

**The Recommendation for Learners to be Provided with Control over their Feedback  
Schedule is Questioned in a Self-Controlled Learning Paradigm**

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**Dedication**

To my mom, dad, and two sisters, Brittany and Katrina.

### Abstract

Researchers have shown that learners who self-control (SC) their knowledge of results (KR) schedule learn the task more effectively than yoked learners. A common recommendation from these results is that learners should be provided choice over their KR schedule, rather than at a coaches' discretion (Wulf & Lewthwaite, 2016). No research to date has compared SC learners to a group that more closely mimics receiving KR from a coach, thus challenging whether such a recommendation can be made. To this end, three groups learned a golf putting task; an SC group, a traditional yoked group (TY), and a group who were led to believe that their KR schedule was being controlled by a golf coach (perceived coach-controlled yoked group; PCC). Participants ( $N = 60$ ) completed three phases; pre-test, acquisition, and two 24-hr delayed post-tests (retention/transfer). All groups lowered their mean radial error (MRE) and bivariate variable error (BVE) throughout acquisition. As hypothesized, the SC group ( $M = 40.10$ ) had lower adjusted MRE compared to the TY group ( $M = 43.12$ ) during the post-tests, yet, the PCC group had the lowest adjusted MRE ( $M = 36.61$ ). These differences, however, were not statistically significant,  $F(2, 54) = 2.81, p = .069$ . BVE did not display the same pattern as MRE during the post-test as group means were clustered together,  $F(2, 57) = 0.38, p = .963$ . Results from a questionnaire indicated that both yoked groups showed moderate ratings for receiving KR on a desired schedule, as well as preferring KR on good trials, or good and bad trials equally. Taken together, these results call into question the recommendation for practitioners to give choice to a learner over KR scheduling.

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## **Chapter 1: Introduction**

In the 21<sup>st</sup> century, movement and movement training have become popularized in the media by Conor McGregor and his coach Ido Portal, among others, which has led to an influx of artistic movement variations that leave many people in admiration. The field of motor behaviour examines how these movements are learned and performed with such efficiency, smoothness, style, and grace. Movement can take many forms, with some forms being regarded as genetically defined and others emerging through growth and development (Schmidt & Lee, 2011). Motor learning researchers are primarily interested in another class of movement, referred to as “learned” movements (Schmidt & Lee, 2011). These “learned” movements are often termed skills and are defined as the ability to bring about some outcome with maximum certainty and minimal energy and time (Guthrie, 1952). Thus, an example of a motor skill would be learning to execute an uppercut in mixed martial arts. Factors that impact the acquisition of motor skills is the focus of motor learning research and the specific factor of interest here is the use of self-controlled learning; more specifically, that of self-controlled scheduling of knowledge of results (KR).

Motor learning has been defined as a set of processes associated with practice or experience that lead to relatively permanent changes in the capability of responding (Guadagnoli & Lee, 2004). It has been proposed that one of the most important features of practice is the information that learners receive following their attempts to produce an action (Salmoni, Schmidt, & Walter, 1984), termed feedback, which arrives via two different sources; intrinsic and extrinsic feedback. Intrinsic feedback (or response-produced feedback; Adam, 1968) comes from various sensory receptors (e.g., somatosensory, visual, auditory) and is always available for a learner (unless purposely removed by the experimenter or a medical condition; Schmidt, 1975). In contrast, extrinsic feedback (or augmented feedback) is information provided about a movement from an

outside source that supplements the information from the sensory systems (Salmoni et al., 1984). Extrinsic feedback is commonly provided in applied settings from coaches, instructors, and therapists. For example, information received from a golf instructor regarding the putter head path during a stroke would be a form of augmented feedback (herein referred to only as feedback).

Recent research on the effectiveness of feedback delivery has shown learning benefits associated with the learners making choices regarding the delivery of feedback (i.e., self-controlled feedback schedules; see Sanli, Patterson, Bray, & Lee, 2013 for a review). The typical research protocol for self-controlled learning involves the comparison of learning outcomes for those that are provided choice over a practice variable (e.g., feedback schedule) with those who are not provided with that choice, termed a yoked group. Participants that are assigned to a yoked group are paired with a counterpart from a self-control group, such that they receive the practice variable in the same manner as their self-control counterpart during the acquisition phase, with the only difference between the groups being the choice.

Researchers have consistently shown that those who self-control their feedback schedule learn motor tasks more effectively than yoked groups (Yantha, McKay, Carter, & Ste-Marie, 2019). Self-controlled experiments have employed a range of skills from laboratory-based to applied. Yantha and colleagues (2019) classified 13 skills as being an applied skill in an applied setting and 12 as being an applied skill in a laboratory setting in their recent meta-analysis. Examples of applied skills included; overhand throwing (Janelle, Barba, Frehlich, Tennant, & Carraugh, 1997), a set shot in basketball (Aiken, Fairbrother, & Post, 2012; Post, Fairbrother, Barros, & Kulpa, 2014), double mini-trampoline (Ste-Marie, Vertes, Law, & Rymal, 2013), a golf pitch shot (Post, Aiken, Laughlin, & Fairbrother, 2016), and a passé relevé (Fagundes,

Chen, & Laguna, 2013). As such, these self-controlled learning advantages have been quite robust. Based on these learning advantages for those in a self-control group, researchers then make recommendations that learners should be provided with choice over their feedback schedule and not have it provided based on a coaches' discretion (Fagundes et al., 2013; Wulf & Lewthwaite, 2016). For example, Wulf and Lewthwaite (2016) argued that optimal practice conditions included the "...scenario in which the instructor gives the learner choices, provides feedback at the request of the learner..." (p. 25).

The problem here though is that no research to date has examined the influence of feedback coming from a knowledgeable source, like that of an instructor or coach, versus that of a learner with self-control. This gap in the literature challenges whether these recommendations can be made by researchers. While it has been demonstrated that allowing a learner to control their feedback schedule is beneficial compared to a predetermined or random schedule, we cannot say that a learner should necessarily replace the role of an instructor or coach. We argue that researchers are not giving consideration to the idea that coaches have knowledge and expertise of the task, and thus, may generate better learning conditions than self-controlled feedback schedules. Therefore, it is necessary to gain an understanding of the impact of learners receiving feedback from one whom they perceive as being knowledgeable and having expertise with the task, instead of a yoked learner that is simply given a predetermined feedback schedule, and compare this learning situation to self-controlled conditions. Indeed, this comparison is more ecologically valid as it is a coach that delivers feedback in typical motor learning situations. Hence, the purpose of my research is to examine the possible effects of a "perceived" coach-controlled feedback schedule compared to a schedule controlled by a learner. The word

perceived is in quotes here, because, in effect, the proposed research will not have a true coach-controlled feedback schedule for reasons elaborated upon in the upcoming sections.

Given this research interest, in the literature review, the typical experimental design used by researchers in the motor learning domain is described in which the importance of delayed retention and transfer tests is highlighted. More elaboration is provided on augmented feedback, both knowledge of performance (KP) and knowledge of results (KR), with a specific focus on key experiments over the last two decades. Then, findings from three recent experiments are described that compare self-controlled practice to a feedback schedule that is more like a real-life setting; that of a peer-controlled schedule. Finally, background information from the coaching literature on athletes' perceptions and their influence on how they interpret the feedback they receive from a coach is presented. Lastly, the experimental design along with the hypotheses are outlined.

## **Chapter 2: Literature Review**

### **Motor Learning Paradigm**

The experimental design used by researchers in the motor learning domain has commonly included two phases; acquisition (i.e., practice), and a post-test phase, with the post-test commonly including both retention and transfer tests (Carter, Smith, Carlsen, & Ste-Marie, 2017; Chiviacowsky & Wulf, 2002). However, more recently, experimental designs have added a third experimental phase; a pre-test phase. This phase has been used to assess the baseline skill level of participants (Leiker, Pathania, Miller, & Lohse, 2018; Rhoads, Daou, Lohse, & Miller, 2019; St. Germain, Lelievre, & Ste-Marie, 2019). The acquisition phase provides a practice interval with the participant completing numerous trials with the independent variable(s) present.

Practice has been said to be the single most important factor that is critical for skill acquisition, however, it would provide little value if it did not result in long-term improvement in the ability to perform a skill (Kantak & Winstein, 2012, Newell, 1976). This brings in the importance of using retention and transfer tests in the post-test phase to measure learning. Both retention and transfer tests involve assessing skill performance for all groups after a specific interval of time following the acquisition phase (Salmoni et al., 1984). Learners are tested after this time delay so that possible transient effects of the independent variables will be removed to expose the relatively permanent effects (Schmidt & Lee, 2011). Examples of transient factors include, but are not limited to; mood, depressing effects of mental fatigue, drugs, and distractions (Salmoni et al., 1984). Retention involves testing the learners on the same task that was practiced in the acquisition phase and is argued to represent the strength of the motor memory representation over time (Kantak & Winstein, 2012). Transfer tests, on the other hand, involve testing the learner on a new variation of the skill, on a related but different skill, or in a different

context, which can be thought of as the flexibility or generalizability of the motor memory (Kantak & Winstein, 2012).

A critical factor in a motor learning experimental design regarding the retention and transfer phases is the time interval between the end of acquisition and the beginning of retention/transfer; i.e., the retention interval. Any memory, whether it is of a piece of information or a motor skill involves three processes; (1) encoding, which is the formation of the motor memory that is thought to occur in acquisition, (2) consolidation, considered the process that leads to the stabilization of the memory, and (3) retrieval, which includes multiple processes such as recall, recognition, and relearning that enables access to the memory from storage (Kantak & Winstein, 2012). To encompass these varied motor memory processes, it is recommended that the retention interval be at least twenty-four hours (delayed) to include the important sleep stages that allow for consolidation of motor memories (Kantak & Winstein, 2012; Salmoni et al., 1984; Schmidt & Bjork, 1992). One important observation is that, in some cases, the differential effects of a practice condition on motor learning are evident during a delayed retention/transfer test, but not so clear from acquisition and/or immediate retention and transfer tests. In their analysis of immediate (i.e., 10 s to hours after acquisition) and delayed retention tests, Kantak and Winstein (2012) found that performance on the immediate retention/transfer tests were poor predictors of long-term changes in motor behaviour for a large proportion of the experiments (63%). Such results demonstrate the importance of separating transient performance from learning by including delayed retention and transfer intervals that are, at a minimum, 24 hrs in duration.

### **Augmented Feedback**

Augmented feedback can be broken down into two categories; KR and KP. KR has been defined as verbalizable, terminal (i.e., after the movement) feedback about the outcome of the movement in terms of the environmental goal (Salmoni et al., 1984). An example would be a golf instructor telling a player that they missed the pin by 15 feet to the left and 2 feet long. KP feedback concerns the movement pattern, which can be provided by viewing a video or being told by an instructor or coach about the position of body parts (Schmidt & Lee, 2011). In the case of a golf swing, an example of KP feedback would be an instructor telling a player that they took the club past parallel at the top of the backswing. KP feedback is commonly provided by instructors or coaches where corrections for improper movement patterns (or reinforcement of proper movement patterns) are given, rather than outcome information.

One challenge that motor learning researchers face when examining augmented feedback is that in most natural environments it is difficult to control the sensory information received by a learner. A common strategy that is implemented by researchers is to alter the environment, or task, such that response-produced feedback is eliminated. In this way, the learner must rely on KR (or KP) from the experimenter. An example of the removal of response-produced feedback is tossing a bean bag with the non-dominant arm while wearing a blindfold. While this strategy results in tasks that may seem artificial, these learning situations are argued to be fundamentally the same as intrinsic error information the individual would receive in a more natural setting (Schmidt & Lee, 2011). Further, although KR and KP are definitionally distinct, it has been suggested that both types of feedback may operate under the same principles (e.g., Young and Schmidt (1992) showed that KR principles of a reduced frequency (less guidance) also generalized to KP; Salmoni et al., 1984; Schmidt & Lee, 2011). For example, in learning a flop

shot in golf, it may be that the principles of learning are the same for KR (“Your distance from the hole”) and KP (“You brought the club too far back”) but each could result in the individual learning something different about the shot or one could be more efficient than the other (Salmoni et al., 1984). As such, experiments that focus on KR manipulations are argued to be transferable to situations in which KP would be used to assist motor learning.

To give some historical context to the feedback literature, early researchers that examined the effectiveness of feedback scheduling concluded that the most effective KR schedule was a 100% absolute frequency (e.g., Bilodeau & Bilodeau, 1958). This early research, however, suffered from a flawed research design in which delayed retention/transfer tests were not used. Later research that included delayed retention tests revealed that a reduced frequency of feedback was better for learning compared to the 100% frequency (Lee, White, & Carnahan, 1990; Sullivan, Kantak, & Burtner, 2008). These findings led Salmoni et al. (1984) to propose the “guidance hypothesis” in which KR was said to provide information about a response outcome that the participant then uses to correct their errors and generate a new response. The authors explained the findings of 100% KR being detrimental for learning via this ‘guidance hypothesis’. It was argued that the constant provision of feedback could result in the dependence on extrinsic feedback, which would lead learners to avoid engaging in their own response evaluation processes of their intrinsic feedback received. This lack of response evaluation processes would then not allow learners to develop their error detection and correction mechanisms.

Consequently, when a learner is placed in an environment that has limited or no extrinsic feedback, such as a no-KR retention test or in a real-life competition, the learner would not be equipped to interpret their intrinsic feedback to generate accurate movements, and performance at this later time will suffer. In summary, it was argued that an effective KR schedule is one that

makes the learner more in-tune with their intrinsic feedback; examples include, but are not limited to; summary KR (Lavery & Suddon, 1962), trials-delay KR (Swinnen, Schmidt, Nicholson, & Shapiro, 1990), and bandwidth KR (Sherwood, 1988).

More recently, researchers have examined the impact of providing the learners with an opportunity to control the scheduling of when they receive augmented feedback, typically in the form of KR; which has been termed self-controlled feedback scheduling. As mentioned, the standard paradigm used in self-control research is to provide one group of participants control over a learning feature on a trial-to-trial basis, whereas another group of participants are not afforded any choice, termed a yoked group. Therefore, the yoked group has the learning feature directly imposed during the practice phase. To exemplify this concept, consider that a participant in a self-control group had requested KR on the first and third trials in a five-trial block. A participant that is yoked to this participant's feedback schedule would receive KR on the first and third trials as well, but not have been provided the choice as to when he/she wanted the KR. Hence, the relative and absolute amount of KR is controlled, with the only difference between the two groups being the provision of choice. The reason for including the yoked group is to be able to infer that having the control over KR is the critical factor accounting for the learning difference between the self-controlled group and the yoked group, rather than the absolute amount of KR, or its scheduling. The self-controlled feedback literature is elaborated upon next.

### **Self-Controlled Feedback Schedules**

Janelle, Kim, and Singer (1995) were one of the first researchers that argued that in previous feedback scheduling research there was an over-emphasis on the role of the teacher or instructor and a lack of emphasis on the role of the learner. The authors borrowed from the psychology literature that indicated that active involvement of the learner in the learning process could lead

to enhanced retention of important information (Zimmerman, 1989). The authors compared participants that were in one of four groups; (1) self-controlled schedule, (2) yoked schedule, and two evidence-based, best learning method groups of, (3) 50% relative performance schedule (i.e., feedback every second trial), and (4) summary schedule (i.e., feedback every 5<sup>th</sup> trial). Participants practiced an underhand throwing task and were provided KP feedback verbally about the mechanics of the throwing arm, force, or trajectory of the toss. Results from the immediate retention test revealed that the self-controlled participants had significantly lower absolute error compared to all other groups. Thus, this was the first experiment to show that a feedback schedule controlled by a learner may be a more effective means of delivering feedback compared to other best practice schedules.

Janelle and colleagues (1997) continued their investigations of self-controlled KP, but moved to the task of an overhand throw, considered the impact of self-controlled scheduling on movement form, switched to the provision of KP via video feedback, and included a delayed retention test. Similar to their first experiment, participants were assigned to a self-controlled KP, summary KP or a yoked KP group, with the addition of a KR only group. During the delayed retention, the self-controlled KP group had superior accuracy and form compared to all other groups. These results extended the self-controlled feedback learning advantage to not just movement outcome but movement form.

Similar to the research with KP, others also examined the effects of self-controlling KR. One of the first experiments that investigated the effectiveness of self-controlled KR schedules for motor skill learning used a five-digit key pressing sequence (Chen, Hendrick, & Lidor, 2002). The authors had participants learn the task in one of four groups; (1) a self-controlled KR group, (2) an experimenter-induced self-controlled KR group, (3) a yoked to self-controlled KR group,

or (4) a yoked to experimenter-induced self-controlled group. The difference between the two self-controlled groups was that the experimenter-induced group received a prompt after every trial asking the learner if they would like to receive KR. In contrast, the self-controlled KR group was just told at the beginning of practice that they had choice over when to receive KR and were not prompted after each trial. The results of the delayed retention test indicated that both self-controlled groups demonstrated enhanced learning compared to their yoked counterparts, but also that the experimenter-induced self-controlled KR group performed the most accurately. This latter finding may be why an experimenter-induced self-controlled group has become the standard method for self-controlled groups used today. These results extend the benefit of self-controlled feedback schedules to KR practice settings.

Self-controlled learning advantages are considered robust, transferring across numerous practice variables, such as KP (Janelle et al., 1997; Ste-Marie, Carter, Law, Vertes, & Smith, 2016), KR (Carter & Ste-Marie, 2017; Patterson & Carter, 2010), assistive devices (Wulf & Toole, 1999), the type of practice order (Keetch & Lee, 2007; Wu & Magill, 2011), the use of observation of a model (Wrisberg & Pein, 2002), and, in some cases, even incidental choices (Lewthwaite, Chiviawsky, Drews, & Wulf, 2015). Moreover, these advantages occur across different age groups (Chiviawsky, Wulf, de Medeiros, Kaefer, & Tani, 2008; Chiviawsky, Wulf, de Medeiros, Kaefer, & Wally, 2008), as well as with different populations, such as persons with Down's syndrome (Chiviawsky, Wulf, Machado, & Rydberg, 2012) or Parkinson's disease (Chiviawsky, Wulf, Lewthwaite, & Campos, 2012).

Noteworthy is that in all these experiments, the main comparison group is a yoked control group in which the learner is naïve to the task. Using a yoked-learner is problematic because these yoked-learners do not represent how a learner would be typically provided with feedback

in a learning scenario; for example, in a real-life setting, a coach would be observing a learner and be using his or her knowledge and expertise to determine the most appropriate times to provide feedback. An exception to the main comparison group typically being a yoked group would be Janelle and colleagues' (1995, 1997) experiments that compared a self-control group to best practice feedback schedules at the time. Regardless, in these experiments, the learners in the best practice groups were still provided the KP on a predetermined feedback schedule, and consequently, the contribution (or perceived contribution) of a coaches' knowledge in terms of when to deliver the KP was not a factor. As such, the relative comparison of self-controlled learners to that of a perceived knowledgeable person with task expertise providing KR has not been tested. Recently, some researchers have started to examine self-control conditions against other comparison groups that better represent real-life sport settings and these findings do suggest that we should consider other comparison groups in self-controlled research.

### **Peer-Controlled Feedback Schedules**

McRae, Patterson, and Hansen (2015) conducted one of the first experiments to compare a self-control group to a non-predetermined feedback schedule and used instead a peer who controlled the schedule. The authors noted that in many practical motor learning situations, learners may rely on one another for feedback, for example, a peer may be asked to control the provision of feedback for another learner in the absence of a coach. In their experiment, participants attempted to learn a 5-digit key pressing task that had to be done in a specific movement time. They learned this task in either a self-controlled KR group or in a peer-controlled KR group. The participants in the peer-controlled group had a peer, who was new to the task, and situated in an adjacent room, decide on when to provide KR. The self-controlled group was yoked to the number of feedback opportunities provided by a peer-controlled

participant and were thus able to choose when they wanted KR under that feedback constraint. KR consisted of whether the order of the keypress sequence was correct or incorrect, too fast or too slow, and the constant error for movement time. Results from the delayed retention test indicated that the peer-controlled group were more consistent (VE) than the self-control group. No difference in overall accuracy (AE) or bias (CE) were found between the two groups.

These results showed that the self-control group, as compared to a peer-controlled group, no longer showed the learning advantages that had been reported in other experimentation. These results emerged even though the learner was aware that their feedback schedule was controlled by someone inexperienced with the task, and in another room; learning conditions which seem far removed from the standard peer-led example that was provided by the authors. Another important limitation, that was noted by the authors, was the absence of a traditional yoked group, which would have allowed for a comparison between the peer-created schedule and a yoked feedback schedule. The lack of a yoked group does not enable one to determine if the typically self-controlled learning advantage was gained and if the peer delivering the feedback removed that advantage. Nonetheless, these findings do hint at the notion that different comparison groups from that of a yoked learner may provide insight into whether self-controlled learning is an optimal strategy to use for feedback provision.

More recently, McRae, Patterson, and Hansen (2017) conducted a follow-up experiment that examined whether or not the task experience of a peer would impact their frequency and preference for providing feedback to a novice learner. The experiment included the same two groups as that of McRae et al. (2015), but also added a group who had their feedback schedule controlled by an experienced peer (i.e., a self-controlled participant after they completed the experimental protocol). Therefore, participants learned the same 5-digit key pressing task in

either a self-controlled, a learner controlled by an experienced peer, or a learner controlled by an inexperienced peer group. Results from the delayed retention test showed no statistical differences between the groups for motor performance measures, thus replicating and extending the findings of their previous experiment. This experimental protocol, however, can be challenged in terms of whether the learning situation maps well onto the general research question of peer-led practices, given the removal of social interactions and the ‘experienced peer’ being someone who had only executed 80 trials of a laboratory task.

Karlinsky and Hodges (2014) also made the comparison between a self-control group and a peer-controlled group, however, in this experiment, the variable of practice that was manipulated was the practice schedule rather than the feedback schedule. Participants were assigned to either a self-controlled group or a peer-scheduled group. Participants in the self-control group were provided with the choice over the order of which to practice the tasks, whereas the peer-schedule group had the task order determined by an inexperienced peer that sat adjacent to the learner. The experimental task was the learning of three different five-keystroke sequences, similar to the task used by McRae et al. (2015, 2017). During retention, the self-controlled group was not statistically different from the peer-scheduled group in percentage of movement time error. Again, however, the peer in this task did not have any experience with the task, which is not representative of a typical peer-learning environment. Together the results of the three peer-controlled experiments challenge the robustness of the self-controlled learning advantage, and bring up a critical question; is providing a learner with choice over their feedback schedule beneficial compared to a learning situation that is more in line with the real world; i.e., a coach-controlled schedule?

What becomes apparent from the reviewed literature is that it has been shown that self-controlled KR schedules relative to yoked schedules and predetermined schedules are more effective, however, no evidence shows a self-controlled schedule being superior to coach- or instructor-led schedules. While the peer-control research begins to question the superiority of self-controlled schedules, the peers used were not portrayed as knowledgeable with how to deliver feedback, nor would they be considered to have a high level of expertise with the task. Coaches, however, are typically individuals who have expertise with the task being taught and are informed about motor learning practices and these factors could have an impact when controlling a learner's feedback schedule.

Even though the impact of a coach scheduling the feedback has not been specifically examined, a recent experiment has examined how music experts structure their practice when given self-control over the scheduling order of three novel flying disc throws (Hodges, Edwards, Luttin, & Bowcock, 2011). It was assumed that the expert musicians would have knowledge of effective practice methods and that they would transfer these habits across domains (i.e., from music to sport). To this end, the expert musicians were compared to two novice groups, one which was a self-control group and another which was yoked to the practice conditions of the expert musicians. The findings showed that the music experts did adopt a successful practice strategy of introducing higher amounts of interference later in practice, as opposed to a progressively less interfering schedule adopted by the novices. The delayed retention results revealed the music experts to be more accurate for two out of the three throws compared to the novice groups (i.e., self-scheduled novice group and the yoked group). The music experts also had significantly more accurate form compared to the novice self-controlled group, with the yoked group, who were paired to the music expert's schedule, not differing from either group.

Thus, the provision of a more effective practice scheduling used by the music experts seemed to thwart the negative effects associated with no choice. These results provide evidence that the knowledge of effective practice methods may be more important for learning than the factor of choice and that further research on this topic is warranted.

### **Coach-Controlled Feedback**

While Hodges et al. (2011) showed that music experts adopted higher quality practice schedules and that a novice yoked to their schedule did as well with the tasks, the comparison was still not from the perspective of a coach-controlled schedule. We argue that learners may benefit from coach-controlled feedback scheduling as compared to self-controlling their feedback schedule and that this may occur for a variety of reasons. One reason might be that coaches have superior knowledge and expertise in both the sport and in motor learning enabling them to provide a more effective schedule than a novice learner. The other reason is that it relates to possible benefits due to the learners' perceptions of the coach having this expertise and knowledge.

Beginning with the coaches' expertise and knowledge, it is well-known that coaches spend a considerable amount of time conveying a range of verbal information to athletes, such as providing technical advice, KP, KR, and praise or scold to an athlete (Cassidy, Jones, & Potrac, 2004). Some of the complexities that come with providing feedback to an athlete are the type, amount, and timing of the augmented feedback (Cassidy et al., 2004). Coaches gain this expertise in a number of ways, such as coaching education, observation of other coaches, and through their own coaching experiences (Ste-Marie & Hancock, 2015). This expertise likely allows coaches to tailor feedback such that it is appropriate for each athlete, enabling learning of the task.

A self-controlled learner who is a novice at the task, however, may not have enough understanding of the learning situation to best determine how to self-control a feedback schedule. Indeed, research has shown that expert athletes in Gaelic football self-selected practice conditions that align better with motor learning principles (e.g., choosing to work on the more difficult skill more and using a practice schedule with more interference) than intermediate level footballers (Coughlan, Williams, McRobert, & Ford, 2014). Moreover, as previously mentioned, expert musicians transferred effective practice habits (e.g., introducing more interference into the second half of practice) from the music domain to the learning of a sport skill, compared to a group of novices that did not introduce this effective practice strategy (Hodges et al., 2011). When self-controlling ones' feedback, this advantage is lost as the learners typically do not have the knowledge of effective motor learning principles that a coach would have.

Given this argument, one would assume that the best research approach would be to compare the learning of a skill when coaches provide feedback as opposed to a learner requesting the feedback. One of the challenges that a researcher would face if directly comparing a "real" coach-controlled feedback schedule to a self-control schedule would be avoiding confounds in the experimental design. For example, it is unlikely that the absolute number of feedback trials and the scheduling of those feedback trials would be the same across the two groups. Moreover, it is likely that one would want to use just one coach in such a design, and thus the generalizability of the results to 'all coaches' would be undermined. Given these issues, it was desirable to have an experimental design in which the absolute feedback frequency and scheduling of that feedback were the same across groups. To this end, the choice for the research design was to instead lead learners to believe that their feedback was being scheduled by a coach who was trying to enhance their learning; i.e., a perceived-coach-controlled situation. The logic

here was that if a learner perceives the coach as competent with the task and knowledgeable in coaching practices, that this would better simulate real-world situations and benefits that might arise from coach-controlled schedules could emerge.

Evidence to support this logic comes from a study that examined the sporting environment and coaching behaviours preferred by Canadian high school athletes (Camiré & Trudel, 2011). The results of this study revealed that athletes prefer coaches that are knowledgeable about the sport they are coaching and those that have experience competing at a high level themselves (Camiré & Trudel, 2011). Coaches that were seen as ineffective were those that had limited knowledge about the sport they were coaching and did not play at high levels themselves. These ideas were also reinforced in another study that examined the relationship between coaches and athletes in youth male soccer clubs. In that study, players also voiced the importance of having knowledgeable coaches who could also do the skills themselves (Hébert, 2000). Further, if the coach was viewed as knowledgeable, it helped the coach gain credibility, which resulted in the athletes being confident with following the feedback provided to them by the coach (Hébert, 2000). These findings highlight the complex nature of a coach providing feedback to an athlete and demonstrate that learners' perceptions of coaches may be critical to their learning of a task.

### **Experimental Design and Hypotheses**

From this literature review, it becomes evident that self-controlled KR practice is a schedule that enhances motor learning when compared to yoked or predetermined schedules. However, the experimental design used in previous experiments is challenged because the comparison group of yoked-learners naïve to the task is not representative of a typical learning environment. Thus, what remains unclear is whether a self-controlled KR schedule is more effective than a schedule that is perceived to be provided by a knowledgeable coach who is

experienced with the task. Therefore, the research question was: *Will a yoked group who perceives that their feedback schedule is controlled by a coach have greater motor skill learning compared to both a traditional yoked group and a self-controlled group?* To this end, participants learned a golf putting task in one of three groups; (1) a self-control (SC) group , (2) a traditional yoked (TY) group , and (3) a yoked group who were duped into believing that their feedback schedule was being controlled by a knowledgeable and experienced golf coach (perceived coach-controlled group; PCC). By duping the yoked group, it allowed us to avoid the confounds previously mentioned.

It was hypothesized that the SC group would have greater learning compared to the TY group, consistent with previous literature (McKay, Carter, & Ste-Marie, 2014). It was also hypothesized that the PCC group would have superior learning than the TY group, given the possible influence of the coach on the perceptions of the learner (Camiré & Trudel, 2011). Lastly, it was uncertain if the PCC group would be statistically different from the SC group due to the fact that the PCC group still did not get feedback in a performance-contingent manner compared to the SC group who did.

### **Chapter 3: Method**

Prior to beginning data collection, the experimental design and analyses were registered on AsPredicted.org as recommended by Lohse, Buchanan, and Miller (2016). For a copy of the pre-registration made available for peer-review see <http://aspredicted.org/blind.php?x=ut22x7>.

## Participants

Sixty young adults with limited golf experience (34 females and 26 males,  $N = 60$ ,  $M_{age} = 21.77$ ,  $SD = 2.72$ , see Table 1 for detailed descriptive data per group) provided written informed consent to an institution-approved research protocol. Sample size was determined with an *a priori* power calculation providing 80% power ( $\alpha \leq .05$ ) to detect a moderate-sized effect ( $f^2 = .15$ ) of self-controlled feedback schedules on motor skill learning, controlling for baseline (pre-test) performance and group in a multiple regression (Faul, Erdfelder, Lang, & Buchner, 2007). The power calculation yielded a sample of  $N = 55$ , but 5 additional participants were added to create equal *n/group* and to account for attrition of data.

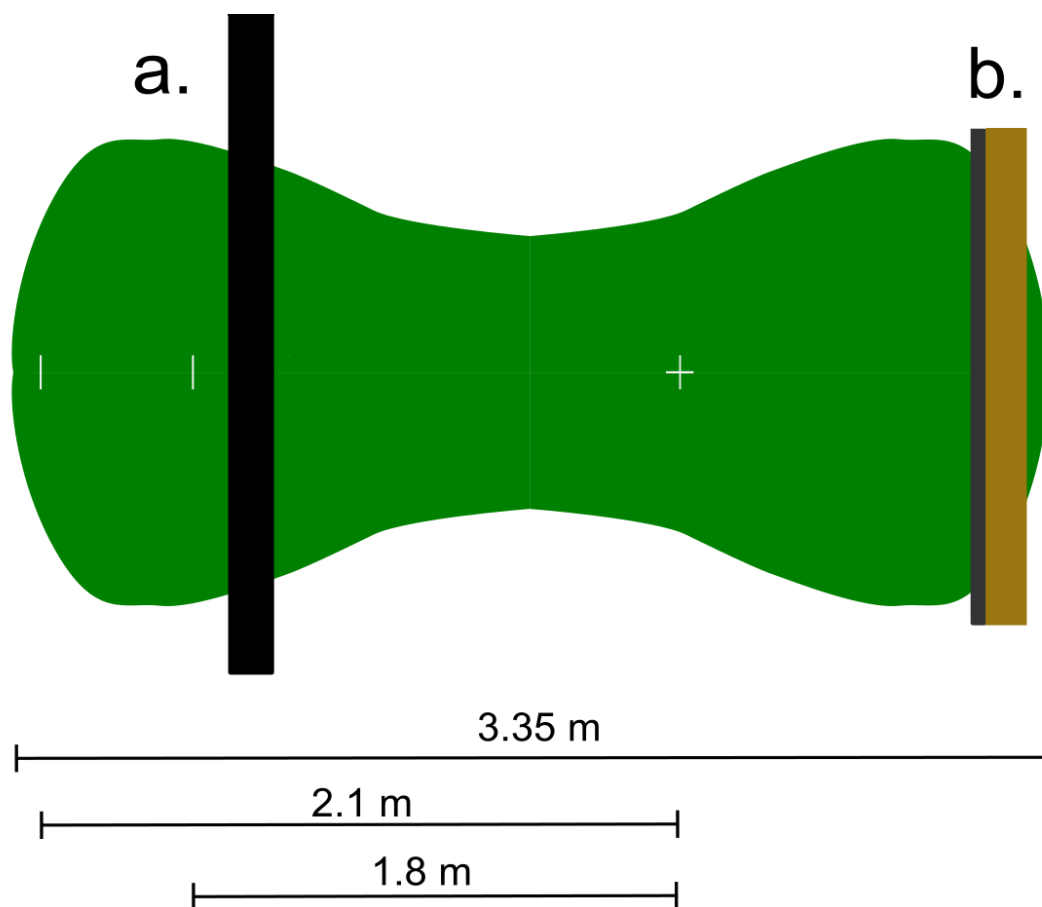
All participants were naïve to the purpose of the experiment, had no self-reported history of sensory or motor dysfunctions, and had limited golf experience (had  $\leq 10$  putting experiences during the past year). A putting experience was defined as anything from one round of golf on a standard 18-hole golf course to one round of miniature putt. One participant was excluded from data collection and a subsequent participant was recruited after finding out after the Day 1 procedure that they were still suffering from concussion symptoms. Two researchers independently recruited participants by word-of-mouth from the university undergraduate and graduate student populations. The first four participants of the experiment were assigned to the SC group; thereafter, the rest of the participants were randomly assigned to one of the three groups while yoking by putting handedness. The quasi-random assignment procedure was

completed to have a set of SC participants' feedback schedules available for pairing with a yoked counterpart.

### **Task and Materials**

**Task.** The experimental task was a short-distance golf putt with the goal of stopping the golf ball directly on top of the target (see Figure 1). Participants used standard left-handed and right-handed blade putters depending on handedness to putt a standard golf ball (5 white Wilson Staff Fifty-Fifty golf balls were used) on an artificial grass surface to a target dot that was marked using white chalk. The objective was to stop the centre of the golf ball directly over top of the target dot.

**Experimental set-up.** For this golf putting task, the putt length was 2.1 m for the pre-test, acquisition, and retention phases. During the transfer phase, the putt distance was shortened to 1.8 m. The target location remained consistent for all phases of the experiment, therefore the starting location moved forward for the transfer phase. The starting locations were indicated by 3 cm black lines that were marked using a permanent marker. The putt lengths were similar to distances used in other motor learning paradigms which have used 1.2 m (Daou, Buchanan, Lindsey, Lohse, & Miller, 2016; Daou, Lohse, & Miller, 2016) and 2.0 m (Bahmani, Wulf, Ghadiri, Karimi, & Lewthwaite, 2017; Chauvel, Wulf, & Maquestiaux, 2015) putt distances. Data collection occurred in a laboratory setting on an artificial practice putting green (GS1018, JEF- World of Golf). The putting green was nominally flat and had an hourglass shape with a maximum length of 3.35 m and a maximum width of 0.91 m (see Figure 1). The artificial green was a dual-speed green designed to stimulate both slow and fast green speeds by having an integrated down-grain and against the grain turf finish, therefore all putts were completed in the fast green speed direction (down-grain) in order to increase the difficulty of the task.



*Figure 1.* Schematic representation of the putting task from an overhead view. The diagram shows the barrier (a.) that allowed the ball to travel to the target unobstructed but occluded the participant's vision of the final endpoint of the ball. For the transfer phase, the barrier was moved back to be the same distance away from the participant as during the other phases. The backstop (b.) stopped balls from rolling off the mat in the y-axis. Putt distances for the pre-test, acquisition, and retention phases was 2.1 m, whereas transfer was 1.8 m.

A 1.78 x 1.27 m barrier was placed between the starting position and the target location to occlude the participants' visual feedback. The barrier had space to allow the golf ball to travel underneath so that it would travel unobstructed to the target. A backstop was placed 0.9 m behind the target in order to stop the golf ball from rolling off the surface. The foam-padded backstop consisted of a 2 x 4-inch board (96.5 cm long) that was affixed to the artificial surface using velcro. Foam padding was fixed to the backstop for two reasons; (1) to remove the sound from the ball hitting the backstop, thus removing any possible auditory feedback, and (2) to reduce any rebound off of the backstop so that the x coordinate of the ball could be accurately measured.

**Scoring system.** A camera system in conjunction with a National Instruments Laboratory Virtual Instrumentation Engineering Workbench (LabVIEW) program was used to measure all putts. The program was a modified version of the one that was developed by Neumann and Thomas (2008). The main modifications were enabling the program to measure and score putts on-line to provide the participants with KR and customizing the program to our experimental paradigm (e.g., number of trials, yoking procedure). A Sony IMX178 camera (Model: BFS-U3-63S4C-C) fitted with an 8 mm fixed focal length Tamron lens (Model: LENS-80T4C) was mounted to a custom-made bracket anchored to a beam. The camera was connected with a 3 m USB 3.0 cable to a data collection laptop and positioned approximately 3.35 m above the putting surface. The camera was positioned parallel to ground level around the target hole, which was achieved by having a circular level attached to the camera. The camera system measured all putts during the experiment and saved the data to a .txt file and saved the photographs of the putts as a .jpg file.

To evaluate the accuracy and consistency of the camera system, we took a sample of 40 putts in which the ball was placed randomly from 0 to 125 cm in each of the four quadrants around the target. The sample of 40 putts had 20 putts collected on day 1 and the next 20 putts collected a week later (day 8). Photographs were taken at each ball position and putts were scored using the camera system as well as by hand, which entailed measuring the distance from the centre of the ball to the target using a tape measure. This procedure was similar to the one used by Neumann and Thomas (2008). We used the concordance correlation coefficient ( $p_c$ ) proposed by Lin (1989) to compare the measurements of the camera system to that of hand measurements. The  $p_c$  method is able to quantify how closely observations fall on the regression line and how close the regression line is to 45° line of perfect agreement, thus the correlation provides information of precision ( $p_c$ ) and accuracy ( $C\beta$ ) (Morgan & Aban, 2016). The results of the correlation were  $p_c = .998$  and  $C\beta = .999$  demonstrating very high levels of precision and accuracy (McBride, 2005).

**Materials.** A demographic questionnaire was used that collected information on the age, gender, putting handedness, and putting experience over the last five years and within the past year of each participant (see Appendix A). Putting handedness was determined by asking participants which way they preferred to swing a baseball bat. The demographic information along with participant information (e.g., Group) was recorded using the LabVIEW program. Instructions for all phases were displayed on a laptop using Microsoft PowerPoint (PPT). A cellphone timer was used to record the KR-delay interval and intertrial interval.

Two questionnaires were used; a pre-experiment questionnaire and a post-experiment questionnaire. All questionnaires were administered through Microsoft Excel on a laptop and saved to a Dropbox folder. The pre-experiment questionnaire consisted of two questions (see

Appendix B) which asked participants to (1) record the number of organized sports that they played which involved the use of a striking implement (e.g., baseball, hockey, tennis, etc.), and (2) to rate their satisfaction with past coaches and/or instructors on a 7-point Likert scale (1 = *poor* and 7 = *excellent*), which included experiences from both sport and art domains.

The post-experiment questionnaire consisted of different questions for each experimental group (see Appendix C). All groups were asked if they found the feedback meaningful and responded according to a 7-point Likert scale (1 = *Not true at all* and 7 = *Very true*). Both yoked groups (TY and PCC) were asked whether feedback was provided when desired and responded to the same 7-point Likert scale. Yoked groups were also asked when they would have liked to receive feedback (e.g., perceived good trials, perceived bad trials, perceived bad and good trials equally, randomly, or other). Whereas SC participants were asked for which trials they asked for feedback and for which trials they did not ask for feedback. All participants were also asked what manner they would have preferred to have their feedback scheduled with this experimental task and could select from the following options; schedule feedback on your own, have a golf coach schedule it, have a predetermined schedule, or other please list. Participants in the PCC group were also asked to respond on a 7-point Likert scale (1 = *Not true at all* and 7 = *Very true*) to the following two statements; (1) if they thought the golf coach was knowledgeable and (2) if they thought the golf coach had knowledge of effective feedback schedules and motor learning principles. Lastly, the PCC group was asked if they had any reason to believe that the coach was not an actual golf coach.

## **Procedure**

Upon arrival at the laboratory, participants completed the consent form. After consenting, participants completed the pre-experiment questionnaire and were then verbally queried for the

demographic information. Next, a calibration process was implemented which involved capturing an image of a calibration board (1 m 2x4 board) and setting up a reference system to measure the distance that the ball would land from the target (see Appendix D for further details). Prior to all experimental phases, participants were read a series of instructions aloud that were simultaneously presented on a laptop.

**Pre-test.** Before beginning the pre-test, participants were provided with the opportunity to examine the target and the putt length however, participants were not informed of the putt length in terms of distance. The pre-test consisted of one block of ten putts, and participants were provided with five golf balls which they could place in front of them for each putt. The golf balls were returned to them after five putts. After the first putt was taken, participants were given a 15 s intertrial delay. During this delay the putt was scored by the experimenter, then the ball was removed from the putting surface, and then the participant was instructed to putt when ready (see Figure 2 for an outline of the intertrial interval). As the purpose of this phase was to determine the participants' baseline skill level, KR was not provided.

**Acquisition.** After the pre-test, participants were provided with instructions corresponding to the group to which they were quasi-randomly assigned to. These instructions consisted of a description of the task goal, how KR would be scheduled, and how KR would be provided. KR consisted of the direction of their error (e.g., long, short, left or right) and the magnitude of their error in centimeters for both the x and y dimensions. To inform participants of how to interpret the KR that was provided, there was a break in the instruction slides where the participant and the primary researcher walked over to the target location. Next, a golf ball was placed at a short distance away from the target and participants were informed how the KR would be provided to them if their ball had landed in that location (e.g., "20 cm long and 5 cm to

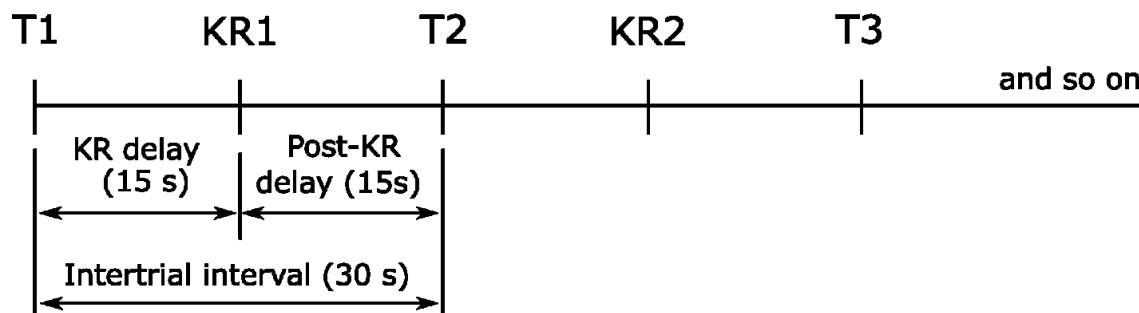
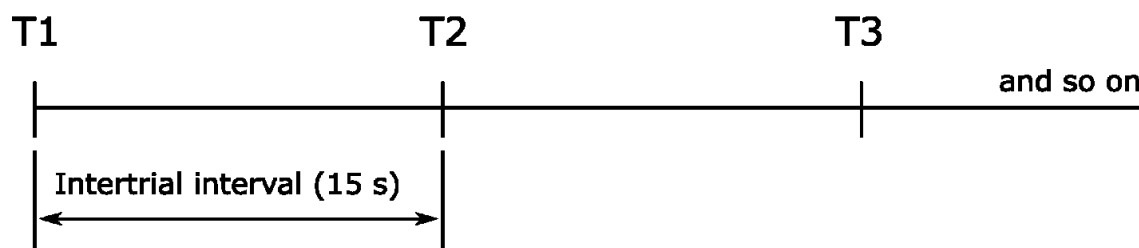
the right”). Then the ball was moved to a second location and the participant was instructed to respond with the appropriate KR statement, this process was repeated at least twice up until the participants reported the correct magnitude and direction. All groups were also instructed that the maximal putt length was 90 cm past the target due to the backstop.

The SC participants were instructed that they would be verbally prompted after each trial during acquisition as to whether they would like to receive feedback (or not) for the just-completed trial. The SC group had no constraint on the amount of KR that they could request. The TY group was instructed that they did not have choice over their feedback schedule and instead it would be provided according to a predetermined schedule. The TY participants received a KR schedule that was identical to their SC counterparts.

The PCC group was informed that they did not have choice over their feedback schedule but would instead have a knowledgeable golf coach determine their KR schedule. In actuality, the PCC participants were also yoked to a participant in the SC group, like that of the TY group. The person providing the feedback was under the guise of being a coach was a third-year undergraduate student. Importantly, the golf coach was only introduced to the participants after the pre-test phase. The PCC participants were told that the coach determining their feedback schedule had experience coaching golf and experience competing at the university level. Further, the participants were also instructed that the golf coach was part of an undergraduate research opportunity program (UROP) and received a scholarship to help with the experiment. These instructions were provided to ensure that the PCC participants were under the impression that their feedback schedule was controlled by a coach with task and motor learning expertise. Lastly, the PCC participants were instructed that the golf coach could not provide any other information

or feedback beyond the KR. The golf coach also wore university golf apparel during testing to further influence the learners' perceptions.

Participants completed ten blocks of five trials (50 total trials). Intertrial intervals were approximately 30 s, which started when the golf ball stopped moving (time 0 s) and ended when the participant was instructed to putt when ready (time 30 s; see Figure 2 for an outline of the temporal sequence). Once the golf ball stopped, the experimenter would start the timer, click the "Score Trial" button, and then click the centre of the golf ball to measure the putt. If the participant was in the SC group, the experimenter would verbally prompt the participant to determine if they wanted to receive KR at the 10 s mark. If KR was requested by an SC participant, the experimenter provided the KR at time ~15 s. Participants in the TY and PCC were not prompted, and if it was a KR-trial, they were provided with KR verbally ~15 s after the stoppage of the ball from the primary researcher (TY) or the golf coach (PCC) respectively. If it was a KR trial, and the golf ball came in contact with the backstop, participants were instructed that their putt was over 90 cm long and provided with the appropriate x-axis feedback. If the ball rolled off the mat in the x-dimension and if it was a KR trial the participants were told, "Your ball rolled off to the right (or left) of the mat". The remaining time of the 30 s intertrial interval was filled with the experimenter removing the ball from the putting surface and the balls being returned after every block during acquisition. After the acquisition phase, participants were thanked, reminded of the time of testing the next day, and were instructed not to engage in any more practice before the final testing session.

**a.****b.**

*Figure 2.* Temporal events in a typical trial as a function of experimental phase (a.) represents the intertrial interval for a trial during acquisition, whereas (b.) represents the intertrial interval during the pre-test, retention, and transfer. For all phases, the intertrial interval started when the ball stopped rolling and ended when the participant was prompted to putt when ready.

All participants were tested during the acquisition phase with three individuals present in the laboratory; the participant, the primary researcher who controlled the LabVIEW program, and either the golf coach for the PCC group or another researcher who was introduced as an undergraduate student helping in the lab for the SC and TY groups. For the SC and TY groups, the undergraduate student would help with the experimental set-up and observe the participant from the same position as the golf coach for the PCC group. Having two experimenters present

in the laboratory for the three experimental groups controlled for possible differences between the groups being due to having an extra person observing the learner during acquisition.

**Retention and transfer.** Approximately 24-hr after completing the acquisition phase, participants returned to the laboratory to complete the experiment. Participants were provided with instructions that reminded the participant of the task goal, the number of trials, and that KR would not be provided. Participants were handed the same putter that they used for day 1. Both tests consisted of one block of ten trials without KR. Ten trials were chosen to mitigate the effects of a warm-up decrement and to have an adequate sample to determine the effects of learning (Schmidt & Lee, 2011). Consistent with the pre-test, putts were paced with a 15 s delay between trials to allow for the location to be measured and recorded.

For the transfer test, the participants were instructed that the starting location had been moved forward making the putt shorter, they were also allowed to examine the target and new starting location before beginning the test. After the transfer test, participants completed the post-experiment questionnaire. Finally, participants were debriefed about the purpose of the experiment, and PCC participants were told that they were deceived into believing that a golf coach was controlling their feedback schedule and asked if they were willing to have their data included in the experiment despite this deception (all participants agreed).

### **Dependent Measures**

Given that the goal of the task was to perform the golf putt with maximum accuracy by stopping the ball directly over top of the target, an outcome error measure was selected. Moreover, because the golf putting task was a two-dimensional (2-D) task it is important for the error measurement to reflect this. In previous motor learning literature, many target accuracy (2-D) tasks including golf putting have measured error using a point system that consists of

concentric circles (Badami, Vaez Mousavi, Wulf, & Namazizadeh, 2011; Lewthwaite et al., 2015). According to Fischman (2015), this measurement technique is inappropriate as it assigns one numerical value to represent every point on each ring around the bull's eye, thus not determining any trial-to-trial variability of the responses. Furthermore, using the concentric circles' measurement system is less precise compared to measuring the placement of the ball in 2-D. To this end, putts were measured as coordinates (x, y) with putts that hit the backstop given a maximum y-score of 90 cm, consistent with the methods used by Chauvel et al. (2015) and Bahmani et al. (2017). It was not expected that putts would go off the mat in the x-axis because of the short distance of the putts (1.8 and 2.1 m). However, if putts went off the mat in the x-axis the trial was removed from all analyses. Measurements were made from the centre of the target to the centre of the golf ball and were measured to the nearest millimeter. The dependent measures for the experiment were mean radial error (MRE) and bivariate variable error (BVE). MRE is a measure of error that is analogous to the absolute error used in one-dimensional (1-D) tasks, therefore it represents the overall accuracy of performance without regard for the direction of the error (Hancock, Butler, & Fischman, 1995). BVE is a measure of consistency that is analogous to variable error for 1-D tasks and represents the dispersion of results. As recommended by Hancock et al. (1995), MRE was calculated as the average distance of each putt from the centre of the target and uses the following equation:  $MRE = \overline{RE} = \left(\frac{1}{m}\right) \sum_{i=1}^m (RE_i)$  where  $m$  is the number of putts. BVE was calculated with the following equation:  $BVE = \left\{ \left(\frac{1}{k}\right) \sum_{i=1}^k [(x_i - x_c)^2 + (y_i - y_c)^2] \right\}^{1/2}$ , where  $k$  = trials in a block and  $c$  = centroid along the given axis (x or y) for that given block (Hancock et al., 1995). The "centroid" represents the average x and y values of the missed putts ( $x_c, y_c$ ), from one participant for a given block (Hancock et al., 1995).

## Statistical Analyses

One-way analyses of variance (ANOVAs) were conducted to verify that there were no group differences with respect to age, past five-year putting experience, past year putting experience, number of sports played with striking implements, participants' ratings of their previous experiences being coached or instructed, and finding the feedback meaningful. Additionally, an independent sample t-test was conducted for the yoked groups' ratings of receiving feedback when they would have wanted it, whereas a chi-square test was conducted for gender.

During the pre-test, 17.2% of trials hit the backstop and were therefore trimmed to be 90 cm long, this percentage was lower for all other phases of the experiment with 3.9% in acquisition, 8.3% in retention, and 2.8% in transfer. Additionally, 0.5% of putts were shorter than 140 cm during the pre-test phase meaning they were not visible for the camera to get an accurate measurement, therefore these putts were trimmed and given a y-axis score of -140. During acquisition, 0.1% of putts were less than -140 cm, followed by 0% in retention and 0.2% in transfer. Lastly, 6 putts (2 pre-test, 1 acquisition, 1 retention, & 2 transfer) were excluded from analysis due to either rolling off the mat in the x-axis, being hit from the wrong starting position, or a computer-camera system malfunction.

Univariate outliers were identified using the median absolute deviation (MAD; Leys, Ley, Klein, Bernard, & Licata, 2013) method as specified in the pre-registration. As Leys et al. (2013) have stated, the most common method of detecting an outlier, using the mean plus/minus three standard deviations, is problematic because the mean is heavily influenced by outlier points and this method is unlikely to detect outliers in small sample sizes. In contrast, the MAD procedure is less sensitive to outliers in the data set, and less influenced by sample size, making

it a more appropriate method. We searched for outliers within each group by averaging the two post-test scores (retention and transfer) together for MRE and BVE separately and used the very conservative criterion value (i.e., 3). Importantly, outliers were only searched for and removed from the data set in the post-test phase given that the main interest is the effects of the independent variable on learning (Schmidt & Bjork, 1992). We identified two univariate outliers within the post-test data for MRE and no outliers for BVE. One outlier was from the PCC group ( $M = 63.40$ , Distance from the Upper Criterion = 5.75, 0 putts in the post-test hit the backstop) and one from the TY group ( $M = 83.96$ , Distance from the Upper Criterion = 2.86, 14 putts in the post-test hit the backstop).

To examine baseline performance, a one-way ANOVA was completed for MRE and BVE for the pre-test phase. Next, to examine performance during acquisition, the dependent variables of MRE and BVE were analyzed in separate mixed-factor analyses of covariance (ANCOVAs) with the between-subject factor of Group (3 levels: SC, TY, & PCC) and the within-subject factor of Block (10 levels: 1-10), controlling for pre-test performance as specified in the pre-registration. In order to measure learning, MRE and BVE was analyzed using separate mixed-factor ANCOVAs with Group (3 levels: SC, TY, & PCC) as the between-subject factor and Test (2 levels: retention, & transfer) the within-subject factor and pre-test MRE or BVE serving as the covariate depending on the ANCOVAs dependent variable.

Of note, these analyses were specified in the pre-registration, however, further reading on pitfalls to be avoided when using an ANCOVA with between-subject classification and within-subject experimental designs (Schneider, Avivi-Reich, & Mozuraitis, 2015) led to the adoption of additional procedures. The first procedure adopted was the centering of the covariate across all the participants in the experiment to withhold the validity of the test (Delaney & Maxwell,

1981; Schneider et al., 2015). Thus, the analyses just stated used centered covariates. Secondly, because an ANCOVA does not provide a valid test of the within-subject effects due to the mean square for the within-subject main effect being contaminated by the presence of the variance for the covariate, it has been recommended that researchers remove the covariate and employ a standard ANOVA to evaluate within-subject effects (Schneider et al., 2015). Therefore, we used ANCOVAs to evaluate all between-subject effects and any interactions, while the within-subject main effects were evaluated with ANOVAs, as recommended by Schneider et al. (2015). For the acquisition analyses, and the within-subject effect of Block, the results were evaluated using a 3 (Group: SC, TY, & PCC) x 10 (Block: 1-10) mixed-factor ANOVA with repeated measures on Block for both dependent variables. For the learning analyses, the within-subject factor of Test was evaluated using a mixed-factor ANOVA with Group (3 levels: SC, TY, & PCC) as the between-subject factor and Test (2 levels: retention, & transfer) as the within-subject factor for both MRE and BVE.

The within-subject effects are expressed in the results as unadjusted means with standard error, whereas the between-subject effects and interactions are expressed as adjusted means for the covariate with standard error or 95% confidence intervals (CIs). Differences with a probability of  $<.05$  were considered significant. Greenhouse-Geisser (G-G) or Huynh-Feldt corrected degrees of freedom was used to correct for any violations of sphericity depending on the value of the G-G epsilon (if G-G epsilon ( $\epsilon$ )  $> .75$  the Huynh-Feldt correction was used). Partial eta squared ( $\eta_p^2$ ) was reported as an estimate of effect size and Bonferroni post hoc procedures were conducted for any significant ANCOVAs or ANOVAs. All analyses were completed using the statistical software program SPSS for Windows (IBM Inc., Armonk, NY, USA).

## **Chapter 4: Results**

### Demographics, Previous Putting and Sport Experience

Demographic information was collected to ensure that the groups did not differ on variables that could affect the performance and learning outcomes. Analyses of these variables showed no significant differences between groups. Specifically, groups did not differ with respect to age, gender, past five-year putting experience, past year putting experience, number of experiences with striking implement sports, and their ratings of their previous experiences being coached or instructed ( $ps \leq .338$ ; see Table 1).

Table 1.

*Descriptive Data for the Three Experimental Groups.*

Group	SC ( $n = 20$ ; 14 females)		TY ( $n = 20$ ; 10 females)		PCC ( $n = 20$ ; 10 females)	
	<i>M</i>	<i>CI</i>	<i>M</i>	<i>CI</i>	<i>M</i>	<i>CI</i>
Age (years)	22.1	20.7-23.4	22.0	20.5-23.4	21.3	20.3-22.3
Past five-year putting experience	5.80	2.59-9.01	4.95	0.87-9.03	7.10	3.05-11.1
Past year putting experience	1.15	0.57-1.72	1.15	0.15-2.15	1.05	0.54-1.56
Number of experiences with striking implement sports (e.g., hockey or baseball)	1.05	0.45-1.65	1.16 <sup>a</sup>	0.49-1.83	1.50	0.83-2.17
Overall experience being coached or instructed (1 poor -7 excellent)	5.60	5.19-6.01	5.89 <sup>b</sup>	5.51-6.27	5.70	5.17-6.23

*Note.* *M* = Means; *CI* = 95% Confidence Intervals

<sup>a</sup> Missing data from one yoked participant ( $n = 19$ ). <sup>b</sup> Missing data from two yoked participants ( $n = 18$ ).

### Pre-Test

A one-way ANOVA for MRE on pre-test performance was not significant,  $F(2,59) = 0.49$ ,  $p = .953$ . Further, the one-way ANOVA for pre-test BVE was not significant,  $F(2,59) = 1.10$ ,  $p = .341$ , demonstrating that the groups were not different in baseline performance.

## Acquisition

**Mean radial error.** MRE decreased across practice blocks for all groups (Figure 3). The ANCOVA revealed non-significant findings for both the main effect of Group,  $F(2,56) = 1.28, p = .285, \eta_p^2 = .044$  and the interaction of Group\* Block,  $F(16.6,464.8) = 0.97, p = .492, \eta_p^2 = .033$ . The covariate, pre-test MRE, was significantly related to the MRE during acquisition,  $F(1,56) = 16.08, p < .001, r = .464$ . The ANOVA revealed a significant main effect of Block during acquisition for MRE,  $F(8.26, 470.6) = 4.95, p < .001, \eta_p^2 = .080$ , where block 1 was significantly different from blocks 4 ( $p = .035$ ), 8 ( $p = .003$ ), 9 ( $p = .002$ ), and 10 ( $p < .001$ ). No other significant differences between blocks were found.

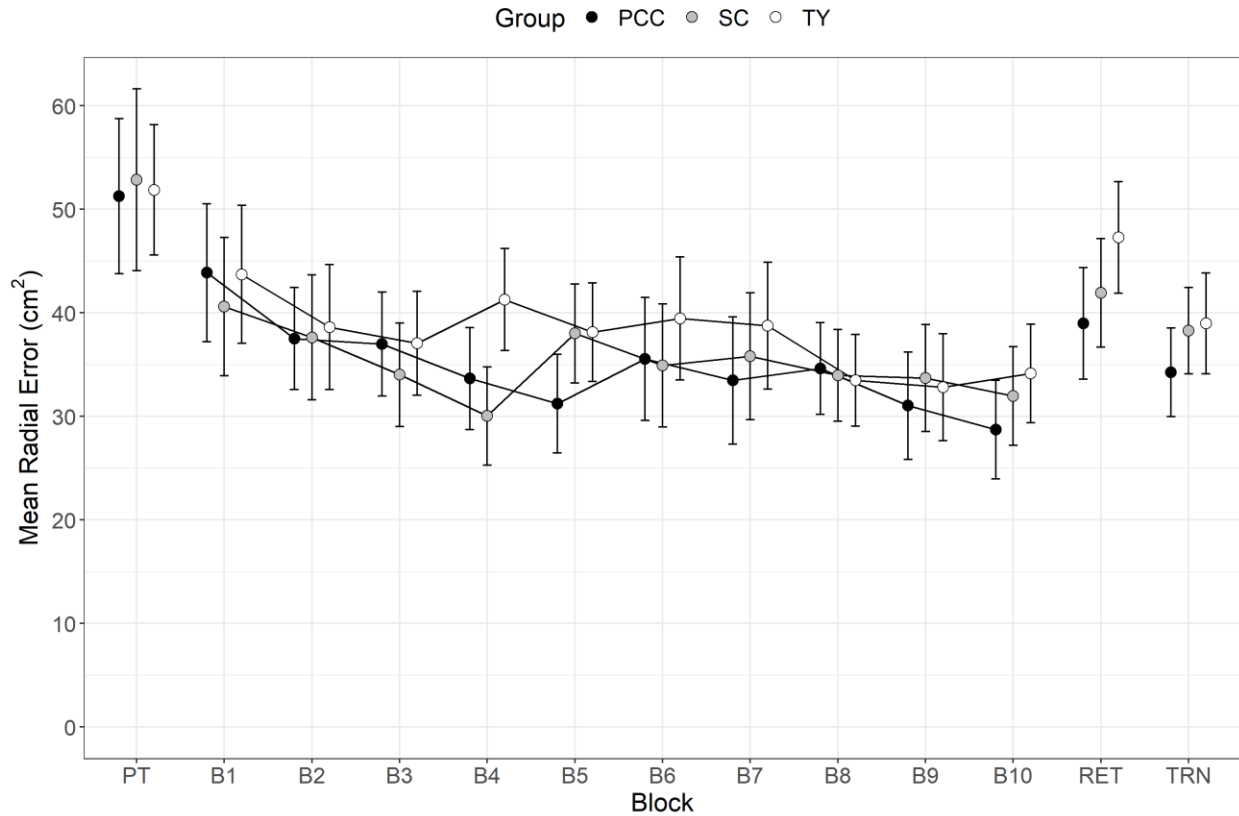
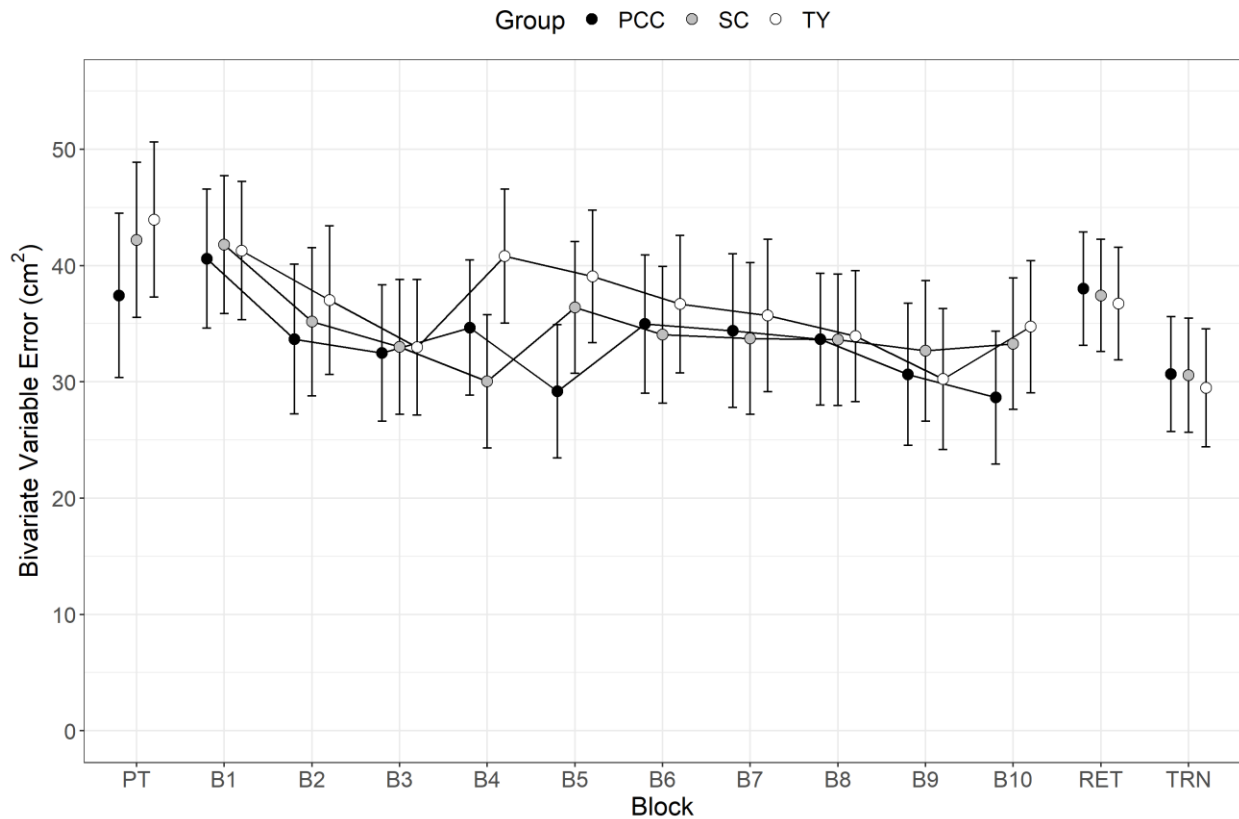


Figure 3. Adjusted mean radial error (with 95% confidence intervals) for acquisition (B1-B10), retention (RET), and transfer (TRN) phases of the experiment (evaluated at the pre-test centered average of  $-0.304$ ). Unadjusted group means are displayed for the pre-test (PT). Each acquisition block consisted of five trials, whereas PT, RET, and TRN consisted of ten trials.

**Bivariate variable error.** Figure 4 shows BVE for the groups across all phases of the experiment. The ANCOVA revealed a non-significant main effect of Group,  $F(2,56) = 0.94$ ,  $p = .397$ ,  $\eta_p^2 = .032$  and non-significant interaction of Group x Block,  $F(18,504) = 0.78$ ,  $p = .729$ ,  $\eta_p^2 = .027$ . The covariate, pre-test BVE, was significantly related to the BVE during acquisition,  $F(1,56) = 6.22$ ,  $p = .016$ ,  $r = .344$ . The ANOVA revealed a significant main effect of Block

during acquisition for BVE,  $F(9, 513) = 3.10$ ,  $p = .001$ ,  $\eta_p^2 = .052$ , where block 1 was significantly different from blocks 3 ( $p = .014$ ), 9 ( $p = .008$ ), and 10 ( $p = .002$ ), with no other differences being significant.



*Figure 4.* Adjusted means (with 95% confidence intervals) for bivariate variable error during the acquisition (B1-B10), retention (RET), and transfer (TRN) phases of the experiment (evaluated at the pre-test centered average of  $4.3 \times 10^{-5}$ ). Unadjusted group means are displayed for the pre-test (PT). Each acquisition block consisted of five trials, whereas PT, RET, and TRN consisted of ten trials.

**KR scheduling within acquisition.** Although the absolute and relative frequency of feedback was controlled for between groups, the self-control participants were still free to select different frequencies. The average amount of KR trials for a self-control participant was 36.1 ( $SE = 2.12$ ) out of 50 allocations, which corresponds to a relative KR frequency of 72.3%. However, of note, is that the range of KR requests was very large (Range = 40 trials) with one participant requesting KR for a total of 49 trials (98%) and another requesting KR on only 9 trials (18%). Overall, the proportion of KR requests per block during acquisition showed a faded schedule with the greatest frequency of requests occurring in the first block (88%), dropping to 69% in the second block, with similar percentages noted thereafter; 72%, 72%, 69%, 66%, 63%, 69%, 78%, and 77%.

### **Learning (Retention and Transfer Performance)**

In retention (Figure 3), at a descriptive level, the PCC group ( $M = 38.92$ ,  $SE = 2.64$ ) had the lowest adjusted MRE, followed by the SC group ( $M = 41.71$ ,  $SE = 2.57$ ), and then the TY group ( $M = 47.19$ ,  $SE = 2.64$ ). In transfer (Figure 3), the same pattern of results emerged; the PCC group ( $M = 34.25$ ,  $SE = 2.18$ ) had the lowest adjusted MRE, followed by the SC group ( $M = 38.27$ ,  $SE = 2.13$ ), and then the TY group ( $M = 38.97$ ,  $SE = 2.18$ ). The ANCOVA, however, revealed that these differences were not significant; main effect of Group,  $F(2, 54) = 2.81$ ,  $p = .069$ ,  $\eta_p^2 = .094$  and Group x Test interaction,  $F(2, 54) = 0.71$ ,  $p = .497$ ,  $\eta_p^2 = .026$ . Further, the covariate, pre-test MRE, was not significantly related to post-test performance,  $F(1, 54) = 0.97$ ,  $p = .329$ ,  $r = .133$ . The ANOVA for MRE had a significant main effect for Test,  $F(1, 55) = 10.69$ ,  $p = .002$ ,  $\eta_p^2 = .163$ , in which transfer ( $M = 37.18$ ,  $SE = 1.22$ ) performance was significantly more accurate than retention ( $M = 42.71$ ,  $SE = 1.60$ ). A sensitivity analysis was

conducted leaving the two univariate outliers in the sample and this analysis did not change the significance of the results.

For BVE, the same pattern did not emerge in retention and transfer as the group means were clustered closely together for both post-tests (Figure 4). The main effect of Group,  $F(2, 56) = 0.10, p = .903, \eta_p^2 = .004$ , and the Group x Test interaction,  $F(2, 56) = 0.008, p = .992, \eta_p^2 < .001$  were not significant. The covariate, pre-test BVE, was significantly related to the post-test performance,  $F(1, 56) = 8.11, p = .006, r = .351$ . The ANOVA for BVE had a significant main effect for Test,  $F(1, 57) = 19.71, p < .001, \eta_p^2 = .257$ , in which transfer ( $M = 30.24, SE = 1.45$ ) performance showed significantly less variable performance than retention ( $M = 37.39, SE = 1.46$ ).

### **Post-Experiment Questionnaire Scores**

The groups did not significantly differ with respect to their rating of finding the feedback meaningful (see Table 2 for means),  $F(2, 59) = 0.14, p = .869$ . Further, the PCC and TY groups were not different from one another in regard to their ratings of receiving feedback when desired,  $F(1, 29) = 0.05, p = .822$ . The self-reported questionnaire results are displayed in Table 3. The majority (60%) of SC participants preferred to receive feedback after perceived good trials and, simultaneously avoided feedback after perceived bad trials (80%). Three participants, however, selected the ‘other’ option that was provided and indicated their own schedule was adopted. One of the three participants responded with, “At the beginning (first 10 trials) I wanted to gain insight about where the ball was landing so I asked every trial, then I asked on perceived good trials and occasionally on perceived bad trials.” Another one of the participants stated that they asked for feedback after every trial, while the other responded with, “Every other at the start (i.e.,

first 10), intermittent in the middle (i.e., when I felt it was necessary for 30), and continuous for the last 10.”

Table 2.

*Post-Experiment Questionnaire Data for the Three Experimental Groups.*

Group	SC ( <i>n</i> = 20)		TY ( <i>n</i> = 20)		PCC ( <i>n</i> = 20)	
	<i>M</i>	<i>CI</i>	<i>M</i>	<i>CI</i>	<i>M</i>	<i>CI</i>
I found the feedback provided was meaningful (1 not true at all – 7 very true)	5.75	5.15-6.36	5.65	5.19-6.11	5.55	4.95-6.15
I found the feedback was provided when I would have wanted it (1 not true at all – 7 very true)	-	-	4.40	3.71-5.09	4.50	3.88-5.12
I thought that the golf coach was knowledgeable (1 not true at all – 7 very true)	-	-	-	-	6.10	5.62-6.58
I thought that the golf coach had knowledge of effective feedback schedules and motor learning principles (1 not true at all – 7 very true)	-	-	-	-	5.20	4.64-5.76

*Note.* *M* = Means; *CI* = 95% Confidence Intervals

In comparison, 45% (9/20) of both the PCC and TY participants would have preferred to receive feedback after perceived good and bad trials equally, and the second-highest option for the TY (35%) and PCC (25%) groups was desiring feedback after perceived good trials. For the question that asked participants for their preferred manner for scheduling feedback, all three groups responded similarly, and these were split between having a golf coach schedule their feedback (*M*; SC = 45%, TY = 40%, PCC = 45%) or them self-controlling their feedback schedule (*M*; SC = 50%, TY = 35%, PCC = 40%).

For the coach-based questions specific to the PCC group; their perceptions as to whether the experimental confederate was a golf coach were high, as were their perceptions of his golf

and motor learning knowledge (see Table 2). Sixteen of the 20 participants from the PCC group indicated that they did not have doubts about the experimental confederate being a golf coach. Three participants clearly responded yes to the manipulation check; indicating that they had doubts at times if the confederate was an actual golf coach, while one other participant responded in an open-ended manner stating, “Would not be surprised, but he made it seem he was knowledgeable and gave me feedback that made sense (e.g., when I felt I shot short, he would tell me it was).” The participants that indicated doubt as to whether the confederate was a coach were retained in all analyses in order to not bias our sample and to retain power from our *a priori* sample size calculation (Aronow, Baron, & Pinson, 2016; Montgomery, Nyhan, & Torres, 2018). Sensitivity analyses were conducted with these four participants excluded and the significance did not change for any main effect or interaction for all MRE and BVE analyses.

Table 3.

*Post-Experiment Survey Results Outlining Preferences for the Receiving of Feedback.*

Group	Number of Responses		
	SC (n =20)	TY (n =20)	PCC (n = 20)
1. Select the option below that best represents when you chose to ask for feedback (SC) or when you would have liked to receive feedback (TY & PCC)?			
a) Perceived good trials	12	5	7
b) Perceived bad trials	2	3	3
c) Perceived bad and good trials equally	3	9	9
d) Randomly	0	0	0
e) Other	3	1	0
f) Not assessed	-	2	1
2. Select the option below that best represents when you did not choose to ask for feedback?			
a) Perceived good trials	2	-	-
b) Perceived bad trials	16	-	-
c) Perceived bad and good trials equally	2	-	-
d) Randomly	0	-	-
e) Other	0	-	-
3. Select the best option below that best represents the manner in which you would have preferred to have your feedback scheduled with this experimental task?			
a) Schedule feedback on my own	10	7	8
b) Have a golf coach schedule my feedback	9	8	9
c) Have a predetermined feedback schedule	1	4	3
d) Other	0	1	0

## **Chapter 5: Discussion**

While it has been consistently shown that self-controlled KR schedules enhance motor learning relative to yoked schedules (see Sanli et al., 2013; Wulf & Lewthwaite, 2016 for reviews), most experiments have only compared a self-control group to a yoked group that is comprised of learners naïve to the task (Yantha et al., 2019). Researchers then use this evidence to make recommendations that learners should be provided with choice over practice variables, including feedback schedules, rather than based on a coaches' discretion (Halperin, Wulf, Vigotsky, Schoenfeld, & Behm, 2018; Gokeler, Neuhaus, Benjaminse, Grooms & Baumeister, 2019, Wulf, 2007). For example, Fagundes et al. (2013) stated, "We encourage teachers to introduce an element of self-control in the learning of dance movements, including self-related modeling" (p. 855). More recently, in a review article highlighting effective motor learning principles for therapists to use in treating an ACL injury, Gokeler et al. (2019, p. 859) suggested that self-controlled feedback schedules will "...positively influence the motor learning process as it can be tailored to individual patients' needs as opposed to depending on generic predetermined schedules".

These recommendations and suggestions are problematic, however, because the self-control research paradigm has not included a comparison group that closely resembles contributions that a coach or therapist may have in an applied learning environment. For example, a coach could provide feedback in a performance-related manner, whereas a yoked group does not have this opportunity. In this context, a coach-led schedule is adaptive and not predetermined or generic, as specified in the preceding example. Moreover, a coach may impact the amount of time a learner interprets and processes the feedback. Specifically, if the feedback is being controlled by a coach who is viewed as knowledgeable, it may cause the learner to process feedback to a greater depth or engage in increased error detection and correction

processes compared to other learners (Grand et al., 2015). We also know that enhanced learning is displayed when learners are provided with conditions that enhance their expectancies (Wulf & Lewthwaite, 2016) and that task interest and enjoyment are increased when the need for competence is satisfied (Ryan & Deci, 2000). Therefore, it is possible that a coach controlling a feedback schedule could influence a learner's motivation and motor learning. Given these points, in the present experiment, we aimed to investigate the possible effects of a perceived coach-controlled feedback schedule compared to a feedback schedule controlled by a learner and a predetermined feedback schedule (i.e., yoked).

It was hypothesized that we would show the customary learning advantages, with the SC group having superior motor learning compared to the TY group, as demonstrated by lower MRE and BVE post-test error scores for the SC group as compared to the TY group. It was also hypothesized that the PCC group would have lower error scores than the TY group given the possible influence of the coach on the learners' perception (Camiré & Trudel, 2011, Hébert, 2000). Finally, considering the comparison between the SC and PCC groups, this was a more difficult prediction. It was recognized that the SC group would have the advantage of receiving feedback in a performance-contingent manner, but, simultaneously, would not have the influence of the coach on the learners' perceptions present. In comparison, the PCC group would not benefit from the performance-contingent KR but may have the anticipated benefit of the perceived influence of the coach providing the feedback. Given that we do not have an understanding of the weighting associated with these potential benefits, it was difficult to predict which of these two groups would be superior, and thus no specific hypotheses were made for this comparison.

In the remaining sections of this discussion, the MRE results are first compared to those just stated *a priori* hypotheses. Next, the BVE results are discussed and possible reasons are provided for the variability measure showing a different pattern of results from the MRE data. Then, the post-experiment questionnaire results are discussed and compared to previous self-controlled experiments. Afterward, possible explanations are provided for the lack of replication noted here from the typical self-controlled learning effects. Lastly, limitations and delimitations, as well as future directions, are provided with a focus on potential mechanisms for which the coach may influence learning.

### **Accuracy**

Contrary, to our first hypothesis the SC group did not have significantly more accurate performance (MRE) during the post-test phase than the TY group, thus failing to replicate previous self-controlled literature (Sanli et al., 2013; Ste-Marie, Carter, & Yantha, 2019). However, both the PCC and SC groups did have numerically lower MRE scores compared to the TY group in both retention and transfer as was hypothesized. Nonetheless, the main effect for group was not significant, albeit the *p*-value was .069. This marginally significant *p*-value might lead one to conjecture that the typical self-controlled KR advantage was simply muted, however, it was the PCC group that had the lowest MRE (see Figure 3). Indeed, in considering the descriptive statistics, the *p*-value was most likely driven by the difference between the PCC group and the TY group (Mean Difference (MD) = -6.56) as opposed to the difference between the SC group and TY group (MD = -2.92). As such, the evidence pointed more to the learning benefits acquired from the perception of a coach providing feedback as opposed to controlling one's KR schedule.

## Variability

It was also expected that the same pattern of results would emerge for BVE, however, both retention and transfer yielded group means that were clustered closely together (see Figure 4). Perhaps this was because a reduction in variability takes more practice, whereas accuracy can change more quickly and plateau after a short period of practice (Schmidt & Lee, 2011). Considering that it has been stated that variability is a more sensitive measure for effects of practice than accuracy, perhaps more practice was still needed for differences in BVE to be noted (Schmidt & Lee, 2011). The results can also be considered within Fitts and Posner's (1967) three-stage view of learning in which performance gains can be dramatic and large in the early stage of learning (i.e., the cognitive stage) than at any other stage, however, performance can still be characterized as quite inconsistent. As the learner moves along into the second and third stages, performance improvements are more gradual and performance starts to become less variable (Fitts & Posner, 1967). If we look at our experiment from the lens of this three-stage model of learning, we know that the participants were novice golf putters. Also, we have removed vision in the task to ensure that participants would be reliant on the KR, but this modification likely makes the task much more difficult. Therefore, it is possible that the participants all remained in the cognitive stage of learning, where performance is characterized by rapid improvements, but inconsistencies. Combine these factors with the fact the acquisition phase only included 50 practice putts, it is thus not surprising that MRE scores showed greater change and distinguished the groups more as compared to BVE.

Also, to consider is that this research is not the first to find different patterns for accuracy and variability measures. Although some self-control experiments have still reported significant differences in variability between self-control and yoked groups during retention for various

practice variables, such as KR (Carter & Patterson, 2012; Patterson, Carter, & Sanli, 2011) and task difficulty (Andrieux, Danna, & Thon, 2012), other experiments have reported no differences (Ali, Fawver, Kim, Fairbrother, & Janelle, 2012; Janelle et al., 1997). However, Ali et al. (2012) did find a significant difference between the self-control group and the yoked group during a delayed transfer test for variability. Regardless, it is still more common for self-controlled experiments to measure and find significant differences in movement form or accuracy scores rather than in measures of variability (Sanli et al., 2013). Future motor learning research interested in variability as a primary dependent variable should look to include more practice, preferably by including more practice sessions or consider using a sample of participants in a later stage of learning.

### **Post-Experiment Questionnaire**

The questionnaire data revealed that self-control participants preferred to request feedback after perceived good trials and that they rarely requested feedback after perceived bad trials, replicating previous literature (Carter, Rathwell, & Ste-Marie, 2016; Chiviacowsky & Wulf, 2002; Chiviacowsky & Wulf, 2005; McRae et al., 2015; Patterson & Carter, 2010). In those studies, yoked participants were typically asked to make a binary decision to the question “Do you think you received KR after the right trials?” In the present experiment, participants were asked to respond using a Likert scale from 1-7, similar to the questionnaire used by Aiken et al. (2012). As can be seen in Table 2, both the PCC and TY groups had moderate ratings (i.e., just above somewhat true) for their feedback schedule lining up with their preferences. What is interesting is that even though the PCC group were led to believe the golf coach was controlling their feedback schedule, they still responded scores close to the TY group indicating that they did not receive a feedback schedule tailored to their own individual needs. This is an important

finding because the PCC group had the lowest MRE numerically in retention and transfer, and this occurred despite not being provided with a feedback schedule that was performance-contingent, or one that fully satisfied their needs.

Further support for the proposition that the PCC group did not receive an ideal feedback schedule came from another questionnaire item associated with the participants' preferences for feedback delivery. One of the advantages of not using a binary decision is that yoked participants were also welcomed to provide information concerning when they would have preferred to receive feedback. Participants in the TY group replicated previous literature showing that they preferred feedback after good trials (Chiviawosky & Wulf, 2002; Fairbrother, Laughlin, & Nguyen, 2012; Patterson & Carter, 2010) and good and bad trials equally (Post et al., 2016). Again, participants in the PCC group responded in accordance with the responses of the yoked group, providing evidence that the feedback schedule they received could have been more individualized to their performance. The questionnaire results taken together with the behavioural measures leaves one to wonder what would occur if the PCC group obtained a feedback schedule that aligned with their preferences. This concept will be expanded on in the future direction section.

### **On the Failure to Replicate Typical Self-Controlled Findings**

Although the learning advantage of self-controlled practice manipulations, and self-controlled feedback, more specifically, are well documented in the literature (Ste-Marie et al., 2019), our results suggest that perhaps the magnitude of these effects have been overestimated and are still not fully understood. In this experiment, we observed the typical pattern of results wherein the SC group had numerically less error than the yoked group during both retention and transfer, nevertheless, this was not statistically significant. This lack of significance occurred in

spite of us having a sample size that was powered to detect a moderate effect size ( $f^2 = .15$ ) and explicitly informing the yoked groups that they were not provided with choice but rather would be provided a predetermined schedule or at the discretion of the coach. Of note is that often in the self-controlled literature participants in yoked groups have not been explicitly denied choice, rather, the common protocol is to tell the participants that they will get feedback according to a predetermined schedule or randomly. Specifically, in this experiment the TY group was informed, “Throughout practice, you will sometimes receive feedback regarding your just completed movement and other times you will not receive feedback. Note, you will not have choice over when to receive feedback and it will instead be provided according to a predetermined feedback schedule”. The rationale for making the yoked groups overtly aware that they were denied choice stemmed from a meta-analysis by Patall, Cooper, and Robinson (2008) on the topic of the effects of choice on intrinsic motivation. In that work, they found that the type of control group was a significant moderator, where the effects of choice were greater when control participants were explicitly denied choice compared to participants who were assigned an option either through random assignment or by an experimenter. Therefore, in the interest of maximizing the possible effects of self-controlled learning, we explicitly denied the yoked groups choice. However, despite these methods used we did not find the typical self-controlled learning advantage. It is recognized, that even an experiment powered at 80% can have a non-significant finding 20% of the time when, in fact, there is a true effect in the population. Therefore, it is possible that a Type II error occurred and that these results are still within the sampling variation of the “true” effect-size for self-controlled learning.

Notwithstanding this possibility of a Type II error, evidence to further support the idea that the effect of size of self-control may be overestimated in the literature was brought forth in a

recent meta-analysis of the self-controlled literature (Yantha et al., 2019). The meta-analysis revealed an unadjusted effect size of self-controlled learning during retention to be a Hedges'  $g = .57$ , 95%  $CI (.44, .69)$  with three outlier experiments removed (Yantha et al., 2019). The caveat is that these results came from a traditional random-effects meta-analysis that did not account for factors such as publication bias or  $p$ -hacking, which have been identified as problematic issues in science (Amrhein, Greenland, & McShane, 2019) and sport and exercise science (Caldwell et al., 2018; Lohse et al., 2016). After completing the traditional random-effects meta-analysis and finding that publication status was a significant moderating variable (i.e., the unpublished experiments had a significantly lower effect-size than the published experiments), the authors proceeded with additional analyses.

First, the authors used a  $p$ -curve analysis; a novel statistical technique that is used to assess the evidential value of a field of literature and may well offer a solution to the age-old problem of the file drawer effect of failed experiments and analyses (Simonsohn, Nelson, & Simmons, 2014). More specifically, the  $p$ -curve analysis considers the distribution of  $p$ -values that are below .05. Figure 5 is used to illustrate how to understand the findings of a  $p$ -curve analysis. To begin, when the null hypothesis is true, the expected distribution would be uniform by definition (the dashed red line in Figure 5) where  $.04 < p < .05$  should occur 1% of the time, and  $.30 < p < .31$  should also occur 1% of the time. Therefore, when a studied effect is non-existent, every  $p$ -value is equally likely to be observed. However, when the null is false, and an effect exists, the  $p$ -curve would have a distribution that is right-skewed, thus demonstrating that there is evidential value, as seen black line in Figure 5. Here, the black line has more low-valued probabilities (e.g., .01s) than high-valued (e.g., .04s)  $p$ -values. Conversely, if a  $p$ -curve is left-skewed, it suggests that the evidence in the literature is lacking, which could be due to effects

such as  $p$ -hacking (the blue line in Figure 5). More recently, Simonsohn, Simmons, & Nelson (2015) spoke to the value of the half  $p$ -curve. The half  $p$ -curve uses the same approach as the full  $p$ -curve, however, instead of the cut-off point being  $p \leq .05$ , it is  $p \leq .025$ . The rationale for using  $p \leq .025$  instead of  $p \leq .05$  was that it is that much more difficult for researchers to  $p$ -hack until .025, therefore the half  $p$ -curve is considered more robust in detecting ambitious  $p$ -hacking (Simonsohn et al., 2015). It is necessary to include both the half and full  $p$ -curve analyses because a half  $p$ -curve is less powerful and has an increased chance for a false-positive, therefore both methods are combined in a single analysis to interpret the data (Simonsohn et al., 2015).

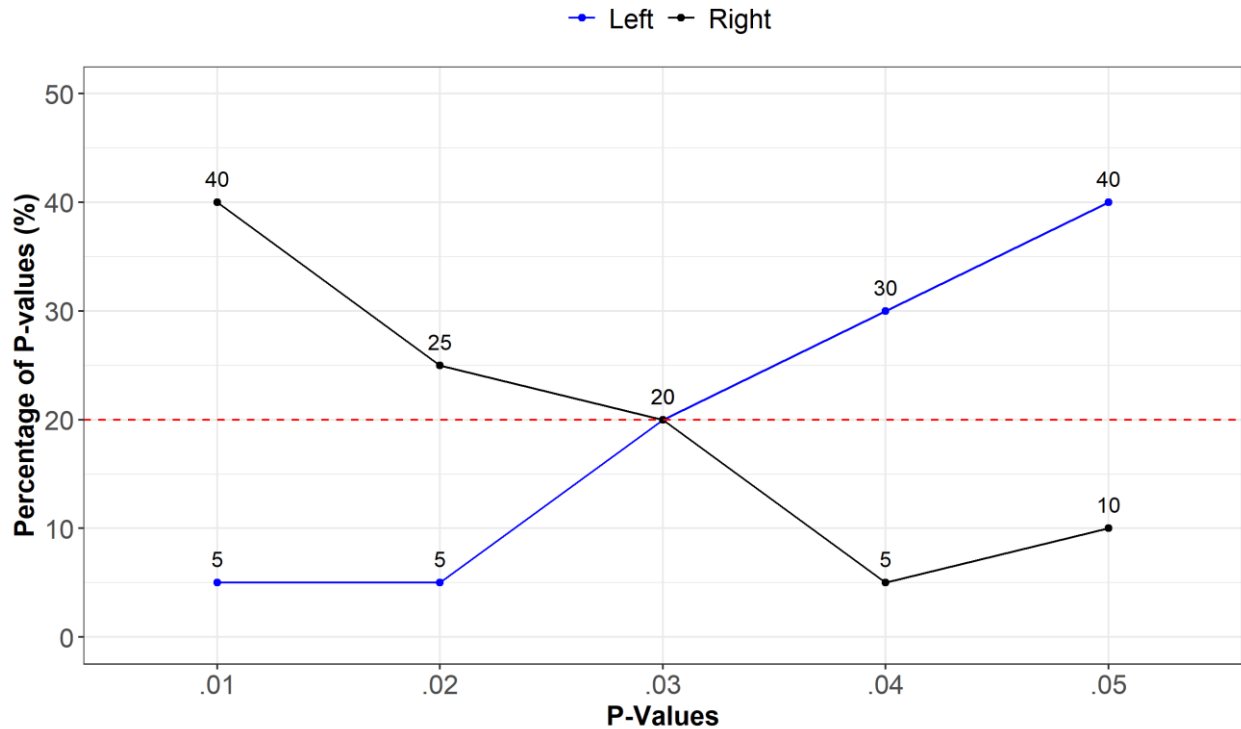


Figure 5. Exemplary data that illustrates the results of a  $p$ -curve in the presence and absence of  $p$ -hacking. Black and blue lines depict  $p$ -curves observed in two sample sets of 20 experiments. The dashed red line represents what a  $p$ -curve would represent if the null was true and there was zero effect, where there is a uniform distribution from 0 to 1.0. The black line (Right) represents a typical right-skewed  $p$ -curve in the absence of  $p$ -hacking, where there are more  $p$ -values less than  $p < .01$  than any other interval. Whereas the blue line (Left) represents a typical left-skewed  $p$ -curve in the presence of  $p$ -hacking, where there is a large proportion of experiments with  $p$ -values near .05.

In the context of the previously mentioned recent meta-analysis, the half  $p$ -curve analysis revealed evidence for a self-controlled learning effect in the literature (a significant right-skewed half  $p$ -curve), however, above  $p < .025$ , the curve was left-skewed (Yantha et al., 2019). This left-skewed result provides evidence of  $p$ -hacking or selection effects by journal reviewers. Due to the  $p$ -curve analysis detecting evidence of  $p$ -hacking, a Vevea-Hedges weight-function model was subsequently used; a statistical technique that determines if an adjusted model for publication bias is a better fit than a random-effects meta-analysis (Veeva & Hedges, 1995). Indeed, the adjusted model was a significantly better fit to the data than the random-effects model, and the best estimate of the effect-size of self-controlled learning during retention was found to be a Hedges'  $g = .26$ , 95%  $CI (.08, .44)$  (Yantha et al., 2019).

It is possible that the effect-size in this experiment was within the sampling variation of the true effect-size of self-controlled learning, so the adjusted effect-size (Hedges'  $g = .26$ ) from the meta-analysis was compared to the effect-size from our experiment during retention. The calculated effect size for self-control compared to yoked during retention in our experiment was a Hedges'  $g = .43$ , which is within the sampling variation (95%  $CI$ s) of the adjusted effect-size from the meta-analysis. Moreover, our experiment was only powered to detect a moderate-sized effect, which at the time of the pre-registration was our best estimate of the self-controlled learning advantage. Therefore, as a result of the meta-analytic finding, it is recommended that future experiments be powered to reliably detect a small effect of Hedges'  $g = .26$  of self-controlled learning as opposed to a Hedges'  $g = .63$  (McKay et al., 2014).

### **Limitations and Delimitations**

First, it is important to distinguish between a limitation and a delimitation. A limitation is a constraint that is beyond control of the experimenter, and often stem from experimental design

choices (Simon & Goes, 2013). In comparison, delimitations are characteristics that arise from defining the boundaries of an experiment and are a result of the conscious choices made by researchers (Simon & Goes, 2013). Using these operational definitions, certain limitations and delimitations associated with the present experiment are elaborated upon.

The first general limitations to mention are those related to the use of questionnaires. As examples, participants were constrained to respond within particular response categories and were limited to responses on a Likert scale from one to seven for certain questions (Simon & Goes, 2013). Further, there is the possibility for social desirability bias, where participants may have answered items in a way that would represent themselves' in a favourable light, whether this behaviour was deliberate or unconscious (Edwards, 1953). It is also possible that individual differences in motivation caused certain participants to be more engaged in the task than others, or to respond differently to the availability of choice. For example, Ikudome, Kisho Ogasa, Mori, and Nakamoto (2019) used an objective motivational measure to divide learners into either a high or low-motivation group and then randomly assigned them to a choice or no-choice group (i.e., yoked). Results from their first experiment demonstrated an interaction effect where the choice group had significantly greater learning than the no-choice group among the low motivation participants, whereas, among the high motivation participants, the effect of choice was not significant. Such findings bring to light that the participants in this experiment may have varied with respect to their motivation. Future research should continue to examine how individual differences interact with various practice conditions to determine what works best for each learner.

Another limitation of the present experiment was that we had to rely on self-report measures as to whether participants were deceived into believing that the confederate was a golf

coach. By asking that question, it is uncertain if the participants only considered this possibility in hindsight. Regardless, four learners reported that they had doubts at times that the “actor” was not a golf coach. Additionally, one of these four participants also expressed verbally that they had doubts at times that the confederate golf coach was even controlling the feedback schedule. However, as mentioned previously sensitivity analyses were conducted with these four participants eliminated and the data still showed the same pattern. Thus, despite this limitation, the results and the conclusions do not differ.

One limitation that is inherent to the experimental design of self-controlled experiments is that there needs to be a quasi-random assignment and not full random assignment. This is due to needing to collect a subset of SC participants before testing a yoked group to have their feedback schedule available for yoking procedures. Nonetheless, this limitation was mitigated in this experiment by only collecting four SC participants before engaging in random assignment thereafter. Lastly, one other limitation inherent to the design of self-controlled experiments is that the researchers collecting the data are not blind to the experimental condition that the learners are placed in. This could potentially cause the experimenter to be biased, treating one group differently from the other (e.g., using different language when delivering the feedback, engaging in more or less social interactions) outside of the experimental manipulation. This may have occurred in some of the published experiments in the self-control literature potentially contributing to the overestimation of the effect-size in the published literature. It is unlikely in such an experimental design that the researcher conducting data collection could be blind to the experimental condition of the learner as the SC participants are typically prompted would you like feedback after every trial. A stronger design might be to have data collection completed by

one researcher and have data analysis completed by another researcher to avoid some potential bias.

As mentioned previously the dominant self-controlled procedural protocol that has been adopted in the literature is to prompt the SC participant after each trial as to whether they would like feedback for that just-completed trail (Chen et al., 2002). This procedural approach, however, creates a confound in the self-control research design because the self-control and yoked groups do not differ solely on the choice manipulation, but also on the fact that they receive a prompt after each trial. It is possible that the prompt itself would cause the SC learner to reflect on the just-completed movement, thereby engaging in increased response evaluation processes as compared to the yoked groups that do not receive such a prompt. This is a limitation in the literature because it is difficult to know each of the contributions of the autonomy, the prompt, or the performance-contingent feedback schedule in regard to the self-controlled learning advantage that is typically observed.

While this methodological weakness was recognized, in this experiment, it was decided that it would be best to replicate the methods of the dominant paradigm used in the existing self-controlled research for both the SC and TY groups. Therefore, the SC group was prompted after each trial if they would like feedback, whereas, the TY and PCC learners did not receive a prompt. The golf coach also was not prompted by the experimenter if they would like to provide feedback to the learner for the previous trial. Despite us adopting the dominant protocol used in the literature, and including this confound, we still did not observe the typical differences between the SC and TY groups that have been reported in the self-controlled feedback literature.

In terms of delimitations, KR was chosen as the practice variable to manipulate. A few of the participants, however, expressed the desire to receive KP. It is possible that if KP was

provided, it would have helped to make the learning environment more realistic, as KP is more often used in an applied setting (Schmidt & Lee, 2011). In fact, this may have reduced the number of participants in the PCC group that expressed doubt about the “confederate” being a golf coach. Indeed, two of the four participants verbalized that most of their doubt about the “actor” not being a golf coach came from him being limited to only providing KR feedback and not KP feedback. A thought-provoking response came from another PCC participant, who did not indicate that they had doubts about the golf coach. That individual responded that they would have preferred consistent visual feedback and had the golf coach correct his technique. Another delimitation was that the golf coach was a student, which might have affected the beliefs of the PCC participants. Having a golf coach who was someone who was a known expert would likely have been a stronger experimental design.

Another delimitation to consider was that of not including any measures of motivation, which may have provided insight on whether the motivation of the PCC participants was altered with the presence of the golf coach. A third delimitation, mentioned earlier, was the lower number of practice trials, which possibly limited the amount of learning and potential reduced the likelihood of group differences. A final delimitation to consider was the task type being an applied skill in a laboratory setting versus having an applied task in an applied setting, perhaps impacting the perception of the coach’s knowledge and expertise. However, a recent meta-analysis revealed that the learning advantage of self-controlled learning did not depend on task type (Yantha et al., 2019), so it is doubted that this was a determining factor in the lack of replication of typical self-controlled learning effects.

### **Future Research Directions**

When recommendations are made by authors for athletes to utilize self-controlled practice it is likely implied that the person providing the KR (or KP) is a knowledgeable coach, however, experimental designs have not explicitly tested this. If this is the implied recommendation, then an extension of this experiment would be adding a fourth group to the present design. This fourth group would have control over their feedback schedule but have a ‘perceived-coach’ deliver them the feedback upon the learners’ request. It is possible that in this situation, the expertise and knowledge of the coach would be additive with the self-controlled benefit and is an interesting future direction that needs to be tested.

As mentioned previously, with the PCC group having the best accuracy in the post-test, and also reporting that they did not receive feedback according to their preferences, it leaves one to contemplate how an actual golf coach in control of a feedback schedule would compare to a self-controlled learning situation. In this manner, it could be argued that both the motivational and informational factors associated with having a coach in control of a feedback schedule would be present, as well as the learner receiving a performance-contingent KR schedule, a factor considered to be important in the learning benefits of self-control (Karlinsky & Hodges, 2014). This combination of factors may well lead to greater learning advantages than that of just self-control.

All the factors listed thus far relate to advantages due to the actual scheduling of the feedback by a coach, however, a coach could still have an effect beyond this by possibly influencing the learners’ perception by being in the present in the environment and being viewed as knowledgeable. One of the propositions here was that having KR provided by a coach might alter the information processing engaged in by the learners. Evidence to support this has come

from the literature that has shown manipulations that result in the learner engaging in increased performance estimation activities, result in superior motor learning (Guadagnoli & Kohl, 2001) and error estimation performance (Barros, Yantha, Carter, Hussien, & Ste-Marie, 2019).

Moreover, it has also been shown that greater feedback processing is positively associated with motor learning (Grand et al., 2015; Luft, Nolte, & Bhattacharya, 2013; Luft, Takase, & Bhattacharya, 2014). However, empirical evidence that coach-controlled participants may engage in greater information (e.g., feedback) processing is lacking.

It is also possible that having a coach in charge of the feedback schedule would impact at a psychological level such as; increased motivation to perform well, more persistent engagement in the task, and/or increased perceived competence because an experienced and trusted person is controlling their feedback schedule (Wulf & Lewthwaite, 2016). Curran, Hill, and Niemiec (2013) found that structure from coaches (e.g., clear and consistent rules, guidance and assistance during the activity, feedback, etc.) related positively to behavioural engagement in sport and that these associations were explained by the satisfaction of the basic psychological needs. Additionally, Choi, Cho, and Huh (2013) found that perceptions of closeness (i.e., displaying liking, trust, and respect) between the coach-athlete relationship were significantly correlated with competence and autonomy. Therefore, it is possible that a coach being in control of a feedback schedule could provide a structured environment that could increase a learner's competence and engagement in the skill in comparison to a yoked or self-controlled learner. It should be noted that these possible effects could also just be due to the coach being present in the learning environment, therefore future work will need to separate this possible confounding factor.

It is surprising that the  $p$ -value was .069 for the effect of group for accuracy given that; (1) a perceived coach-controlled group was used and not an actual coach-controlled group, and (2) that KR was manipulated artificial (putting underneath a barrier that occluded the endpoint of the ball) rather than KP. Providing KP would more closely resemble an applied setting and could influence the perceptions of the learners in the PCC group and ultimately motor learning. Therefore, another future direction would be to integrate a group that has an actual coach control a KP schedule and to compare this group to the typical self-controlled learning paradigm. We predict that a group receiving a KP schedule from a coach would have a larger effect than the one observed in the present experiment.

## **Conclusion**

To summarize, the main findings were (1) the PCC group had numerically lower MRE than both the SC and TY groups during retention and transfer, albeit not-significant, (2) the PCC responded to the questionnaire similar to a TY group, indicating that they did not receive feedback according to their preferred schedule. Given these findings, it is proposed that recommendations on the use of autonomy and choice over KR should be toned-down until future research is done which aligns more closely with the role of the coach in applied motor skill learning settings. This future research will need to parcel out what practice variables are most appropriate to leave a coach in control of and what ones are best suited for the learner to be provided choice, while not negatively impacting performance and learning. It is also worth noting that these findings emerged even though the comparison group was a perceived coach-controlled group, and not an actual coach determining the feedback. One might assume that an actual coach would draw on their expertise from motor learning and their domain to provide a

more effective feedback schedule for motor learning, thus future research on this topic is encouraged.

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**Appendix A**

## Demographic Questionnaire

Name: \_\_\_\_\_

Gender: \_\_\_\_\_

Age: \_\_\_\_\_

Golf Putting Handedness: \_\_\_\_\_

1. Approximately how many times have you had a putting experience (e.g., mini putt or a round of golf) in the last year? \_\_\_\_\_
2. Approximately how many times have you had a putting experience (e.g., mini putt or a round of golf) in the last five years? \_\_\_\_\_

**Appendix B**

Pre-Experiment Questionnaire

1. Have you had any experience being part of an organized sport in which there is a striking implement (such as, hockey, baseball, or tennis, etc.)? If so, please list all sports and include the total number of sports in the blank below. \_\_\_\_\_

\_\_\_\_\_

Total Number of Sports \_\_\_\_\_

2. Have you been involved in an activity in which you had a coach or instructor (e.g., played on a youth competitive hockey team, took music lessons with an instructor)? If yes, provide an overall rating in terms of whether your experiences being coached or instructed have been positive or negative. Use the following scale to rate your overall experience and place your answer in the box that is underlined below.

1	2	3	4	5	6	7
Poor		Fair		Good		Excellent

Please rate your previous experiences \_\_\_\_\_

### Appendix C

#### Post-Experiment Questionnaire (Self-Controlled Group)

We would like to gain some further information about your choices regarding feedback delivery.

Please read the following item carefully and then respond by indicating how true it is for you.

Use the following scale to respond:

1	2	3	4	5	6	7
Not true at all		Fair		Good		Excellent

1. I found the feedback provided was meaningful. \_\_\_\_\_

**Please read each question and respond accordingly by bolding your answer or filling in the blank.**

2. Select the option that best represents when you chose to ask for feedback?
- After perceived good trials
  - Perceived bad trials
  - Perceived bad and good trials equally
  - Randomly
  - Other: \_\_\_\_\_
3. Select the option that best represents when you did not choose to ask for feedback?
- Perceived good trials
  - Perceived bad trials
  - Perceived bad and good trials equally

- d. Randomly
  - e. Other: \_\_\_\_\_
4. Feedback scheduling can occur in different ways. Select the option below that best represents the manner in which you would have preferred to have your feedback scheduled with this experimental task?
- a. Schedule feedback on my own.
  - b. Have a golf coach schedule my feedback.
  - c. Have a predetermined feedback schedule.
  - d. Other: \_\_\_\_\_
5. On the trials that you received feedback, how do you think it helped you learn the task?
-

## Post-Experiment Questionnaire (Traditional Yoked Group)

We would like to gain some further information about your choices regarding feedback delivery.

Please read the following item carefully and then respond by indicating how true it is for you.

Use the following scale to respond:

1	2	3	4	5	6	7
Not true at all		Fair		Good		Excellent

1. I found the feedback provided was meaningful. \_\_\_\_\_

2. I found the feedback was provided when I would have wanted it. \_\_\_\_\_

**Please read each question and respond accordingly by bolding your answer or filling in the blank.**

3. If you **did not** receive feedback when you would have wanted it, select the option below that best represents when you would have liked to receive feedback?

a. Perceived good trials

b. Perceived bad trials

c. Perceived bad and good trials equally

d. Randomly

e. Other: \_\_\_\_\_

4. Feedback scheduling can occur in different ways. Select the option below that best represents the manner in which you would have preferred to have your feedback scheduled with this experimental task?

- a. Schedule feedback on my own.
  - b. Have a golf coach schedule my feedback.
  - c. Have a predetermined feedback schedule.
  - d. Other: \_\_\_\_\_
5. On the trials that you received feedback, how do you think it helped you learn the task?
- \_\_\_\_\_

## Post-Experiment Questionnaire (Perceived Coach-Controlled Group)

We would like to gain some further information about your choices regarding feedback delivery.

Please read the following item carefully and then respond by indicating how true it is for you.

Use the following scale to respond:

1	2	3	4	5	6	7
Not true at all		Fair		Good		Excellent

1. I found the feedback provided was meaningful. \_\_\_\_\_

2. I found the feedback was provided when I would have wanted it. \_\_\_\_\_

**Please read each question and respond accordingly by bolding your answer or filling in the blank.**

3. If you **did not** receive feedback when you would have wanted it, select the option below that best represents when you would have liked to receive feedback?

a. Perceived good trials

b. Perceived bad trials

c. Perceived bad and good trials equally

d. Randomly

e. Other: \_\_\_\_\_

4. Feedback scheduling can occur in different ways. Select the option below that best represents the manner in which you would have preferred to have your feedback scheduled with this experimental task?

- a. Schedule feedback on my own.
- b. Have a golf coach schedule my feedback.
- c. Have a predetermined feedback schedule.
- d. Other: \_\_\_\_\_

We would like to gain some further information about how you perceived the golf coach that controlled your feedback schedule. Please read each of the following items carefully and then respond to the first two statements by indicating how true it is for you. Use the following scale to respond:

1	2	3	4	5	6	7
Not true at all		Fair		Good		Excellent

- 5. I thought the golf coach was knowledgeable. \_\_\_\_\_
- 6. I thought the golf coach had knowledge of effective feedback schedules and motor learning principles. \_\_\_\_\_
- 7. On the trials that you received feedback, how do you think it helped you learn the task?  
\_\_\_\_\_
- 8. At any point during the experiment, did you have any reason to believe that “the actor’s name” was **not** a golf coach? \_\_\_\_\_

## **Appendix D**

### Calibration Procedure

To calibrate the camera system a straight-line was indicated by a red chalk line marked on the putting origin, this line went from the starting location to the centre of the target cross. Importantly it was only marked behind the barrier out of the participants' vision. Next, a 1 m calibration board was needed to be placed in view of the camera. A photo was then captured by pressing the "Calibrate" button. Next, the two ends of the 1 m calibration board were marked in the image in order to have a reference for the number of pixels per meter. The calibration also included referencing a straight line by clicking on two points along the chalk line. Lastly, the target was referenced by clicking on the center of the cross.

## Appendix E

### Ethics Approval

20/02/2019

**Université d'Ottawa**

Bureau d'éthique et d'intégrité de la recherche

**University of Ottawa**

Office of Research Ethics and Integrity

#### CERTIFICAT D'APPROBATION ÉTHIQUE | CERTIFICATE OF ETHICS APPROVAL

<b>Numéro du dossier / Ethics File Number</b>	H-06-18-714
<b>Titre du projet / Project Title</b>	Examining the effectiveness of a self-controlled feedback schedule compared to a perceived coach-controlled yoked schedule on motor skill acquisition
<b>Type de projet / Project Type</b>	Thèse de maîtrise / Master's thesis
<b>Statut du projet / Project Status</b>	Approuvé / Approved
<b>Date d'approbation (jj/mm/aaaa) / Approval Date (dd/mm/yyyy)</b>	20/02/2019
<b>Date d'expiration (jj/mm/aaaa) / Expiry Date (dd/mm/yyyy)</b>	04/07/2019

#### Équipe de recherche / Research Team

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**Conditions spéciales ou commentaires / Special conditions or comments**