionnaires

Participants' average score for each subscale of the IMI and the adapted partner-related choice and competence subscales was calculated. Across all five groups, Cronbach's alpha values were good for the interest/enjoyment ($\alpha = .95$), own-competence ($\alpha = .85$), partner-

related competence (α = .84), and pressure/tension (α = .90) subscales; however, the values were weak for the own-choice (α = .52), partner-related choice (α = .60), and effort (α = .65) subscales. Thus, these three latter subscales have been omitted. For the dyad groups, these data were analyzed in separate 2 Schedule × 2 Partner between-subject ANOVAs. For competence, an additional factor of Reference (i.e., "self" or "partner" competence) was included. We also compared the three self-controlled groups in a one-way ANOVA. Responses to the dyad practice experience questions were analyzed using separate 2 Schedule × 2 Partner between-subject ANOVAs. The self-controlled learners' responses to questions regarding the influence of their partner's practice schedule and performance on their own practice scheduling decisions were analyzed using a 2 Group (blocked-self, random-self) × 2 Partner-influence (schedule, performance) mixed ANOVA.

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 $F > 1$. Any correlations of medium effect size or greater ($r > .30$; Cohen, 1992) are reported, along with the associated *p* value.

6.3 Results

Trials that were performed incorrectly (i.e., wrong key or key pressed too early) were removed before analysis (1.7% of all trials). Trials during test phases on which MT errors were greater than 1000 ms were considered errors and excluded from analysis (1.0% of all correct test trials). Based on high errors in the pretest (>100% MTE), one blocked pair was excluded from all statistical analyses (leaving $n = 15$ for each of the blocked and blocked-self groups).

6.3.1 Timing error

6.3.1.1 Comparisons across the dyads

For all five groups, average %MTE for the pretest and acquisition phase is presented in Figure 15A and average VE is presented in Figure 15B. For both measures, there were no significant differences in the pretest (all *F*s < 1.4). For %MTE, the pairs improved across practice, $F(4.38, 253.99) = 16.10$, $p < .001$, $\eta_p^2 = .22$, confirmed by a linear trend component to the block effect ($p < .001$). Consistent with the CI effect, there was a significant Schedule effect in acquisition. The blocked pairs were more accurate ($M = 8.7\%$, $SD = 4.5\%$) than the random pairs ($M = 11.2\%$, $SD = 5.8\%$), $F(1, 58) = 8.66$, $p = .005$, $\eta_p^2 = .13$. Apart from a Schedule × Block interaction, $F(4.38, 253.99) = 2.90$, $p = .019$, $\eta_p^2 = .048$, due to decreasing differences between groups as practice progressed, there were no other significant effects or interactions (*F*s < 1) in acquisition. These effects were mirrored in the VE data (Figure 15B). VE decreased across practice, $F(5.12, 296.68) = 9.23$, $p < .001$, $\eta_p^2 = .14$, confirmed by a linear trend component to the block effect ($p < .001$). The blocked pairs were more consistent in acquisition ($M = 124.4$ ms, $SD = 64.8$ ms) than the random pairs ($M = 164.6$ ms, $SD = 78.8$ ms), $F(1, 58) = 14.73$, $p < .001$, $\eta_p^2 = .20$, but there were no partner-related effects or interactions (*F*s < 1).

Means for %MTE in retention are displayed in Figure 16A. Comparisons of the pairs in retention did not yield group differences (as a function of either schedule or partner) on the first random-order/no-KR test (*F*s < 1). There was, however, a Schedule effect for the blocked-order/no-KR test in the expected direction; the random pairs had lower %MTE ($M = 12.3\%$, $SD = 4.9\%$) than the blocked pairs ($M = 15.0\%$, $SD = 4.8\%$), $F(1, 58) = 4.61$, $p = .036$, $\eta_p^2 = .074$. There were no partner-related effects or interactions (*F*s < 1). Although the random pairs

continued to perform with lower error in the random-order/KR retention test (*M* = 9.3%, *SD* = 3.7%) than the blocked pairs (*M* = 10.7%, *SD* = 2.9%), the difference was not significant, $F(1, 58) = 2.64$, $p = .11$, $1 - ß = .39$. On the final blocked-order test with KR, there were no schedule-related effects or interactions (*F*s < 1), but the imposed-schedule Partner 1s had lower error (*M* = 7.1%, *SD* = 2.0%) than the self-controlled Partner 2s (*M* = 9.1%, *SD* = 3.7%), $F(1, 58) = 6.47$, $p = .014$, $\eta_p^2 = .10$. This last effect was mirrored in the VE data (see Figure 16B), with the imposed-schedule partners being more consistent (*M* = 105.2 ms, *SD* = 30.2 ms), than the self-controlled partners (*M* = 140.2 ms, *SD* = 65.2 ms), $F(1, 58) = 7.15$, $p = .010$, $\eta_p^2 = .11$. There were no other schedule- or partner-related effects for VE in retention (all *F*s < 1.3).

6.3.1.2 Comparisons between the alone and dyad self-control groups

In general, there were no group-related effects for either %MTE or VE. Regardless of group, self-controlled participants improved with practice with respect to %MTE, $F(5.39, 237.06) = 7.14$, $p < .001$, $\eta_p^2 = .14$, and VE, $F(4.62, 203.29) = 5.60$, $p < .001$, $\eta_p^2 = .11$. There were no group differences in retention (blocked-order/no-KR test: $F(2, 44) = 1.56$, $p = .22$, $1 - ß = .31$; random-order/KR test: $F(2, 44) = 1.25$, $p = .30$, $1 - ß = .26$; all other *F*s < 1).

6.3.2 Self-controlled Practice Behaviours

6.3.2.1 Task-switching frequency

6.3.2.1.1 Comparisons across the dyads

As illustrated in Figure 17, self-controlled task-switching frequency was dependent on group. Practice with a random-schedule partner led to more frequent task-switching (~41 trials) than practice with a blocked partner (~22 trials), $F(1, 29) = 13.71$, $p = .001$, $\eta_p^2 = .32$. Participants also chose to switch sequences with reference to their partner's preceding trial more often than with respect to their own preceding trial (~37 partner-referenced switches vs. ~26 self-

referenced switches), $F(1, 29) = 11.19$, $p = .002$, $\eta_p^2 = .28$. There was no Group × Reference interaction ($F < 1$).

6.3.2.1.2 Comparisons between the alone and dyad self-control groups

A comparison of the three self-controlled groups confirmed that those who practiced with a random-schedule partner chose to switch tasks (with reference to their own previous trial) more frequently than those who practiced with a blocked-schedule partner ($p = .002$) or alone ($p < .001$), $F(2, 44) = 11.02$, $p < .001$, $\eta_p^2 = .33$. Practice with a blocked-schedule partner did not modulate task-switching frequency compared to practice alone ($p = .81$).

Figure 15 (A and B). (A) Percentage movement time error (and *SE* bars) and (B) Variable error (and *SE* bars) for the pretest and acquisition blocks as a function of predetermined or self-controlled practice schedules. KR = knowledge of results, R = random sequence presentation order.

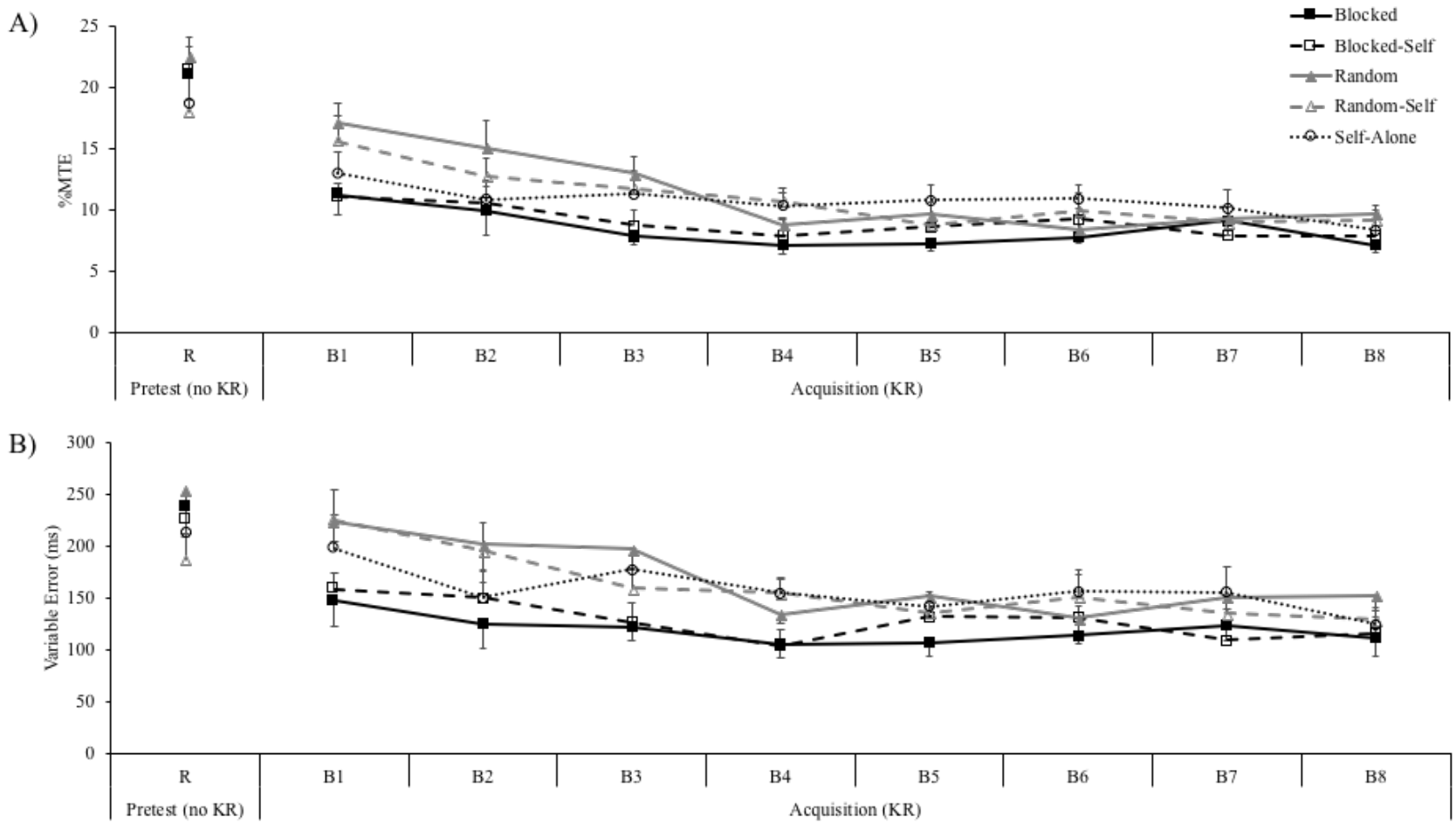


Figure 16 (A and B). (A) Percentage movement time error (and *SE* bars) and (B) Variable error (and *SE* bars) for the retention tests as a function of predetermined or self-controlled practice schedules. KR = knowledge of results, R = random sequence presentation order, B = blocked sequence presentation order.

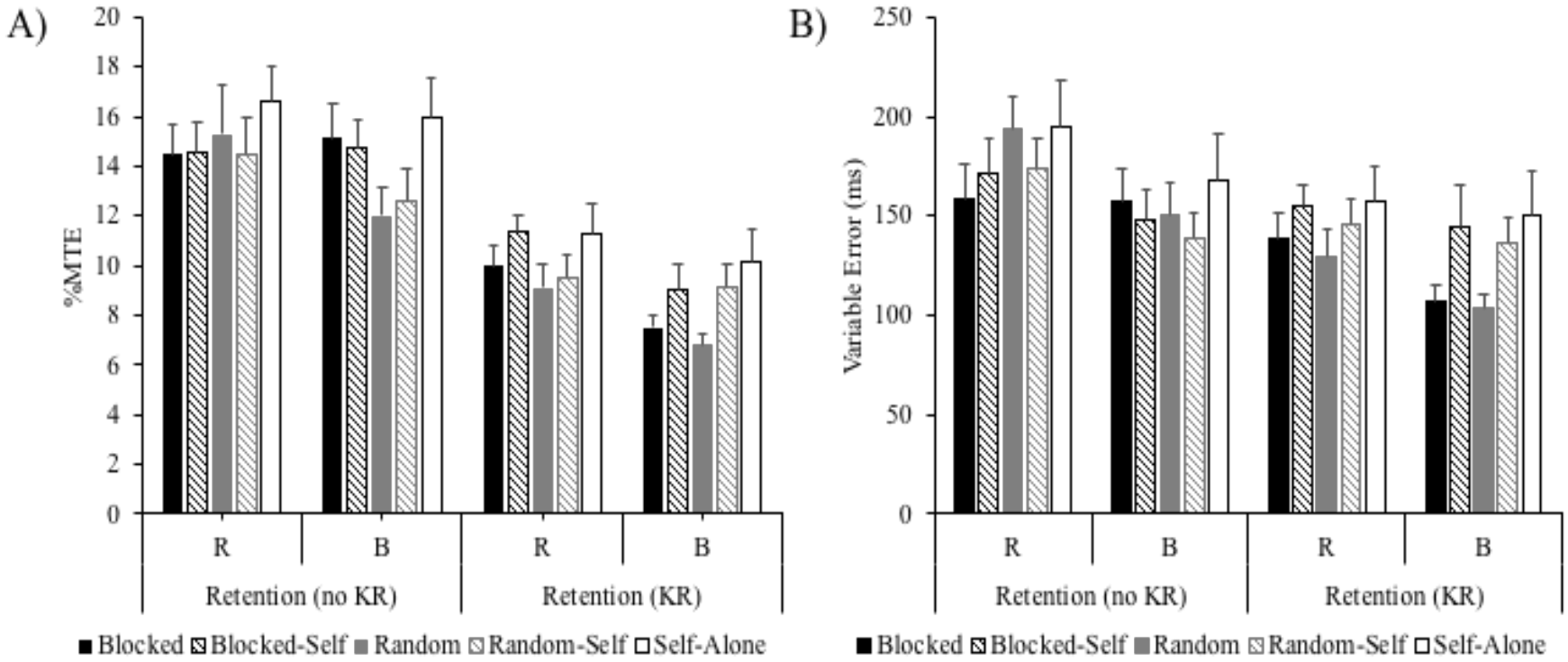
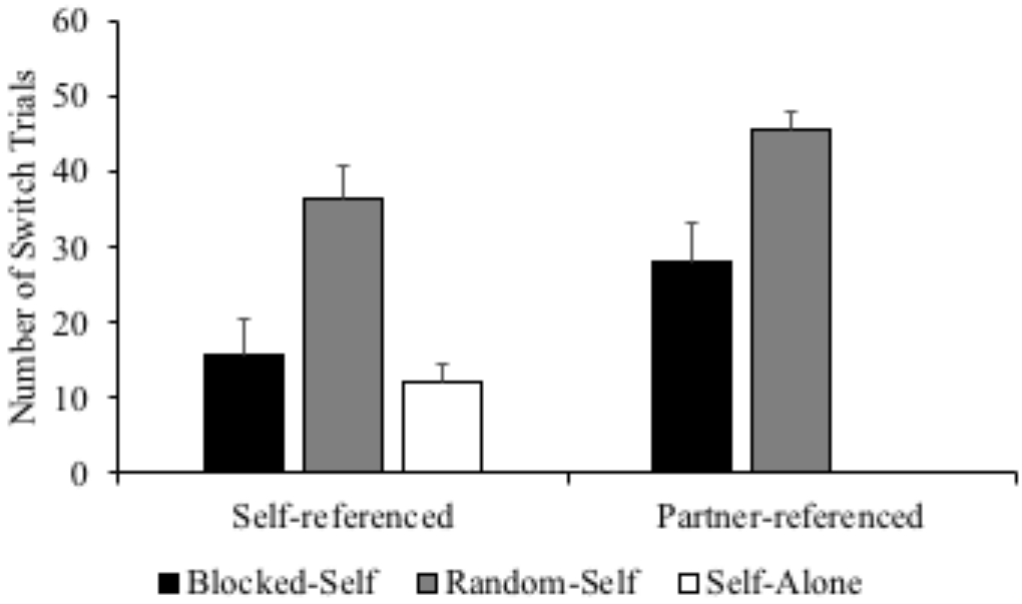


Figure 17. Mean number of trials (and *SE* bars) where self-controlled participants switched to a different sequence with reference to their own or their partner's previous trial.



6.3.2.1.3 Relations between switching and retention outcomes

As shown in Table 10, self-referenced task-switching was generally weakly correlated with %MTE on the delayed retention tests. The one exception was an unexpected positive correlation between the blocked-self group's number of self-referenced switch trials and %MTE on the final blocked-order/KR retention test. Switching more often was related to higher error, $r(15) = .37$, $p = .17$, but it was not statistically significant (all other rs < .30).

There were also generally weak correlations between the frequency of partner-referenced task-switching and %MTE (see Table 10). Although switching in reference to a partner's blocked practice (i.e., mismatching) was related to *lower* error on the random-order/no-KR test (blocked-self: $r(15) = –.39$, $p = .15$), mismatching led to *higher* error for this blocked-self group on the final, blocked-order/KR retention test, $r(15) = .45$, $p = .09$ (all other rs < .30). The

predetermined partners tended to benefit from (involuntarily) mismatching their partner's preceding trial. Mismatching was related to lower error on the random-order/no-KR test for both the blocked-schedule partners, $r(15) = -.31$, $p = .27$, and the random-schedule partners, $r(16) = -.50$, $p = .048$. The benefits of mismatching persisted for the random schedule group on the blocked-order/no-KR test, but was not significant, $r(16) = -.41$, $p = .12$ (all other rs < .30).

Table 10. Correlations between self- and partner-referenced task-switching frequency and mean percentage movement time error (%MTE) in retention.

Retention test	Blocked	Blocked-Self	Random	Random-Self	Self-Alone
Self-referenced switching					
Random-order/no-KR		-.19		-.14	-.12
Blocked-order/no-KR		-.15		-.27	-.11
Random-order/KR		.05		-.15	-.20
Blocked-order/KR		**.37**		.14	-.23
Partner-referenced switching					
Random-order/no-KR	**-.31**	**-.39**	**-.50***	.29	
Blocked-order/no-KR	-.19	-.26	**-.41**	.21	
Random-order/KR	.02	.14	-.09	.14	
Blocked-order/KR	-.24	**.45***	-.16	.25	

Note. Correlations of medium effect size or greater are bolded ($r > .30$; Cohen, 1992). Significant correlations ($p < .05$) are indicated with an asterisk (*).

6.3.2.2 Task-switching strategy

6.3.2.2.1 Comparisons across the dyads

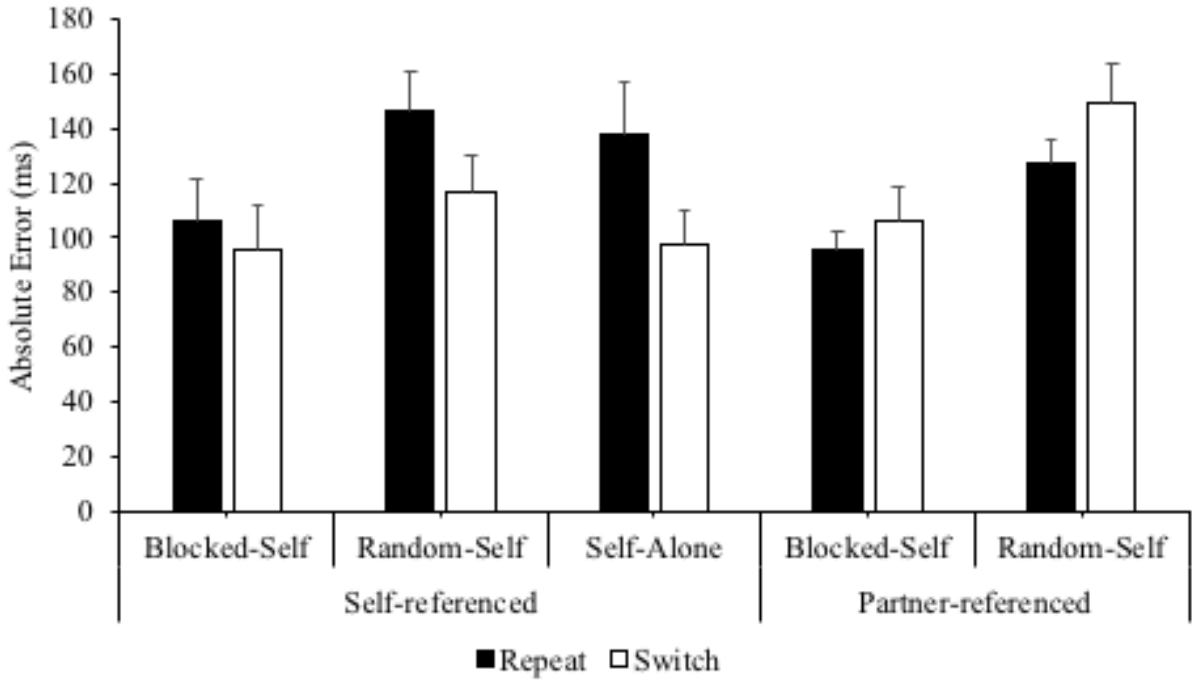
Consistent with the strategy previously observed in individual learners, self-directed participants chose to repeat the same task if they performed relatively poorly (i.e., higher error) and to switch to a different task if they performed relatively well. In contrast, they tended to repeat (match) their partner's task if it was performed relatively well and to switch to a different task (mismatch) if their partner performed relatively poorly. These data are illustrated in Figure 18 and the effects were confirmed by a significant Reference (self or partner) × Trial-type (repeat or switch) interaction, $F(1, 26) = 5.55$, $p = .026$, $\eta_p^2 = .18$ (but no main effects for either reference or trial-type).

Although the 3-way interaction with Group was not significant, $F(1, 26) = 2.95$, $p = .10$, $1 - ß = .38$, as seen in Figure 18, differences between repeat and switch trials were more salient for the random-self than blocked-self participants. Simple, within-group pairwise comparisons showed that the random-self group tended to exhibit performance-contingent switching with reference to their own performance, $t(15) = 2.83$, $p = .013$, $d = 0.53$, as well as with reference to their partner's, $t(15) = –1.88$, $p = .079$, $d = 0.48$. The blocked-self group did not show differences between their repeat or switch trials, in reference to their own performance, $t(14) = .46$, $p = .65$, $d = 0.17$, nor in reference to their partner's, $t(11) = –.85$, $p = .41$, $d = 0.28$.

6.3.2.2.2 Comparisons between the alone and dyad self-control groups

There were no group-related effects when comparing across all self-control groups (all Fs < 1.53). In general, participants chose to switch tasks if they performed relatively well (low error), rather than repeat, $F(1, 44) = 7.54$, $p = .009$, $\eta_p^2 = .15$.

Figure 18. Absolute error (and *SE* bars) for trials on which self-controlled participants chose to repeat the same sequence ("repeat" trials) or to switch to a different sequence ("switch" trials) with reference to their own or their partner's previous trial.



6.3.2.3 Effects of a partner's demonstrations on performance

We assessed whether observing the same (matched) or different (mismatched) sequence before a practice attempt impacted timing error on the next trial. Overall, matched demonstrations led to lower error (M = 9.6%, SD = 3.3%) compared to mismatched demonstrations (M = 10.7%, SD = 4.2%), $F(1, 55) = 4.73$, $p = .034$, $\eta_p^2 = .079$. This did not depend on practice schedule, $F(1, 55) = 1.78$, $p = .19$, $1 - ß = .26$, or partner-type ($F < 1$). As reported above, the blocked-schedule dyads generally performed with lower error in practice compared to the random-schedule dyads, $F(1, 55) = 4.09$, $p = .048$, $\eta_p^2 = .069$, but there were no partner-related effects or interactions (Fs < 1.1).

6.3.2.4 Questionnaires

6.3.2.4.1 Intrinsic Motivation Inventory

Responses to the IMI are presented in Table 11. Comparing across the dyads first, regardless of Schedule or Partner grouping, a partner's competence was rated higher (M = 5.0, SD = 0.9) than self-competence (M = 4.2, SD = 1.1), $F(1, 57) = 28.42$, $p < .001$, $\eta_p^2 = .33$. There was no Schedule effect, $F(1, 57) = 1.80$, $p = .19$, 1 – ß = .26, nor any other main effects or interactions (Fs < 1). Participants moderately enjoyed the task and reported relatively low perceptions of pressure/tension, regardless of Schedule or Partner-type (Fs ranged from < 1–1.2).

Compared to practice alone, practice with a partner did not significantly modulate self-controlled learners' perceived competence ($F < 1$), interest/enjoyment, $F(2, 44) = 2.62$, $p = .084$, 1 – ß = .50, nor their perceptions of pressure/tension during practice, $F(2, 44) = 2.16$, $p = .13$, 1 – ß = .42. As apparent from the means in Table 11, the Alone group had the highest value for interest/enjoyment and the lowest value for pressure/tension.

Table 11. Mean ratings (and *SD*s) for the Intrinsic Motivation Inventory subscales and customized partner-related competence subscale.

Subscale	Blocked	Blocked-Self	Random	Random-Self	Self-Alone
Competence					
Own	4.3 (1.3)	4.3 (1.0)	3.9 (0.7)	4.2 (1.1)	4.2 (1.1)
Partner	5.1 (0.7)	5.3 (0.9)	4.9 (0.9)	4.9 (1.1)	N/A
Interest/enjoyment	3.3 (1.5)	4.2 (1.4)	3.6 (1.3)	3.4 (1.4)	4.5 (1.5)
Pressure/tension	3.4 (1.4)	3.2 (1.5)	3.0 (1.3)	3.2 (1.5)	2.3 (1.1)

Note. Scales ranged from 1–7.

6.3.2.4.2 Dyad practice experience questionnaire

These data and results are presented in Table 12. Ratings were generally high in response to whether watching a partner helped their own performance/learning (4.9–5.7/7). This was not dependent on practice schedule, $F(1, 58) = 2.13$, $p = .15$, $1 - ß = .30$, and there were no partner-related effects or interactions (Fs < 1). Similarly, low ratings were given for the question as to whether watching a partner was interfering for their own performance/learning. This time there was a Schedule effect; the blocked pairs thought having a partner was more interfering ($M = 3.9$, $SD = 1.7$) than the random pairs ($M = 2.8$, $SD = 1.5$), $F(1, 58) = 7.08$, $p = .010$, $\eta_p^2 = .11$. This did not vary as a function of partner, $F(1, 58) = 2.17$, $p = .15$, $1 - ß = .31$, and there was no interaction ($F < 1$). Overall, participants reported a moderate to high desire to be more accurate than their partner (4.5–5.9). An effect of Schedule revealed that the random pairs ($M = 5.7$, $SD = 1.2$) experienced a greater desire to outperform their partner than the blocked pairs ($M = 4.9$, $SD = 1.8$), $F(1, 58) = 4.03$, $p = .049$, $\eta_p^2 = .065$. There was no effect of Partner, $F(1, 58) = 2.44$, $p = .12$, $1 - ß = .34$, nor an interaction ($F < 1$). Scores were in the midpoint range for whether dyad participants would have preferred to practice alone (suggestive of no preference) and this did not vary as a function of Schedule ($F < 1$) or Partner, $F(1, 58) = 3.02$, $p = .087$, $1 - ß = .40$, and there was no interaction, $F(1, 58) = 1.33$, $p = .25$, $1 - ß = .21$.

Table 12. Mean ratings (and *SD*s) to customized dyad practice experience questionnaire.

	Blocked	Blocked-Self	Random	Random-Self
1. Watching my partner helped my own performance	4.9 (1.5)	5.0 (1.4)	5.7 (1.3)	5.3 (1.6)
2. Watching my partner interfered with my own performance	3.5 (1.7)	4.2 (1.7)	2.6 (1.3)	3.1 (1.6)
3. I wanted to be more accurate than my partner	4.5 (1.7)	5.2 (1.9)	5.4 (1.3)	5.9 (1.2)
4. I would have preferred to practice alone	4.5 (2.1)	5.2 (2.1)	5.1 (1.5)	4.9 (1.4)

Note. Scales ranged from 1–7.

6.3.2.4.3 Self-controlled practice questionnaire

We were interested in how self-controlled learners thought their practice scheduling decisions were influenced by their partners. The self-controlled participants tended to rate the influence of their partner's practice (task-switching) schedule on their own task-switching decisions higher than the influence of how well their partner performed ($M = 4.5$, $SD = 2.1$ vs. $M = 3.6$, $SD = 2.0$), $F(1, 29) = 3.90$, $p = .058$, $1 - ß = .48$. This did not depend on Group ($F < 1$). Although the interaction was not significant, $F(1, 29) = 3.90$, $p = .058$, $1 - ß = .48$, the behavioural data suggested that it was only the blocked-self partners who perceived themselves to be more greatly influenced by their partner's practice schedule ($M = 4.7$, $SD = 2.3$) than by their partner's performance success ($M = 2.9$, $SD = 2.2$). In contrast, the random-self group perceived themselves to be similarly influenced by both partner-related factors (schedule: $M = 4.3$, $SD = 2.0$; performance: $M = 4.3$, $SD = 1.7$).

Frequency of responses to the remaining questionnaire items are presented in Table 13. No statistical analysis was conducted on these data. Consistent with the task-switching strategies observed in the behavioural data, more self-controlled learners (64%) reported a preference to repeat the same sequence on their next turn following their own poor(er) performance. Fewer participants (49%) indicated they would repeat the same sequence if they performed well, although this was still nearly half the sample. A performance-contingent switching strategy was most clearly expressed by the self-alone group (75% would repeat following high error), which was not exposed to any partner-related influences. Although the behavioural data suggested that the self-controlled partners preferred to repeat a partner's sequence when it was performed with relatively low error, there was little indication of a preference regarding when to match/repeat (blocked-self, 53% and random-self, 50%).

Table 13. Frequency of responses to questions assessing self-controlled practice choices.

	Blocked-Self	Random-Self	Self-Alone
1. If I did well on a trial practicing a certain pattern, on my next turn I would choose			
(A) The same pattern again	8	7	8
(B) A different pattern	2	5	4
(C) Random	3	2	2
(D) Other	2	2	2
2. If I did poorly on a trial practicing a certain pattern, on my next turn I would choose			
(A) The same pattern again	10	8	12
(B) A different pattern	1	4	2
(C) Random	2	0	1
(D) Other	2	4	1
3. If my partner did well on a trial practicing a certain pattern, on my next turn I would choose			
(A) The same pattern again	8	8	
(B) A different pattern	2	2	
(C) Random	2	2	
(D) Other	3	4	
4. If my partner did poorly on a trial practicing a certain pattern, on my next turn I would choose			
(A) The same pattern again	8	8	
(B) A different pattern	3	2	
(C) Random	2	4	
(D) Other	2	2	

6.4 Discussion

We studied self-controlled practice organization in the context of dyad learning, to determine if and how a partner's practice impacted self-controlled learners' practice choices, performance and learning outcomes, as well as perceptions of the practice experience compared to individual practice. Building on a previous study where partners alternated turns after 9-trial blocks of practice (Karlinsky & Hodges, 2018a), partners now alternated turns on consecutive

trials, to provide insight into the immediate impact of a peer's practice (both the task practiced and their success) on self-controlled practice behaviours.

6.4.1 Influence of a Partner on Self-controlled Practice Organization

Practice with a random-schedule partner promoted self-controlled task-switching, such that learners incorporated greater CI into their practice compared to practice with a blocked-schedule partner or practice alone. This finding is consistent with previous research showing that exposure to random practice increases the task-switching learners choose to undertake, compared to exposure to lower CI blocked and self-directed practice schedules (Hodges et al., 2014; Karlinsky & Hodges, 2018a). Moving forward, it will be important to study what other aspects of a co-learner's practice influence self-controlled behaviours, such as practice duration (e.g., Post, Fairbrother, & Barros, 2011), changes in task difficulty (e.g., Andrieux, Danna, & Thon, 2012), and requests for augmented feedback (e.g., Chiviacowsky & Wulf, 2002). This is an intriguing avenue for future research considering that learners do not always know which conditions of practice are most effective for learning (e.g., Simon & Bjork, 2001, 2002). Strategic exposure to desirable practice principles could potentially encourage learners to adopt beneficial behaviours (e.g., more variable practice, less frequent augmented feedback, etc.), without compromising their sense of autonomy and its putative motivational benefits for motor learning (for reviews, see Lewthwaite & Wulf, 2012; Wulf & Lewthwaite, 2016).

Across groups, self-controlled learners chose to switch tasks with reference to the partner's preceding trial more often than with reference to their own previous turn. A mismatch between what is observed and what is practiced on interleaved trials has previously been shown to enhance learning, when individual learners watched modeled demonstrations of perfect task performance (Lee et al., 1997; Simon & Bjork, 2002). This led to the hypothesis that trainees in

applied settings, where inter-trial delays are often imposed (e.g., due to sharing equipment or physiological demands), would benefit more from watching a co-learner perform a different skill from the one they were about to perform, rather than the same (Simon & Bjork, 2002). Our data provide some support for this idea that the benefits of increasing CI via interspersed observational practice extend to situations where demonstrations are provided by a co-learner. This is evidenced by the negative correlations between partner-referenced task-switching and retention outcomes (although a significant correlation was only shown for the random partners on the random-order/no-KR retention test). We have some evidence that even for experimenter-imposed blocked and random subjects who had to match/mismatch what they observed, that mismatching was related to lower error on retention tests. This is in contrast to our previous study, where there were no significant differences between pairs who practiced golf putting skills under imposed matched versus mismatched schedules (Karlinsky & Hodges, in review). We are currently testing fully matched/mismatched-schedule control groups for the current keystroke sequences, to elucidate the effects of partner-referenced switching for these more simple tasks and to further contextualize the implications of learners' decisions to match or mismatch a peer's practice.

In terms of task-switching strategy, these data provide a first comparison of self- vs. peer-referenced switching (i.e., when learners chose to repeat (match) vs. switch (mismatch) tasks following their own errors or those of a peer's). Consistent with previous findings, self-controlled learners tended to adopt a performance-contingent switching strategy with reference to their own performance, such that they repeated the same task again if they performed poorly and switched to a different task if they performed well (e.g., Hodges et al., 2014; Karlinsky & Hodges, 2014, 2018a; Wu & Magill, 2011). With respect to a partner's performance, learners

instead tended to repeat what was observed if the partner provided a relatively successful (low error) model of performance, and to switch if the demonstration was relatively poor. Thus, learners appear to monitor a partner's performance and take this into consideration when making decisions in shared practice contexts. Given that whether or not partners adapt their behaviour in response to a peer's performance can differ under cooperative vs. competitive conditions (e.g., de Bruijn et al., 2008, 2012), of future interest is to manipulate the competitive nature of the dyad learning context, to further our understanding of when and why shared practice conditions elicit certain behaviours.

The finding that self-controlled learners displayed error-dependent task-switching in social contexts is important, considering that engaging in self-evaluation and performance appraisals is thought to be integral to the benefits of self-controlled practice (e.g., Carter et al., 2014; Chiviacowsky & Wulf, 2005). Having said this, however, performance-contingent switching was not as clearly exhibited by individuals paired with a blocked-schedule partner. This was somewhat surprising, as we previously showed that self-controlled learners maintained an error-dependent style of task-switching in paired practice contexts (Karlinsky & Hodges, 2018a). Whereas the mean difference between repeat and switch trials for the current blocked-self group was only ~10 ms, we previously observed this difference to be ~19 ms when self-controlled learners alternated blocks of practice with a blocked-schedule partner (Karlinsky & Hodges, 2018a). It is likely that the current dyad practice paradigm, where partners alternated turns on consecutive trials, exerted a stronger influence on decision-making than our previous block-to-block turn-taking manipulation. In the latter protocol, learners could focus solely on their own performance for a set of 9 trials (see Karlinsky & Hodges, 2018a). Indeed, watching a

partner's practice was rated as more interfering by the current blocked-self group (4.2/7) than by the blocked-self group previously studied (2.3/7; see Karlinsky & Hodges, 2018a).

6.4.2 Influence of a Partner on Motor Performance and Multi-skill Learning

In addition to influencing practice behaviours, observing a partner's practice also impacted immediate motor performance. "Matched" demonstrations (i.e., that modeled the next to-be-performed task) facilitated performance in practice compared to "mismatched" demonstrations, consistent with previous research based on individual learners (Lee et al., 1997; Simon & Bjork, 2002). Of note, we did not previously observe matched demonstrations to facilitate (or mismatched demonstrations to impede) immediate motor performance in alternating practice of golf putting skills (Karlinsky & Hodges, in review). It is possible that this difference in results is linked to the sensitivity of the experimental tasks to contextual interference, with the current relatively simple keystroke tasks yielding more prototypical effects of task repetition than the relatively complex putting skills (Wulf & Shea, 2002).

In terms of retention performance, the data provide some evidence that the CI effect extends to dyad practice, as the random pairs exhibited higher error and variability in acquisition but lower error in the blocked-order/no-KR retention test compared to the blocked pairs (although they did not outperform the blocked pairs on the other tests; see also Karlinsky & Hodges, 2018a). However, comparisons between the dyad and individual self-controlled groups did not reveal any benefits (or costs) of practice with a partner, despite the partner clearly impacting practice-related behaviours. Why, then, did this modulated behaviour not ultimately translate into modulated learning? It is possible that the switching frequency differences were not significant enough to yield behavioural differences when considered on a group level. There were also no significant negative correlations between self-controlled task-switching and error in

retention. Although the two extreme practice conditions; blocked and random, usually yield learning-related outcome differences, the retention outcomes associated with learner-controlled task-switching are less clear, likely because switching is adjusted to the performance of the individual (e.g., Hodges et al., 2014; Karlinsky & Hodges, 2014, 2018a; Wu & Magill, 2011).

Despite the fact that dyad practice was not more effective than individual practice with respect to retention, dyad practice can still afford efficiencies in training time (e.g., two people can be trained in the time otherwise devoted to one, by practicing during one another's breaks). Such efficiencies may be particularly beneficial in applied settings where there are enforced delays between physical practice attempts, such as in physiologically demanding activities, when there are fewer instructors than trainees, or when equipment must be shared.

6.4.3 Influence of a Partner on Perceptions of Practice

In addition to the behavioural processes and outcomes of dyad practice, we were also interested in the subjective experiences associated with practice with a partner and the opportunity to control one's own practice schedule. Across groups, participants rated their partner's competence as higher than their own, despite there being no partner-related effects in timing accuracy or variability during the acquisition session (see also Karlinsky & Hodges, 2018b). Whether this truly indicates participants perceived themselves to be less competent at the task than a partner or reflects perhaps that individuals generally hold higher standards for their own versus a peer's performance remains to be clarified. It is possible that if pairs practiced with a collaborative goal, perhaps in competition with other dyads, that individuals' standards for a partner's performance and associated perceptions of their competence would be affected. Considering learners often have to make judgments about their own proficiency (contributing to decisions about what and how much to practice, to seek further instruction, etc.; Simon & Bjork,

2002), it will be important for future research to consider how such self-assessments and associated decisions might be affected by social comparisons in shared practice settings.

Learners' perceptions of how a partner influenced their own performance/learning varied between dyad groups. Blocked pairs perceived watching a partner's interleaved practice as more interfering than random pairs. We speculate that experience dealing with contextual interference oneself might mitigate against perceiving a co-learner's practice as disruptive to one's own performance and learning. Indeed, correlations between self-controlled learners' task-switching frequency and their ratings of how interfering and helpful watching a partner's practice was offers preliminary support for this conjecture: More frequent task-switching was associated with lower ratings of a partner's interference, $r(31) = -.33$, and higher ratings of their helpfulness, $r(31) = .43$, towards participants' own performance and learning.

In summary, we have provided evidence that practicing in a social context can impact self-controlled practice behaviours and that these effects depend on the partner's practice schedule. Considering the pervasiveness of social motor learning settings (e.g., physical education classes, team sports), and the training time and financial savings that can be afforded by shared practice conditions (e.g., Shea et al., 1999), it will be important to continue such inquiries into when and why learners are susceptible to the behaviours of their peers, and how such factors can be harnessed to optimize practice and learning.

Chapter 7: General Discussion

In this dissertation, I presented five studies which have extended the breadth and depth of the dyad learning literature by introducing novel forms of dyad practice and providing the first tests of dyadic multi-skill learning. In the following sections, I will provide a synthesis of my findings and elaborate on the conceptual contributions of this research program to the field of motor learning. These contributions will be outlined in terms of how they inform our understanding of the efficiency and effectiveness of dyad practice, as well as how a co-learner can impact an individual's practice behaviours and perceptions of practice. I will then revisit the potential factors impacting dyad practice elucidated in my general introduction with respect to the studies presented herein. Finally, I will conclude by considering future dyad learning-related research directions, specifically with regards to issues of task difficulty and performer skill level, alternative forms of dyadic task sharing, scaling up dyad learning to larger groups, and the generalizability of dyad learning to novel partners.

7.1 Synthesis of findings

7.1.1 Concurrent whole-task practice versus alternating practice of a single skill

In Experiment 1, I replicated and extended a previous study (Shea et al., 1999), to test the effects of peer observation 'in-between' versus 'during' physical practice (Karlinsky & Hodges, 2018b). The results supported prior findings that alternating practice can enhance the efficiency of practice, as observing a partner during rest intervals enabled two people to be trained in the time otherwise devoted to one without any costs (e.g., Granados & Wulf, 2007; Shea et al., 1999, 2000). These efficiency benefits were also true of my novel concurrent practice paradigm, where co-learners simultaneously physically practiced and observed one another. Although concurrent observation was perceived to be more interfering than alternating practice and brought about

movement coupling within pairs, this perceived interference was not to the detriment (or benefit) of motor performance or learning. Moreover, neither dyad protocol yielded superior learning compared to pure physical practice. There were trends, however, for the dyad practice groups to outperform the alone group during testing, suggesting that the study may not have been powered enough to detect statistically significant group differences. Overall, these data showed that a co-learner can have an immediate effect on practice behaviours and influence perceptions of performance, but not that co-learners impact motor learning outcomes.

7.1.2 Alternating practice of multiple skills

In the remainder of the research presented in my dissertation, I studied questions related to multi-skill dyad learning, which had not been addressed prior to this research program. An important factor for how effectively multiple skills are learned is the order, or task-switching schedule, in which they are practiced. This order can be determined in a variety of ways, including experimenter-directed (e.g., Shea & Morgan, 1979), self-directed (e.g., Hodges et al., 2014), or algorithmically-directed schedules (e.g., Simon et al., 2008). In Experiment 3, I introduced a novel means of practice organization: peer-directed practice (Karlinsky & Hodges, 2014). In general, practice schedules which expose learners to greater "contextual interference" (CI) have been shown to hinder performance in practice, but enhance longer-term learning. The degree of CI can be manipulated by altering the frequency and predictability of task-switching, with blocked (repetitive) and random (unsystematic) practice schedules anchoring the CI spectrum. However, these principles of CI have been derived based on research involving experimenter-controlled schedules and individual learners. In Experiments 2–5 I evaluated how such CI principles hold up under dyad and learner-controlled practice conditions.

7.1.2.1 Experimenter-directed practice schedules

For my first foray into dyadic multi-skill learning (Experiment 2), I employed experimenter-directed practice schedules, which allowed me to control the level of within-person CI individuals experienced and manipulate the degree of interference via a partner's interspersed practice trials. To do this, partners either matched or mismatched the skill practiced by their partner, to minimize or increase the degree of CI experienced, respectively. Prior dyad learning research has not been able to speak to such content-specific effects of a partner's demonstrations on subsequent performance, as there has always been a "match" between what is practiced and what is observed in the extant single-skill paradigms (e.g., Granados & Wulf, 2007; Shea et al., 1999, 2000). The underlying assumptions in Experiment 2 of the present work were that, i) the experimental tasks (two golf putting skills) were sensitive to traditional within-person CI such that the skills would benefit from a higher degree of interference, and ii) this increase in CI could be achieved by physical and/or observational practice. In hindsight, a limitation of this study is that I did not include individual low- versus high-CI control groups, nor a no-practice control group, to test the sensitivity of the golf putting tasks to within-person CI and the efficacy of observational practice, respectively. The inclusion of such control groups is needed in future studies, to juxtapose the effects of between-person CI manipulations. Nevertheless, this study provided insight into another way that co-learners can impact practice behaviours (beyond the coupling observed in Experiment 1). Specifically, participants appeared to compensate for both self- and partner-produced errors, such that both one's own or the partner's overshooting (or undershooting) of the target led to shorter (or longer) putts on the subsequent trial. However, in terms of learning outcomes, alternating dyad practice (where partners took turns observing and

practicing) did not modulate retention compared to pure physical practice (where a passive partner just watched), thus replicating the behavioural results from Experiment 1.

7.1.2.2 Learner-directed practice schedules

In Experiment 3, I provided the first test of peer-directed practice. This paradigm was conceived as a tool to address debate in the motor learning literature, between information processing- versus motivation-driven arguments for the benefits of self-controlled learning. I was specifically interested in whether the learning advantages associated with self-directed practice schedules are potentially related to the cognitive activities surrounding decision making and individualized practice schedules, as opposed to more general motivation-related benefits associated with the opportunity to exercise control. I suggested that peer-directed practice could serve as an alternative means of providing learners with customized practice schedules, in the absence of autonomy-supportive control over one's own practice schedule (Karlinsky & Hodges, 2014). Indeed, both self- and peer-schedulers designed practice in a performance-contingent manner, choosing to switch tasks following relatively good (low error) performance (see also Hodges et al., 2014; Keetch & Lee, 2007; Wu & Magill, 2011). Considering that the self- versus peer-directed learners did not differ in retention and that the peer-directed pairs reported higher perceptions of motivation and enjoyment than the self-directed pairs, these data supported an information-processing interpretation of self-controlled learning more so than a motivation-driven one. As would be consistent with the latter mechanism, autonomy over practice-related decisions did not influence motor learning.

Having observed the practice scheduling strategies adopted by self-directed learners in the presence of a resting observer, in my final two experiments I studied whether and how self-directed practice decisions might be susceptible to the practice behaviours of a fellow performer.

The results of Experiments 4 and 5 showed that self-directed task-switching frequency is modulated by a partner's practice schedule. Of note, task-switching frequency was only affected when a partner followed a high-CI random practice schedule, which promoted self-directed task-switching. In contrast, practice with a blocked-schedule partner did not alter self-directed learners' task-switching frequency compared to control levels. These findings complement previous research showing that *personally* experiencing random practice leads learners to later adopt higher CI practice (Hodges et al., 2014), and add that *vicariously* experiencing random practice in a dyad learning context, also encourages learners to bring CI into their own training (Karlinsky & Hodges, 2018a).

Self-directed learners generally upheld the performance-contingent nature of their task-switching (i.e., choosing to switch tasks following relatively good performance) in Experiment 4, despite the external influences presented by the paired learning settings (Karlinsky & Hodges, 2018a). These findings were also supported in Experiment 5, where partners alternated turns on a trial-to-trial basis to determine the immediate impact of a partner's demonstration on subsequent task selection and performance. The trial-to-trial alternation in Experiment 5 futher revealed that learners tended to show a different switching strategy with respect to their partner's errors. In this case, learners tended to repeat a partner's task when it was performed relatively well (i.e., provided a good demonstration) but not when it was performed relatively poorly. This difference in self- versus partner-related switching strategy provides an interesting example of how learners do not necessarily choose to respond to others' and their own errors in the same way. In contrast, when learners followed imposed practice schedules in Experiment 2, they showed similar corrective behaviours to both self- and peer-generated errors. It would be of value in future work to test learner-control of other practice variables (e.g., KR requests) within alternating dyad

practice, to futher untangle those practice behaviours that are immune to a partner's performance and those which are susceptible to modulation.

7.2 Conceptual contributions

The results from this dissertation offer several contributions to our knowledge and understanding of the processes and outcomes associated with dyad practice conditions. These include support for the efficiency, but not the effectiveness, of dyad practice compared to pure physical practice and provide evidence of the susceptibility of self-directed practice decisions to observed behaviours. Additionally, the approaches used to obtain these results represent methodological contributions to the field, serving as the first implementations of concurrent whole-task practice, multi-skill dyad practice, and peer-directed practice protocols, while the questionnaires administered also bring unique empirical attention to learners' perceptions of paired practice experiences. Thus, the studies presented herein contribute both to the breadth and depth of the extant dyad learning literature.

7.2.1 On the efficiency of dyad practice

In general, this dissertation supports previous reports of the efficiency of dyad practice (e.g., Granados & Wulf, 2007; Shea et al., 1999; Shebilske et al., 1992). As efficiency can be operationalized in a number of ways, here I refer to the efficiency of dyad practice as decreased training time and resources compared to individual practice. That is, training conditions would be deemed efficient if two individuals were trained to at least the same level of proficiency an individual would achieve in the same amount of time (Shebilske et al., 1998). Such efficiency gains were observed in Experiment 1, where both alternating and concurrent practice of a balance-related task yielded similar transfer performance as practice alone. So too did Experiment 2's matched and mismatched alternating practice of golf putting skills confer

equivalent retention results as individual practice, as did Experiment 5's alternating practice of keystroke sequences compared to self-directed practice alone.

Of note, I did not test whether dyad practice can confer efficiencies in terms of absolute training time. Specifically, I imposed breaks within individuals' practice sessions such that they equalled the duration of the corresponding dyad practice sessions. On the one hand, it is possible that individuals did not in fact need so much rest and could have completed the training in less time. On the other hand, it is possible that the individual learners benefited from these imposed rest intervals (i.e., distributed practice; Baddeley & Longman, 1978), and that they would not have learned to the same extent from more massed practice conditions (I discuss distributed practice in more detail below). Furthermore, I also did not test whether dyad practice affords efficiencies in terms of decreased practice trials. Specifically, in all my studies, each member of the dyad always received the same number of physical practice trials as their individual counterparts. Yet, there is some evidence that physical practice can be reduced when practicing with a peer (e.g., Shea et al., 2000; Shebilske et al., 1992). Determining the conditions under which one person's practice can replace some need for practice in another is an exciting avenue for future research, which would be particularly important for rehabilitation settings, as well as for training situations where there are high physiological demands, training-related expenses, or limited training time.

7.2.2 On the effectiveness of dyad practice

The opportunity to switch between practicing and observing a partner has previously been reported to be more effective than pure physical practice (e.g., Granados & Wulf, 2007; Shea et al., 1999, 2000). However, the consistent equivalence in outcomes of alternating and pure physical practice across four experiments reported herein clearly indicates that the

advantages of alternating practice cannot be taken for granted. That is not to say that dyad practice cannot still confer benefits (for example in terms of efficiency, as discussed above), but rather that much remains to be learned about when and why dyad practice enhances learning above and beyond physical practice alone. Study-specific considerations regarding this issue can be found in their respective chapters, with what I consider to be key considerations moving forward delineated below.

First, Experiment 1 highlighted the importance of considering how procedural details might alter the amount and type of information available to be picked-up via observation, and ultimately mediate the benefits of dyad practice (Karlinsky & Hodges, 2018b). Constraining individuals' fixation point and stance on the stabiliometers during practice of a balance-related task might have diminished potential benefits of dyad practice associated with opportunities to test a wider range of techniques and strategies (*cf.* Shea et al., 1999). Second, for investigations involving multi-skill learning, determining the sensitivity of the experimental tasks to contextual interference is warranted. Establishing effects on an individual level prior to or alongside testing dyad conditions would help to strengthen conclusions as we build our understanding of multi-person motor learning. Third, the effectiveness of dyad practice will also need to be considered against a backdrop of other practice variables. That is, the opportunity to observe and interact with a co-learner may be more or less likely to enhance learning depending on other aspects of the practice conditions (e.g., experimenter- versus self-directed practice). Much remains to be learned about the unique versus additive benefits of practice variables within dyad contexts.

Moving forward, a priori power analyses will be necessary in making conclusions about the effects of dyad practice conditions on learning outcomes. This is particularly important given the amount of between-participant variability that tends to be observed in learning studies. It is

possible that the current studies may not have been powered enough to detect differences in outcome measures and/or that between-group effects in prior research may be a result of existing individual differences. Well-powered replications will be important in making conclusions about benefits (or costs) associated with dyad practice in comparison to alone conditions. The lack of any statistical differences between the pure physical practice groups (who practiced alone) and dyad pairs, coupled with no apparent trends for dyads to do worse in any retention or transfer test bodes well for applied learning settings. Whereas the presence of co-learners could be perceived as distracting and a hinderance to learning, there was no evidence that the presence of a co-learner negatively impacted performance during practice nor longer-term retention and transfer.

7.2.3 On the influence of a partner on practice behaviours

Throughout this dissertation, I sought to better understand not just the outcomes, but also the processes and mechanisms associated with dyad practice conditions. Measures of partner-related practice behaviours yielded several novel findings. Experiment 1 revealed that concurrent action observation modulated online motor performance, eliciting greater coupling between partners compared to pairs who did not practice simultaneously (Karlinsky & Hodges, 2018b). These results extend previous findings of online motor contagion in unidirectional "partnerships" (between an experimenter or videoed model and participant; e.g., Kilner et al., 2003; Sebanz & Shiffrar, 2007; Tia et al., 2011, 2012) to veridical, bidirectional pairings.

Experiment 2 indicated that peer observation does not need to be online (i.e., concurrent with actual physical practice) to impact performance. Alternating dyads displayed both self- and partner-referenced compensatory behaviours, attempting to correct for their own and/or their partner's errors with similar frequency (Karlinsky & Hodges, in review). These findings complement previous evidence of "post-error slowing" in dyads (where people tend to slow

down following their own or a co-actor's error in reaction time tasks; e.g., de Bruijn et al., 2011, 2012) and provide initial evidence of dyadic "post-error compensation" (where people attempt to correct for their own or a co-actor's outcome errors).

A partner can also influence a co-learner's behaviours with what they say, rather than what they do. This was the case in Experiment 3, where a partner directed a co-learner's practice schedule (Karlinsky & Hodges, 2014). The peer-schedulers incorporated significantly more contextual interference into the actor's practice than self-directed learners chose for themselves. Importantly, however, the peers were similar to the self-directed learners in their error-dependent task-switching strategy. Despite the arguably more difficult practice schedule imposed by the peer-schedulers, the peer-directed practice condition was perceived as more motivating and enjoyable than the self-directed practice condition by both members of the pair. Peer schedulers and self-directed learners have also been shown to differ in their requests for KR, but once again these peer-directed conditions were positively received (McRae, 2015; McRae et al., 2015). Thus, although peers might not instigate the same practice behaviours learners choose for themselves, this is not to the detriment of skill acquisition.

Finally, Experiments 4 and 5 provided evidence that a partner's practice schedule modulated the behaviours of self-directed learners. A random-schedule partner increased learners' chosen task-switching frequency, compared to a blocked-schedule partner, self-directed partner, and no-observation conditions. These results highlight the value of curating what learners are exposed to during social practice conditions, as a means of encouraging learners to adopt desirable practice behaviours (in this case, higher CI) whilst preserving their opportunity to experience autonomy over their own practice. Experiment 5 also allowed us to assess the immediate impact of self- versus partner-produced errors on subsequent practice decisions.

Different to self-generated errors, participants tended to repeat the same task if their partner performed well and to practice something else if their partner performed poorly. These analyses represent an important advancement in the literature, as previous research has not identified connections between self-directed practice decisions and motor performance under dyad conditions (e.g., Wulf et al., 2001). Similar analyses assessing the relationship between performance and practice-related decisions should be included in future dyad learning research, as a better understanding of these links would help to build a framework of when and why dyad practice can enhance learning.

7.2.4 On the influence of a partner on perceptions of practice

Throughout this dissertation, I have probed participants' perceptions of the experimental practice conditions using customized, study-specific questions as well as the Intrinsic Motivation Inventory (IMI; Deci & Ryan, n.d.). I begin this section by discussing the findings related to the former, exploratory questions and address the constructs measured by the IMI in a later section with respect to factors impacting dyad practice.

As anticipated, concurrently observing a partner during physical practice trials was rated as more interfering than watching a partner in alternation with physical practice (Karlinsky & Hodges, 2018b). From a theoretical standpoint, this perhaps relates to ideomotor theory (James, 1890) and the more contemporary theory of event coding (e.g., Hommel et al., 2001; Prinz, 1997). Building on these theories, it has been posited that observed actions can evoke similar neural activity in the observer (e.g., Jeannerod, 2001), and neurophysiological and neuroimaging research has provided compelling support that action observation can activate the action representations recruited during the physical execution of the movement (e.g., Rizzolatti & Craighero, 2004). In the case of concurrent practice, it is possible that observation-induced action

representations interfere with individuals' efforts to optimize their own motor performance (although surprisingly we did not show evidence for interference as evidenced by poorer performance in practice).

In Experiment 2, where individuals took turns putting to a target with different putters, counter to my hypothesis, mismatching a partner's task on consecutive trials was not perceived as more interfering than matching (Karlinsky & Hodges, in review). It appears that as long as what is observed is practice-relevant (and does not interfere with online action production, *cf.* Experiment 1's concurrent whole-task practice), individuals do not perceive a peer's practice attempts as harmful to their own performance. Individuals also reported positive impressions of the novel peer-directed practice paradigm in Experiment 3 (Karlinsky & Hodges, 2014). The peer-directed actors and peer-schedulers rated the practice session as more motivating and enjoyable than the self-directed actors and observing peers. Peer-directed practice has also been positively received when peers decided when to provide actors with KR feedback (McRae, 2015; McRae et al., 2015).

When partners alternated blocks of practice in Experiment 4, observing a partner's practice was rated as more helpful and less interfering by those partners who were allowed to self-direct their own practice schedule (Karlinsky & Hodges, 2018a). In Experiment 5, however, there were group- (as opposed to partner-) level effects. The blocked-partner pairs perceived their partner as more interfering than the random-partner pairs. It is possible that the blocked pairs' low-CI schedules made a partner's practice seem more intrusive on a trial-to-trial basis (in contrast to Experiment 4, where each partner was able to practice for a block of 9 trials before switching turns). A limitation of these data is that only a single item was used to probe these subjective perceptions. In future, it will be important to incorporate multi-item and ideally

validated questionnaires for assessing these and other subjective experiences. The inclusion of anxiety-related questionnaires would also be helpful to parcel out individual differences which may mediate people's perceptions of (and their success within) social learning settings (e.g., Arthur et al., 1996; Goettl, Anthony, Derek, Snooks, & Shebilske, 1998; Naber, McDonald, Asenuga, & Arthur, 2015).

7.2.5 On the factors impacting dyad practice

7.2.5.1 Social facilitation, audience effects, and co-action effects

Although the studies detailed in this dissertation were not specifically designed to test the contributions of social facilitation-, audience-, or co-action-related effects towards dyad learning, I would be remiss not to consider their potential impact on shared practice settings. My ability to comment on social facilitation is limited, however, as I did not include a comparison of learners who practiced alone versus those who practiced in the presence of a non-observing co-learner. Research by Granados and Wulf (2007) provides some insight into this question. Alternating practice when partners did not observe one another's turns (but rather discussed the task in between trials) did not modulate performance or learning compared to practice alone (Granados & Wulf, 2007). Thus, I can infer that the presence of a non-observing co-learner did not significantly affect practice or learning compared to "individual" practice conditions, with the caveat that conclusions regarding social facilitation effects are still prevented by the presence of an experimenter in both conditions.

In the experiments presented herein, participants were instructed (with one exception) to observe their co-learner's practice. Thus, considerations of potential audience effects and co-action effects are more relevant to the current research. The strongest evidence that audience effects did *not* have a significant impact on the efficacy of dyad practice is provided by

Experiment 4 (Karlinsky & Hodges, 2018a). This study included a "self-self" dyad group, which consisted of two learners who both self-directed their practice schedule in alternating blocks. Critically, one learner wore a blindfold when it was their partner's turn to practice. As such, the former partner was subjected to an "audience" (observing co-learner) while the latter was not. There were no significant differences between the observed and non-observed partner subgroups in terms of self-directed task-switching frequency or strategy, errors during practice or retention, or perceptions of the practice experience. Supplementary data for Experiment 3 (presented in Appendix B) also revealed there were no significant differences in the practice behaviours or outcomes of self-directed learners who practiced alone versus while observed by a passive observer or peer-scheduler. Thus, there was no evidence that being watched by a co-learner (without the opportunity to watch them in return) affected motor performance or learning.

When there was the opportunity for bidirectional observation and co-action, a variety of behavioural measures indicated practice with a partner impacted motor performance (including movement coupling in Experiment 1, post-error compensatory putting in Experiment 2, and partner-mediated task-switching in Experiments 4 and 5). However, for the most part, these co-action-related effects did not ultimately impact retention. It might be that the experience of being observed by and interacting with a peer is mediated by individual personality traits (e.g., interaction anxiety, see Arthur et al., 1996; Goettl et al., 1998; Naber et al., 2015). As such, it would be of value for future researchers to measure individual difference variables to provide further insight into when and for whom shared practice can confer benefits/harm.

Clearly, there remains much to learn regarding whether and how the *mere* presence, *observing* presence, and *interactive* presence of another person impacts motor learning-relevant issues. I anticipate that the identity of those present and the nature of the performance contexts

will also be important to manipulate, as it is likely that the presence of peers versus instructors and cooperative versus competitive settings will differentially impact learners' motivational and affective experiences, as well as their behaviour.

7.2.5.2 Observational practice and learning

Overall, I did not observe any evidence that combined physical and observational practice enhanced retention or transfer compared to pure physical practice. As a result, it is difficult to speak to the processes that occured during peer observation. Comparisons to Shea et al. (1999) do however allow speculation as to the role of observation in Experiment 1, where I used the same stabiliometer balance-related task and similar individual versus alternating practice groups (Karlinsky & Hodges, 2018b). I might have inadvertently decreased the benefits of observational learning by instructing participants how to stand on the balance platform (as opposed to Shea et al., 1999, who did not issue these same constraints). While I chose to provide these postural instructions for safety and consistency, they could have limited the opportunity for partners to learn how to balance on the unstable platform from watching one another. This speculation is also relevant to Experiment 2, where the experimenter demonstrated the golf putting technique (i.e., how to hold the putter) and where again alternating practice did not enhance learning compared to pure physical practice (Karlinsky & Hodges, in review).

While I anticipated that the role of peer-scheduler would enhance learning more than passive observation, there were no differences in Experiment 3 between the passive observer partners compared to the active peer-schedulers. Both these observation-only groups showed poorer motor learning compared to individuals who physically practiced (Karlinsky & Hodges, 2014). While there is evidence elsewhere that observation is superior to no practice at all (e.g., Badets, Blandin, & Shea, 2006; Black & Wright, 2000; Shea et al., 2000), we did not include a

no-practice control group, which would allow for stronger conclusions about any benefits (or lack thereof) of these passive versus active observation opportunities.

In Experiment 4, where individuals were paired with blocked or random partners, but were allowed to choose how to practice, the opportunity to observe a partner's practice did not benefit learners compared to being made to wear a blindfold during a partner's turn (Karlinsky & Hodges, 2018a). This lack of an observation-related benefit was reinforced in Experiment 5, where learners who practiced in alternation with a partner did not outperform learners who practiced alone. Of note, both of these sets of data involved self-controlled learners. It is possible that the opportunities for peer-observation and for self-controlled practice confer unique but not additive benefits. Similar conclusions were drawn by Wulf and colleagues (2001), who suggested that self-control likely benefits the acquisition of movement characteristics that are difficult to pick up just from watching, and require physical practice and active awareness, whereas peer observation likely supports the learning of movement characteristics that are easily observable. Clearly, the opportunity for peer observation in and of itself does not guarantee augmented learning compared to physical practice alone, underscoring the need for further research to identify the necessary and sufficient factors which render dyad learning more effective than individual practice.

7.2.5.3 Motivation-related factors

In addition to the behavioural outcomes associated with paired practice settings, I also evaluated learners' perceptions of these more social learning contexts. Participants responded to the IMI in all studies excluding Experiment 3. The IMI is a multidimensional questionnaire which is designed to assess participants' interest and attitudes toward the experimental task,

including their perceptions of interest/enjoyment, competence, choice, effort, and pressure/tension (Deci & Ryan, n.d.).

7.2.5.3.1 Interest/enjoyment

One dimension of the IMI I expected to differentiate between individual versus dyad practice, as well as between self- versus experimenter-directed practice, was interest/enjoyment. This subscale indexes intrinsic motivation (Deci & Ryan, n.d.), and several authors have suggested that mixed physical and observational practice and the opportunity to control practice enhance motivation through interest and enjoyment (e.g., Lewthwaite & Wulf, 2012; Sanli et al., 2013; Schmidt & Lee, 2011). However, I did not observe this finding to be the case in the present work. These (lack of) findings can be interpreted in two ways. Either the reported dyad and self-control protocols do not comprise motivational benefits compared to the physical practice only and experimenter-directed conditions, or the interest/enjoyment subscale of the IMI was not sensitive to the perhaps more subtle differences in motivation. Thus, future research should include additional measures (e.g., visual analog scales for motivation and anxiety, see Daou et al., 2016) to further assess the contribution of motivation-related mechanisms towards social and self-controlled learning.

7.2.5.3.2 Social comparisons and perceptions of competence

Shared practice contexts inherently provide opportunities for participants and instructors to compare individuals' performance and make competency-related judgments. Critically, research on comparative feedback (about a peer or average performance on a given task), has relied upon virtual or experimenter-provided comparisons, limiting our understanding of how such comparative feedback is implicated in genuine social settings. Considering that the criteria used to judge success impacts perceptions of competence and ultimately learning (e.g.,

Chiviacowsky, Wulf, & Lewthwaite, 2012), it follows that if individuals refer to their co-learners' performance to assess their own capabilities, this could have implications for perceptions of competence and potentially learning.

Learners' perceptions of competence were shown to differ depending on the type of practice schedule they (or their partner) were assigned to follow. Consistent with previous research on individuals, learners following a blocked practice schedule reported higher perceptions of competence than those following a random practice schedule (Karlinsky & Hodges, 2018a; see also Simon & Bjork, 2001, 2002). These findings have been attributed to learners' (typically inaccurate) tendency to judge their learning based on their performance during practice, with relatively easy (in this case, blocked) practice resulting in overestimations of learning (Simon & Bjork, 2001, 2002). Interestingly, learners' perceptions of competence also seemed to be sensitive to the relative ease/difficulty of their practice compared to that of a co-learner. In Experiment 4, where co-learners alternated turns after 9 practice trials on a sequence timing task, there were no differences between partners' perceptions of competence within the dyad groups, but an interaction was shown. Self-directed learners who practiced with a random-schedule partner rated their own competence higher than those who practiced with a blocked-schedule partner (despite there being no significant differences in acquisition performance; Karlinsky & Hodges, 2018a). When partners alternated turns on consecutive trials in Experiment 5, now there were differences within groups, but not between groups: participants generally rated their partner's competence as higher than their own, despite there again being no partner-related differences in performance. I speculate that the different styles of alternating practice in Experiments 4 and 5 (every 9 trials versus every trial) contributed to these differing patterns of results. Specifically, in Experiment 4, self-directed learners saw their partner perform and the

corresponding error feedback for 9 trials in a row. Blocked-schedule partners would have generally decreased their error across these repetitive 9 trials and could have appeared to be achieving lower errors than their self-directed partner (who in the majority of pairs did not choose to follow a fully blocked practice schedule). In contrast, the random-schedule partners would have shown smaller performance improvements within practice blocks, potentially making their errors/performance seem worse than their self-directed partner (who did not follow a fully random practice schedule). Thus, if self-directed learners assessed their own competency in relation to perceptions of their partner's competency, differences in performance were likely more salient in the blocked turn-taking conditions of Experiment 4.

Overall, considering learners often have to self-assess their own proficiency (contributing to decisions about whether to engage in additional practice, to seek further instruction, etc.; Simon & Bjork, 2002), it will be important for researchers and practitioners alike to consider how such self-assessments and associated decisions might be affected by social comparisons in shared practice settings. I suggest an important first step will be to compare the effects of comparative feedback provided by a real partner versus by an experimenter, to assess whether previous research on comparative feedback is likely to generalize to veridical social settings.

Another related, interesting line of inquiry relevant to perceptions of competence within social learning settings relates to concepts of "other-efficacy" and "relation-inferred self-efficacy" (Lent & Lopez, 2002). Other-efficacy refers to the beliefs individuals have about the capabilities of a partner (e.g., teammate, coach), while relation-inferred self-efficacy refers to the beliefs individuals hold about how their efficacy is viewed by their interaction partner (e.g., Partner 1's beliefs about how Partner 2 sees Partner 1's efficacy). While exploring these concepts was beyond the scope of the current dissertation, such percepts have been shown, for

example, to predict satisfaction among athlete-athlete (Jackson, Beauchamp, & Knapp, 2007) and coach-athlete partnerships (Jackson & Beauchamp, 2010) as well as to significantly affect motor performance (Dunlop, Beatty, & Beauchamp, 2011). Further research is needed to better understand the relative contributions of self-, other-, and relation-inferred self-efficacy towards skill acquisition in social settings.

7.2.5.3.3 Perceptions of choice

Several authors have argued that allowing learners to control aspects of their practice provides an autonomy-supportive environment that yields superior motivation and learning compared to less autonomy-supportive conditions (for reviews, see Lewthwaite & Wulf, 2012; Wulf & Lewthwaite, 2016). Data from this dissertation do not support this perspective. As expected, learners allowed to self-direct their own practice schedule perceived themselves to have more choice (i.e., autonomy) over their practice than learners following predetermined practice schedules, who commensurately perceived themselves to have less choice than their self-directing partners (Experiment 4; Karlinsky & Hodges, 2018a). However, these differences in perceived choice were not accompanied by corresponding differences in interest/enjoyment (i.e., motivation) nor learning outcomes. These missing links between autonomy, motivation, and motor learning were previously highlighted when self-controlled learning benefits were observed in the absence of enhanced autonomy (choice) or motivation (interest/enjoyment; Ste-Marie et al., 2013). Although I did not measure perceptions of choice in Experiment 3, this study also challenged the motivational account of self-controlled learning. Specifically, I manipulated whether actors themselves or their observing partner controlled the actor's task-switching schedule (to provide more or less autonomy-supportive contexts, respectively), and showed that both conditions yielded similar learning outcomes. Thus, having choice was not necessary for

learners to benefit from customized practice schedules, supporting instead the importance of the information processing activities undertaken during practice.

Overall, these findings suggest that motivational factors associated with self-controlled practice do not necessarily work the same way under social conditions. Instead, it seems plausible that social settings could confer alternative sources of motivation (e.g., self-presentation-related motivations; Baumeister & Hutton, 1987), potentially masking or outweighing the contribution of motivational mechanisms deemed important in individual settings (e.g., autonomy).

7.2.5.3.4 Perceptions of effort and pressure/tension

There is reason to think that dyad practice might differentially elicit perceptions of effort and pressure/tension compared to practice alone (e.g., related to learners' desire to protect their self-image and keep up with peers; Rhea et al., 2003). For instance, there is a phenomenon known as the Köhler motivation gain effect, which occurs when less capable individuals exert more effort and perform better when performing a task with others, compared to performing the same task alone (e.g., Hertel, Kerr, & Messé, 2000; Osborn, Irwin, Skogsberg, & Feltz, 2012). While dyad practice was indeed associated with higher ratings of effort in Experiment 1 (Karlinsky & Hodges, 2018b), perceptions of effort and pressure/tension did not vary between groups in subsequent studies. Of note, participants always practiced in neutral (non-competitive) contexts, which likely helped to downplay pressure/tension-related perceptions. Of future interest is to manipulate the competitive context of the practice conditions, to feature competition either within or between pairs. This would allow me to assess how being responsible for another person's and/or one's own success impacts effort, perceptions of pressure/tension, and motivation to improve/learn.

7.2.5.4 Dialogue

Throughout this dissertation, I did not permit co-learners to discuss the task(s) they worked on during the practice sessions, as my primary research questions targeted observation-related impacts of dyad practice on motor performance and learning. It is likely, though, that allowing co-learners to discuss the task(s) would be helpful for the acquisition of challenging skills and/or those that do not have a predetermined strategy (e.g., McNevin et al., 2000; Prislin et al., 1996; Shea et al., 1999). For instance, there were no benefits of alternating (or concurrent) practice compared to pure physical practice of a balance-related task in Experiment 1 (Karlinsky & Hodges, 2018b), when participants were instructed how to stand on the balance platform from the outset. In contrast, alternating practice enhanced learning of the same balance-related task when learners were not provided these instructions and allowed to test out and discuss different strategies (Shea et al., 1999).

7.2.5.5 Distributed practice

It is well established in the motor learning literature that allowing learners to rest in between trials enhances learning compared to more massed practice conditions (for reviews, see Donovan & Radosevich, 1999; Lee & Genovese, 1988). Thus, I controlled the inter-trial breaks of those participants who practiced alone to match the breaks received by those participants who practiced with a partner, to control for any benefits of dyad practice associated with spacing effects. Considering I did not observe any advantages of dyad practice compared to pure physical practice throughout this dissertation, these rest intervals may indeed be at the root of some dyad practice benefits. The future inclusion of massed individual practice conditions would help to clarify the potential contributions of practice distribution effects to dyad learning. Allowing

learners to self-pace their practice and rest intervals would also provide insight into whether the efficiency of dyad practice would persist if practice/rest time was learner-controlled.

7.3 Future research directions

A number of future directions have been alluded to in the previous chapters, in the context of highlighting the limitations of each of the reported studies. Importantly, as one of the overarching goals of this research program was to inform the design of practice conditions that yield efficient and effective learning, the external and ecological validity of the study results will need to be confirmed in real-world settings. There are also, of course, countless questions and conditions that were beyond the scope of the current dissertation. A selection of these outstanding questions are outlined in the following subsections with regards to, i) task difficulty and performer skill level, ii) task sharing and division of labour within different dyad practice paradigms, iii) scaling up dyad learning to "team" skill acquisition, and iv) the generalizability of dyad learning to novel partners.

7.3.1 Task difficulty and performer skill level

I anticipate that systematically testing tasks of varying difficulty and participants of differing skill level will help not only to frame the current dyad learning findings, but also to guide future dyad learning research. It may be that certain dyad practice paradigms are more or less beneficial depending on the difficulty of the task (representing the nominal task difficulty) and/or the skill level of the performers (impacting the functional task difficulty; Guadagnoli & Lee, 2004). For example, the CI effect tends to be more robust for the learning of relatively simple skills as opposed to more complex skills (e.g., Lee, 2012; Wulf & Shea, 2002). Additionally, more skilled individuals tend to benefit more from higher-CI practice schedules, whereas less skilled individuals tend to benefit more from lower-CI practice schedules (e.g.,

Guadagnoli, Holcomb, & Weber, 1999). It is possible that so too would more skilled individuals benefit more from higher between-person CI (particularly for the learning of simple skills), potentially in the form of mismatched practice, whereas less skilled individuals might benefit more from lower between-person CI, potentially achieved through matched dyad practice. To begin teasing apart whether task difficulty might impact the efficacy of dyad practice protocols, we are currently testing the matched versus mismatched dyad practice protocol introduced in Experiment 2 (when pairs practiced relatively difficult golf putting skills) when pairs now alternate practice of relatively simple keystroke timing tasks.

Task difficulty has also been shown to moderate individual learners' practice decisions (e.g., Andrieux, Danna, & Thon, 2012; Keetch & Lee, 2007) and might also mediate the influence of a partner's practice on self-directed practice behaviours. For instance, self-directed learners who practiced an easier set of computer sequence tasks chose to switch between tasks more frequently than self-directed learners who practiced a more difficult set of sequences (Keetch & Lee, 2007). While we showed that practicing in alternation with a random-schedule partner promoted self-directed task-switching (Experiments 4 and 5), these effects occurred during the practice of relatively simple keystroke tasks. Whether or not learners' practice decisions would remain susceptible to partner-related behaviours during the practice of more (nominally and/or functionally) difficult skills, will be important for determining the conditions under which dyad practice is likely to modulate performance and learning outcomes.

7.3.2 Task sharing and division of labour

An intriguing paradigm that merits future attention stems from the "active interlocked modeling" (AIM) protocol forwarded by Shebilske and colleagues (e.g., Shebilske et al., 1992, 1998). In this part-practice protocol, partners completed physical practice at the same time, but

they distributed the task demands by alternating turns performing the ‘pilot’ (joystick) tasks or the ‘copilot’ (mouse) tasks (which involved different hands). This division of labour during practice enabled learners to perform the entire game individually during testing as effectively as participants who had trained individually on the whole task (bimanually). This was achieved despite the fact that dyad members completed only half the physical practice on each task component and never practiced the components at the same time. While the efficiency garnered by this task-sharing protocol is impressive in its own right, I am interested in whether the opportunity to physically practice both task components is necessary, or whether part-task practice with a partner might still confer benefits in the absence of physical practice. That is, if partners each only practiced one of the two complementary task components, how would this impact the acquisition of the whole skill?

In anticipation of this line of research, I have novelly adapted a 2:3 polyrhythm task (where the left index finger taps two beats for each three beats of the right index finger; see Kurtz & Lee, 2003), to determine the effects of interlocked part-practice when a partner takes ownership of one half of the task (research in progress). It is possible that even in the absence of alternating tasks, learners can benefit from this form of task sharing. Indeed, a number of other (non-motor learning) studies have shown that easing complexity by performing part of a task with a partner can reduce interference and facilitate performance compared to performing the whole task alone (e.g., Heed, Habets, Sebanz, & Knoblich, 2010; Sellaro, Treccani, & Cubelli, 2018; Wahn, Keshava, Sinnett, Kingstone, & König, 2017). Studying how different forms of shared practice might enable a reduction in each learner’s physical demands and yield efficiencies in training would be relevant to researchers and practitioners alike.

The predictability versus unpredictability of task-sharing, and relatedly, the strictness versus flexibility of labour divisions also provide interesting avenues for future research. Consider the cases of doubles' table tennis versus doubles' tennis. In doubles' table tennis, the game rules stipulate that partners alternate turns returning strokes. In contrast, in doubles' tennis, only the initial serve requires a certain partner to perform the return stroke. In the former case, task distribution is predictable (players always know when it is their turn to hit the ball) and strictly determined from the outset. In the latter case, task distribution is unpredictable (after the initial serve and return) and the division of labour is flexible. Strategies might involve partners distributing the court (e.g., one partner is responsible for the front versus back, or left versus right side of the court), but there is also the possibility for task-sharing strategies to emerge dynamically, perhaps with differences in skill levels leading one partner to cover more than their "fair share" of the court (for a review of distributed versus redundant joint task sharing, where in the latter co-actors have the option to distribute the task flexibly, see Wahn, Karlinsky, Schmitz, & König, 2018). I recently studied the impact of predictability in a paired reaction time task, and showed that even more important than *turn* predictability was *response* predictability (Karlinsky, Lam, Chua, & Hodges, 2017). This can be conceptualized as the difference between knowing that it is your *turn* in doubles' table tennis to make the next return stroke (given the predictable task distribution) compared to additionally knowing what *response* you will have to make when it is your turn (e.g., lob, down-the-line, cross-court). Similar manipulations to the predictability of task sharing conditions could be applied in motor learning research to promote attention/cognitive effort in practice, and potentially to help train decision-making "on the fly".

7.3.3 Scaling up dyad learning

Based on my own and other authors' data (e.g., Granados & Wulf, 2007; Shea et al., 1999, 2000; Shebilske et al., 1992), dyad learning clearly does not double learning compared to individual practice. There is, however, some preliminary evidence that increasing the group size can afford additional benefits to members of the group (although these do not increase proportionally with the size of the group). When groups of three learners practiced a speeded cup-stacking task one after another (i.e., Learner 1 completed all of their trials before Learner 2, etc.), observing two co-learners prior to physical practice was better than observing a single co-learner, which was in turn better than not watching anyone before physically practicing (Hebert, 2018). However, as the learners completed their physical practice in a blocked fashion, rather than rotating turns on consecutive trials, it is unclear whether alternating between three (or more) learners on a trial-to-trial basis would confer any more benefits relative to paired turn-taking. Notably, the opportunity to observe two video-recorded models of different skill levels interspersed with physical practice has been shown to enhance learning compared to observing a single model (Andrieux & Proteau, 2013). Whether similar benefits would arise in larger group practice settings, and whether this would be due to the opportunity to observe more variable action strategies (Andrieux & Proteau, 2013; Hebert, 2018; Rohbanfard & Proteau, 2011), or simply the opportunity for more observation in between physical practice trials remains to be disentangled.

Paired practice benefits have been evidenced under larger group conditions, when the amount of hands-on practice was decreased via interlocked, part-practice (e.g., Shebilske, Jordan, Goettl, & Paulus, 1998; Worchel, Shebilske, Jordan, & Prislin, 1997). Rotating three or four group members through the part-task practice Space Fortress protocol (so that participants

alternated between the ‘pilot’, ‘co-pilot’, and ‘observer’ roles), yielded equivalent results as the paired and individual training conditions (Shebilske et al., 1998; Worchel et al., 1997). This was the case even though the larger groups decreased the hands-on practice trainees received to one third or one quarter of the amount provided to individuals. It should be noted that these larger group conditions were not more *effective* than individual or paired practice, but just more *efficient*, such that three or four learners could be trained to the same level of proficiency in the amount of time otherwise devoted to one person. Whether there is an upper limit where the optimal (or at least non-detrimental) group size has been reached, such that further increasing the number of learners will not lead to larger benefits (or more importantly, not lead to smaller gains), remains to be tested.

7.3.4 Generalizability of dyad learning to novel partners

Another worthwhile future direction is to study how skills might be specific to the partner(s) with whom they were acquired. Consider practicing a volleyball set pass or a basketball alley-oop to a particular teammate, or a hockey player who practises an offensive play with the same line-mates. It is possible that after first acquiring a skill that requires coordination with a particular peer or set of peers, that individuals will need to modify how they interact when switching to different co-actors (possibly leading to initial decrements in performance). Learning to decrease one’s variability could be a useful way to facilitate coordination with an action partner (e.g., Vesper, van der Wel, Knoblich, & Sebanz, 2011, 2013; for a review, see Vesper et al., 2017) and potentially ease transfer to different co-learners. This novel line of inquiry, where individuals physically practice with one co-learner (or one set of co-learners) and are tested with another, will be critical for better understanding when social practice should be considered as a viable and generalizable means of skill acquisition.

7.4 Concluding remarks

In summary, this dissertation adds to our understanding of how a practice partner impacts individuals' motor performance, learning, and perceptions of the practice experience. Overall, paired training was as effective as individual training, while also conferring some efficiency-related advantages. However, there are currently many outstanding questions with respect to when and why paired practice leads to motor learning benefits compared to individual practice. Active interlocked modeling protocols have most robustly been shown to support learning compared to individual practice, but such part-task practice is not necessarily applicable for many sport skills. More relevant would be alternating or concurrent whole-task forms of dyad practice, where learners have the opportunity to observe a partner interleaved or simultaneous with their own physical practice of a complete skill. Although I have provided evidence that partners influence each other's practice and that there may be some beneficial interference conferred under some conditions, there has not been strong evidence that for learning outcomes, paired practice is preferable to practice alone. Additional well-powered research is needed to explore the necessary and sufficient conditions to yield learning benefits for individuals within social practice settings.

Researchers and practitioners should also consider how peers, especially those simply observing practice, could be more actively involved in the skill acquisition process (such as through peer-directed practice), not just to the potential benefit of the observer's learning, but also as a means to provide informational and/or motivational support to the physically active learner. There is much to be gained theoretically and practically by continued efforts to determine the factors that make paired (and team) learning a success and in future it will be important to complement behavioural measures with those probing psychological and

neurophysiological processes (such as measures of error processing captured using EEG), to provide a more thorough understanding of the mechanisms underpinning shared learning.

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Appendices

Appendix A Intrinsic Motivation Inventory

In recent years, much attention has been given to the importance of motivation-related factors towards optimizing motor learning (for reviews, see Lewthwaite and Wulf, 2012; Wulf & Lewthwaite, 2016). However, a review of 26 peer-reviewed studies related to learner-controlled practice (a training protocol posited to promote motivation) revealed that the benefits of motivation towards motor learning have been largely assumed (Sanli, Patterson, Bray, & Lee, 2013). As one means of overcoming this limitation, Sanli and colleagues (2013) suggested that researchers include the Intrinsic Motivation Inventory (IMI) in future studies, in order to provide some quantification of motivation-related factors and better understand their contribution to the learning process.

The IMI is a multidimensional tool designed for laboratory use, which measures participants' subjective, task-specific experience (Deci & Ryan, n.d.). The full version of the IMI comprises seven different subscales and consists of 45 items. Although the tool is called the Intrinsic Motivation Inventory, only the interest/enjoyment subscale directly assesses intrinsic motivation (Deci & Ryan, n.d.). McAuley and colleagues tested the internal consistency of the IMI, and each subscale was found to display satisfactory validity and reliability (McAuley, Duncan, & Tammen, 1989). Thus, researchers often choose to use only those subscales most relevant to their research questions, with no deleterious effects (Deci & Ryan, n.d.). Select subscales of the IMI have recently been used in conjunction with behavioural measures towards the study of learner-controlled model observation (Ste-Marie, Vertes, Law, & Rymal, 2013; interest/enjoyment and perceived choice subscales) and learner-controlled augmented feedback

(Chiviacowsky, Wulf, & Lewthwaite, 2012; interest/enjoyment and perceived competence subscales).

There is a standard, 22-item version of the inventory that is considered to be appropriate for most motor learning studies (Sanli et al., 2013). This version of the IMI is referred to as the Task Evaluation Questionnaire and consists of four subscales: interest/enjoyment (7 items), perceived competence (5 items), perceived choice (5 items), and pressure/tension (5 items). I used the Task Evaluation Questionnaire in Experiment 2 (Chapter 3), and added the effort subcale (5 items) to the questionnaire for Experiments 1, 4, and 5 (Chapters 2, 5, and 6). I also included an adapted partner-related competence subscale (5 items) in Experiments 1, 4, and 5 (Chapters 2, 5, and 6) as well as an adapted partner-related choice subscale (5 items) in Experiments 4 and 5 (Chapters 5 and 6). These adaptations consisted of changing self-references within the items (e.g., “I am”, “I did”, “my performance”) to references to the partner (e.g., “my partner is”, “my partner did”, “my partner’s performance”).

All subscale items are responded to using a Likert-type scale ranging from 1 to 7, where a score of 1 indicates that the item statement is *not at all true* and a score of 7 indicates that it is *very true*. Some items are reverse-coded, so that a higher score always indicates more of the concept described by the subscale name. Across subscales, the generic item wording can be customized to the experimental task (Deci & Ryan, n.d.). The item wording was modified for Experiment 1 (Chapter 2) by replacing ‘task’ with ‘balance task’, for Experiment 2 (Chapter 3) by replacing ‘task’ with ‘putting tasks’, and for Experiments 4–5 (Chapter 5–6) by repacing ‘task’ with ‘keystroke tasks’. A sample version of the inventory used in Experiments 4 and 5 is presented next.

Participant ID: _____ Group: _____

For each of the following statements, please indicate how true it is for you (and for some questions, your <u>partner</u>), using the following scale:

Not at all true			Somewhat true			Very true
1	2	3	4	5	6	7

___ 1. While I was practising the keystroke tasks I was thinking about how much I enjoyed them.

___ 2. I did not feel at all nervous about doing the keystroke tasks.

___ 3. I felt that it was my choice to do the keystroke tasks to the best of my ability.

___ 4. I felt that it was **<u>my partner's</u>** choice to do the keystroke tasks to the best of her ability.

___ 5. I think I am pretty good at the keystroke tasks.

___ 6. I think **<u>my partner</u>** is pretty good at the keystroke tasks.

___ 7. I put a lot of effort into practicing the keystroke tasks.

___ 8. I found the keystroke tasks very interesting.

___ 9. I felt tense while doing the keystroke tasks.

___ 10. I think I did pretty well at the keystroke tasks, compared to others (including my partner).

___ 11. I think **<u>my partner</u>** did pretty well at the keystroke tasks, compared to others (including myself).

___ 12. Doing the keystroke tasks was fun.

___ 13. I did not try very hard to do well on the keystroke tasks.

___ 14. I felt relaxed while doing the keystroke tasks.

___ 15. I enjoyed doing the keystroke tasks very much.

___ 16. I didn't really feel I had control over how I practised the keystroke tasks.

___ 17. I didn't really feel **<u>my partner</u>** had control over how she practised the keystroke tasks.

___ 18. I am satisfied with my performance at the keystroke tasks.

___ 19. I am satisfied with **my partner's** performance at the keystroke tasks.

___ 20. I tried very hard on the keystroke tasks.

___ 21. I was anxious while doing the keystroke tasks.

___ 22. I thought the keystroke tasks were very boring.

___ 23. I felt like I was doing what I wanted to do while I was practising the keystroke tasks.

___ 24. I felt like **my partner** was doing what she wanted to do while she was practising the keystroke tasks.

___ 25. I felt pretty skilled at the keystroke tasks.

___ 26. I felt **my partner** was pretty skilled at the keystroke tasks.

___ 27. I thought the keystroke tasks were very interesting.

___ 28. It was important to me to do well at the keystroke tasks.

___ 29. I felt pressured while doing the keystroke tasks.

___ 30. I felt I had to practise the keystroke tasks in a certain way.

___ 31. I felt **my partner** had to practise the keystroke tasks in a certain way.

___ 32. I would describe the keystroke tasks as very enjoyable.

___ 33. I practised the keystroke tasks in a certain way because I felt I had no choice.

___ 34. I felt **my partner** practised the keystroke tasks in a certain way because she had no choice.

___ 35. After working at the keystroke tasks for a while, I felt pretty competent.

___ 36. I felt **my partner** was pretty competent after working at the keystroke tasks for a while.

___ 37. I did not put much energy into practicing the keystroke tasks.

Appendix B Supplementary results of Experiment 3: Self- vs. other-directed practice

B.1 Introduction

The principle goals of Experiment 3 were to assess the importance of decision-making towards the benefits of self-scheduled practice by applying a dyad-learning paradigm, as well as to evaluate the effectiveness of peer-directed practice. As a secondary aim, I also tested two groups of participants who practiced alone (n = 12/group), in order to reveal any potential benefits of paired compared to individual practice. The *self-alone* group consisted of Actors who self-directed the organization of their 90-trial acquisition phase. This group provided a means to assess the effects of an observing partner on self-scheduled practice, through comparisons to the self-scheduled pair (*self-pair*) group. The *yoked-alone* group comprised individuals who were each yoked to an Actor in the peer-scheduled pair (*peer-pair*) group, and followed the same task-switching schedule that had been selected for this Actor by the observing partner. Thus, the yoked-alone Actors were not expected to experience performance-contingent practice, as their task-switching schedule would not have been tailored to their own trial-to-trial performance. Rather, this group provided a means to assess the general efficacy of the peer-selected practice schedules (presumably related to their degree of contextual interference, CI), dissociated from their performance-contingent nature. As the Partner data (i.e., from the passive observers and peer-schedulers) are presented and discussed in Experiment 3 (Chapter 4), only the Actor results are presented below.

B.2 Methods

B.2.1 Participants and Groups

Seventy-two women (18–35 years of age) volunteered to participate and were assigned to practice alone or in pairs, based on availability. Within each pair, partners were randomly

assigned the role of Actor or Partner. Actors physically practiced while the responsibilities of the Partner varied depending on group. In the *self-pair* group, the Actor self-directed practice and the Partner passively observed. In the *peer-pair* group, the Partner directed the Actor's practice schedule. The *self-alone* and *yoked-alone* groups consisted solely of Actors, who either self-scheduled their practice or were yoked to an Actor in the peer-pair group, respectively. All participants self-reported as right-handed, provided informed consent, were naïve to the specific goals of the study, and were paid $10 per hour.

B.2.2 Task and Apparatus

The task and apparatus were as reported in Experiment 3 (Chapter 4).

B.2.3 Procedures

The procedures were as presented in Experiment 3 (Chapter 4), with the addition of two individual groups. The *self-alone* group consisted of Actors who self-directed the organization of their 90-trial acquisition phase. The *yoked-alone* group comprised individuals who were each yoked to an Actor in the peer-scheduled pair (*peer-pair*) group, and followed the same task-switching schedule that had been selected for this Actor by the observing partner. An experimenter directed the yoked-alone Actors' practice schedule by announcing which keystroke sequence to select at the beginning of each trial from the back of the testing room.

B.2.4 Measures and Analysis

The measures and analysis were as reported in Experiment 3 (Chapter 4).

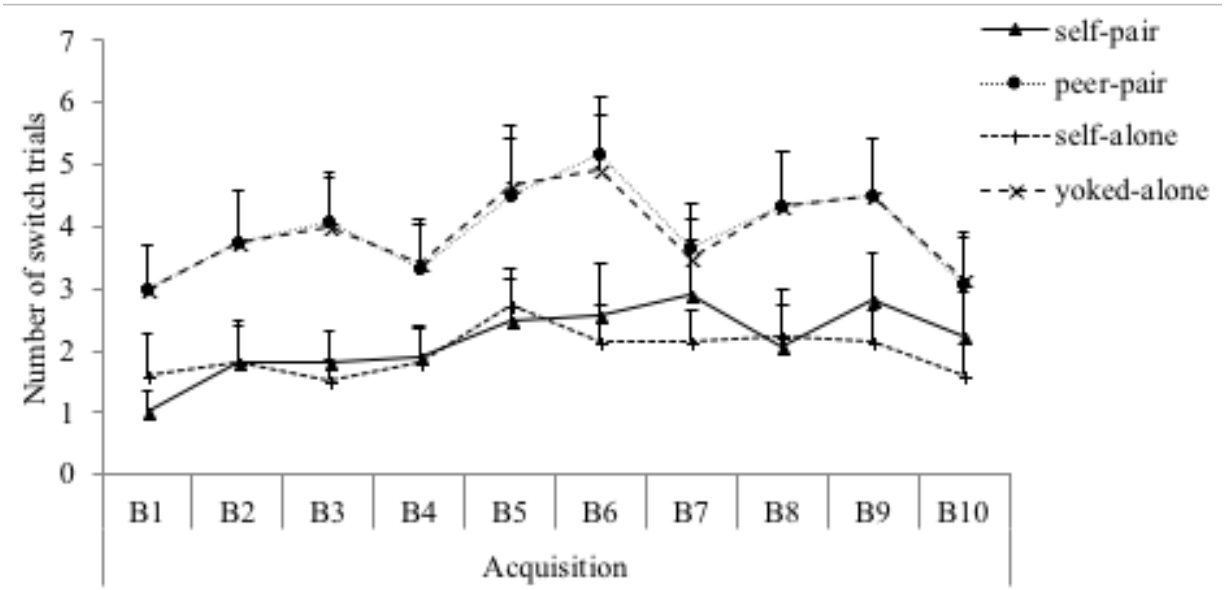
B.3 Results

B.3.1 Evidence for performance-contingent switching

B.3.1.1 Switching frequency

Figure 19 shows the number of task switches per block of 9 trials across the acquisition phase. These switches between the keystroke sequences were self-directed by the Actors in the *self-pair* and *self-alone* groups, and imposed upon the Actors in the 'other'-directed *peer-pair* and *yoked-alone* groups. The number of switch trials was submitted to a 3-way mixed ANOVA, with Condition (pair vs. alone) and Scheduler (self vs. other) as between factors and Block (1–10) as within. The peer-schedulers directed more switches between sequences per block (M = 3.9 switches, SD = 2.8 switches) than the self-schedulers (M = 2.1 switches, SD = 2.1 switches), $F(1, 44) = 11.34$, $p = .002$, $\eta_p^2 = .21$. The Scheduler × Block interaction was not significant, $F(5.15, 226.55) = 1.06$, $p = .39$. There were no effects or interactions related to the paired vs. individual nature of the practice condition (Fs < 1). Sequence switching covaried as a function of block, generally increasing until the middle of practice, $F(5.15, 226.55) = 4.37$, $p = .001$, $\eta_p^2 = .09$. There was no Block × Condition or 3-way interaction (Fs < 1).

Figure 19. Average number of switch trials (and *SE* bars) as a function of 9-trial blocks for the self- and peer-scheduled Actors who practiced in pairs, and the self-scheduled and yoked-to-peer-scheduled Actors who practiced alone.



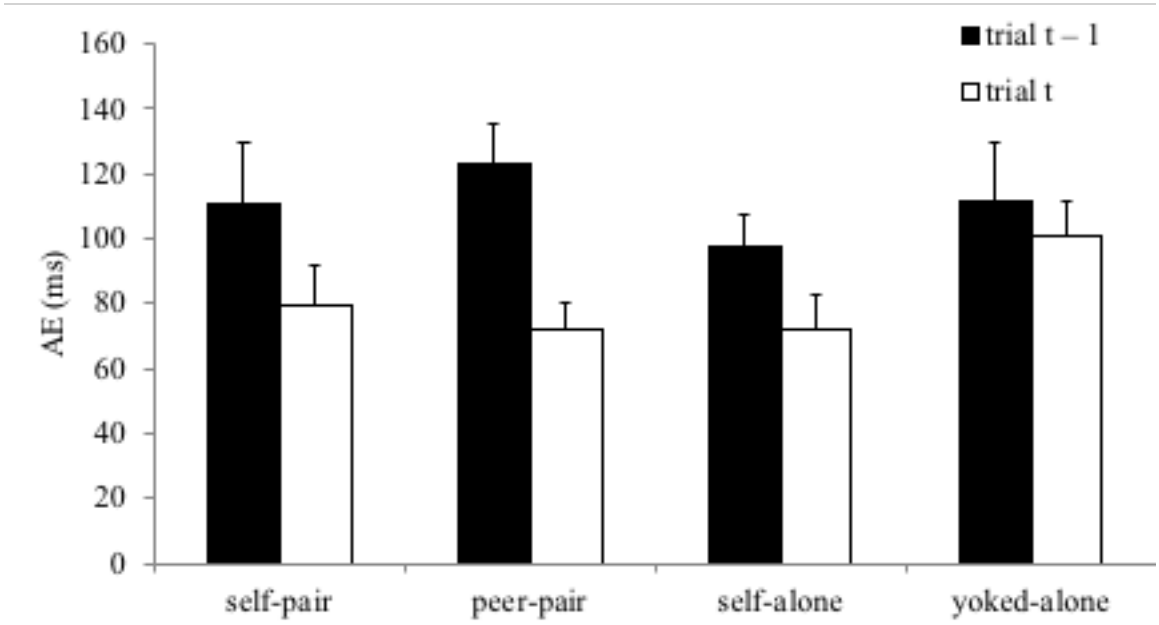
B.3.1.2 Switching strategies

The Actors' mean AE on switch trials (trial t) and the trial preceding the switch (trial t – 1) were submitted to a 3-way mixed ANOVA, with Condition (pair vs. alone) and Scheduler (self vs. other) as between factors and Trial-type (t 'switch', t – 1 'nonswitch') as within. As shown in Figure 20, there was a main effect of Trial-type, such that AE was significantly lower on switch trials (M = 81.2 ms, SD = 13.7 ms) than on the same sequence trial preceding the switch (M = 110.7 ms, SD = 10.4 ms), $F(1, 44) = 21.56$, $p < .001$, $\eta_p^2 = .33$. There was no effect of Scheduler, $F(1, 44) = 1.12$, $p = .30$, nor any Condition-related effects or interactions (Fs < 1). Surprisingly, there were also no significant interactions related to Trial-type, despite the hypothesized difference between customized practice schedules (afforded in the self-alone, self-pair, and peer-pair groups) and predetermined practice schedules (assigned to the yoked-alone group). However, the Trial × Condition interaction did show a trend towards significance, $F(1, 44) = 3.44$, $p = .070$, which appeared to be driven by the yoked-alone group's lack of performance-contingent switching pattern (discernible in Figure 20).

I further investigated the switching strategy data by conducting paired samples t-tests to compare the AE scores on switch trials and pre-switch trials on a group-by-group basis. The self-pair group tended to switch to a different sequence following trials on which their error was lower (M = 79.0 ms, SD = 42.8 ms) than on the preceding trial (M = 110.9 ms, SD = 64.5 ms), $t(11) = 1.89$, $p = .085$. With respect to the peer-pair group, there was a significant difference between error scores on switch trials (M = 72.1 ms, SD = 28.7 ms) and the preceding trial (M = 122.8 ms, SD = 42.6 ms), $t(11) = 4.73$, $p = .001$. A significant difference between trial types was also revealed for the self-alone group, with lower error scores on switch trials (M = 72.4 ms, SD = 36.6 ms) compared to the preceding nonswitch trials (M = 97.4 ms, SD = 33.8 ms), $t(11)$ =

3.08, p = .010. As expected, there was no significant difference between the yoked-alone Actors' AE scores on switch (M = 101.2 ms, SD = 36.8 ms) versus preceding nonswitch trials (M = 111.7 ms, SD = 61.8 ms), $t(11)$ = .78, p = .45. From these results, I inferred that the self- and peer-schedulers tended to organize practice in a performance-contingent manner, switching between sequences following relatively good performance. This same switching strategy was not seen in the yoked group's data, as they did not have the opportunity to experience adaptive practice schedules.

Figure 20. Actors' mean AE (and *SE* bars) on switch trials (trial t) in comparison to same-sequence trials immediately preceding the switch (trial t – 1) for the self- and peer-scheduled Actors who practiced in pairs, and the self-scheduled and yoked-to-peer-scheduled Actors who practiced alone.



B.3.2 Self-scheduled Actors retained the tasks better than other-scheduled Actors

B.3.2.1 Acquisition

The percent movement time error (%MTE) scores over the course of the acquisition session were submitted to a 3-way mixed ANOVA, with Condition (pair vs. alone) and Scheduler (self vs. other) as between factors and Block (1–10) as within. Analysis revealed an effect of Block. As shown in Figure 21, participants generally improved (reduced error with respect to the sequences' goal MT) with practice, $F(4.68, 205.84) = 11.07$, $p < .001$, $\eta_p^2 = .20$. This was confirmed by a significant linear trend component to the block effect, $F(1, 44) = 46.87$, $p < .001$, $\eta_p^2 = .52$. There were no main effects or interactions related to the Condition or Scheduler factors.

B.3.2.2 Retention

The Actors' %MTE scores on the retention test were submitted to a 3-way mixed ANOVA, with Condition (pair vs. alone) and Scheduler (self vs. other) as between factors and Block (1–2) as within. The retention test results are displayed on the right side of Figure 21. There was no main effect of Scheduler, Condition, or Block (Fs < 1.1). However, there was a significant Scheduler × Block interaction, $F(1, 44) = 4.54$, $p = .039$, $\eta_p^2 = .09$. Post-hoc tests revealed that while there was no significant difference between blocks within groups, self-scheduled participants were less errorful in the second block of retention (M = 12.4%, SD = 5.6%) than the 'other'-scheduled Actors in both retention blocks (Block 1: M = 15.4%, SD = 10.1%; Block 2: M = 16.5%, SD = 12.2%). This indicates that while undertaking physical practice alone or with an observing partner did not differentially influence learning, the opportunity to engage in self-scheduled practice enhanced learning. However, as can be seen in Figure 21, this interaction effect appears to be predominantly driven by the more errorful

performance of the yoked-alone group, suggesting that it is more likely the opportunity to practice under performance-contingent conditions that enhances learning than self-scheduled practice per se.

B.3.3 'Other'-scheduled practice was as motivating and enjoyable as self-scheduled practice. Paired practice was as motivating and enjoyable as practicing alone.

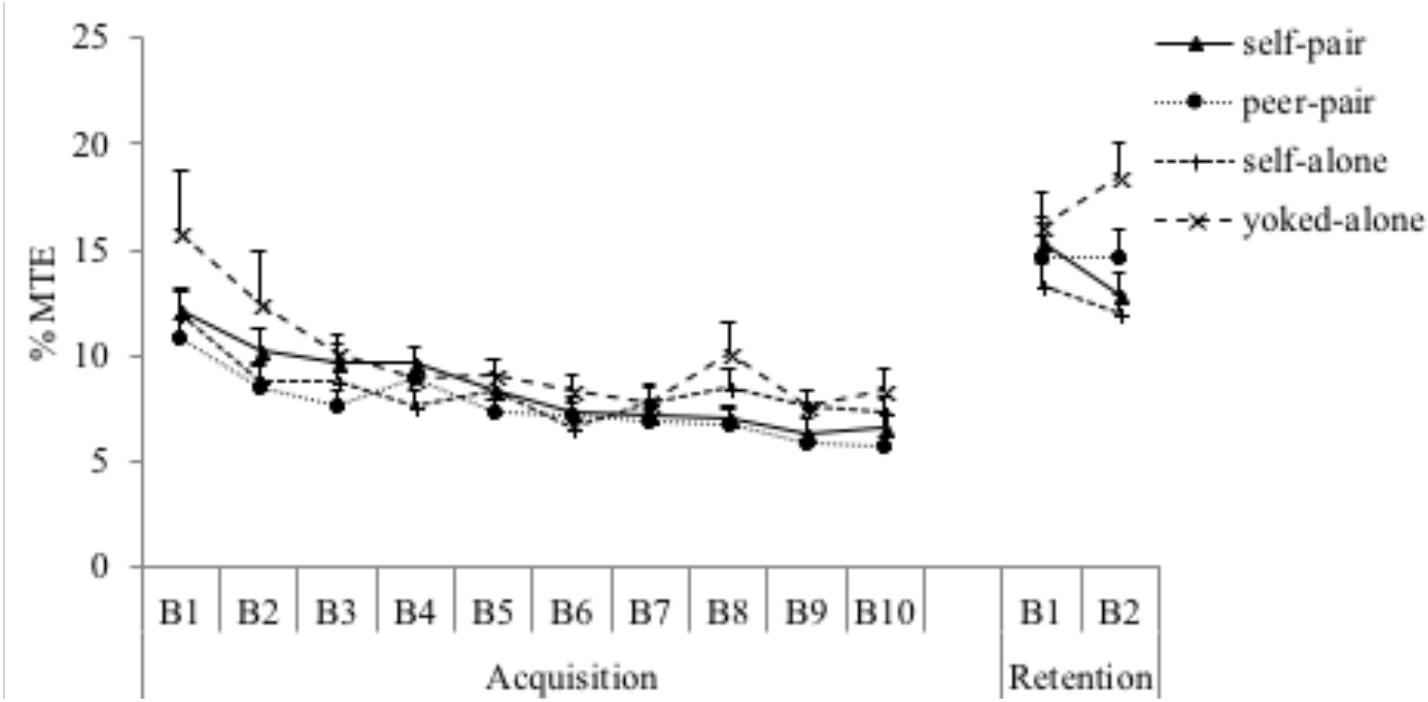
B.3.3.1 Motivation

The Actors' ratings of their motivation during acquisition were submitted to a 2 Condition (pair vs. alone) × 2 Scheduler (self vs. other) ANOVA. There was no effect of practice Condition or Scheduler (Fs < 1). There was a Condition × Scheduler interaction, but post-hoc tests revealed no significant differences, $F(1, 44) = 6.00$, $p = .018$, $\eta_p^2 = .12$. The Actors did not differ in their motivation to learn the keystroke sequences, regardless of whether or not their practice was self-directed, and whether it was undertaken with an observing partner or alone.

B.3.3.2 Enjoyment

The Actors' ratings of how much they enjoyed practicing the sequences were submitted to a 2 Condition (pair vs. alone) × 2 Scheduler (self vs. other) ANOVA. As there was no effect of Scheduler, $F(1, 44) = 2.14$, $p = .15$, Condition, nor a Condition × Scheduler interaction (Fs < 1), I inferred that practice was similarly enjoyable whether or not it was self-directed, and whether it was completed with an observing partner or alone.

Figure 21. Average percentage movement time error (and *SE* bars) as a function of 9-trial blocks for the acquisition and retention phases for the self- and peer-scheduled Actors who practiced in pairs, and the self-scheduled and yoked-to-peer-scheduled Actors who practiced alone.



B.4 Discussion

In Experiment 3 (presented in Chapter 4), I sought to determine whether the learning advantages associated with self-directed practice schedules are potentially related to decisions about when to switch between skills during practice. By applying a dyad-learning paradigm, I was also able to evaluate the effectiveness of peer-directed practice schedules. Actors either self-directed their own practice schedule with an observing partner (self-pair group) or their practice was directed by their observing peer partner (peer-pair group). I also tested two individual practice groups to enable comparisons between individual versus paired practice. Through comparisons to the self-pair group, the self-alone group allowed assessment of the influence of an observing partner on self-directed practice. Members of the yoked-alone group were paired to an Actor in the peer-pair group, and followed the same practice schedule that had been selected for this Actor by their observing partner. Thus, the yoked group allowed consideration of the general efficacy of the peer-directed schedules, uncoupled from their presumed customization to the Actor partner.

The peer-schedulers directed more frequent switching between the keystroke sequences than selected by the self-scheduling Actors. As suggested in the main discussion of Experiment 3 (Chapter 4), the lower contextual interference schedules adopted by the self-directing Actors might have been a means of managing the difficulty of their practice, given the heightened information processing demands inherent in self-controlled practice conditions. The self-scheduled Actors may also have considered more subjective experiences when making their scheduling decisions, in contrast to the peer-schedulers' presumably exclusive reliance on the augmented MT feedback provided after every trial. Regardless of the potentially different sources of information contributing to the scheduling decisions, both self- and peer-schedulers

tended to strategically organize practice in a performance-contingent manner, such that the Actors switched to a different sequence following relatively good (low error) performance. This pattern of results was understandably not seen in the yoked Actors' data, as they followed practice schedules that had been tailored to another learner.

While there were no differences between the self- and other-directed Actors' acquisition performance, with all learners improving their keystroke sequence timing with practice, there was a significant interaction between practice scheduler and retention block when Actors were tested alone the following day. Self-scheduled Actors demonstrated the best performance in block 2 of the retention test, significantly outperforming the peer-scheduled and yoked Actors.

The Actors' responses to the debrief questions revealed that neither the scheduler's identity (self vs. other) nor the nature of the practice context (pair vs. alone) modulated the Actors' motivation to learn the keystroke sequences or their enjoyment of the practice session. That there were no significant differences between the self-pair and self-alone Actors' practice schedules, learning, or affective experiences, suggests that the presence of an observing partner did not alter learners' self-scheduled practice experience compared to practicing alone. I am interested in whether, and to what extent self-scheduled practice might be influenced by a partner who also physically engaged in the task (e.g., turn-taking practice). I have studied this and related questions in Experiments 4 and 5 (presented in Chapters 5 and 6, respectively).