The effects of practicing a motor skill with the expectation of teaching it: Benefits to skill learning, potential underlying mechanisms, and effects on skill performance under psychological pressure

by

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Abstract

This dissertation describes a program of research (consisted of five studies) that focused on investigating a practical technique intended to enhance individuals' motor learning. Specifically, I examined whether learners who practice a skill with the expectation of teaching it to another person would exhibit superior learning relative to learners who practiced a skill with the expectation of being tested on it. The first two chapters reveal expecting to teach enhances motor learning, as indexed by skill accuracy and precision as well as declarative knowledge about the skill during posttests. Both experiments suggest the learning benefit cannot be attributed to motivation or pressure, but the second experiment suggests expecting to teach may enhance learning by increasing the amount of time participants spend preparing movements for practice trials. The third and fourth experiments further explored motor preparation as a potential mechanism underlying the learning benefit of expecting to teach. Taken together, however, these experiments suggest neither the length of motor preparation time during practice nor the cerebral cortical dynamics during motor preparation while practicing explain the learning benefit of expecting to teach. The fifth experiment investigated a potential pitfall of expecting to teach. Specifically, I examined whether learning a skill with the expectation of teaching it impairs the skill's performance under psychological pressure, due to the gains in declarative knowledge about the skill caused by expecting to teach. Results reveal expecting to teach does indeed cause learners to choke under pressure, but only to the extent that they exhibit performance equal to that of learners who practice without the expectation of teaching. Taken together, the five experiments indicate expecting to teach enhances motor learning, but this benefit is eliminated when the learned skill is performed under psychological pressure, although only to the degree that individuals who learn with the expectation of teaching perform equally well as individuals who learn without this expectation. The mechanisms underlying the benefit of expecting to teach remain elusive, leaving plenty of open questions for future research.

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List of Abbreviations

BVE	Bivariate Variable Error
EEG	Electroencephalogram/Electroencephalography
IMI	Intrinsic Motivation Inventory
RE	Radial Error

Chapter 1: Expecting to teach enhances learning: Evidence from a motor learning paradigm

Introduction

Determining practical ways to enhance people's learning while they study material or practice a skill is crucial to improving their behavior and mental processes. One way might be having them study or practice with the expectation of teaching the material or skill to another person. Indeed, some studies have shown that when participants study academic information with the expectation of teaching it, they exhibit augmented learning (Bargh & Schul, 1980; Benware & Deci, 1984; Nestojko, Bui, Kornell, & Bjork; 2014). However, other studies have failed to reveal this effect (Renkl, 1995; Ross & Di Vesta, 1976) or demonstrated ambiguous learning effects (enhancements on short-term, but not long-term, test performance; Fiorella & Mayer, 2013; Fiorella & Mayer, 2014). Importantly, no study has investigated whether expecting to teach enhances learning of perceptual-motor skills, which rely heavily on procedural knowledge and somewhat on declarative knowledge. Thus, perceptual-motor skill acquisition is distinct from learning academic information, which relies primarily on declarative knowledge (Rosenbaum, Carlson, & Gilmore, 2001). Thus, in the present experiment, we addressed this shortcoming by examining whether the expectation of teaching enhances motor learning and, in so doing, whether expecting to teach may yield a general learning benefit to different types of skills.

There are several mechanisms whereby expecting to teach theoretically may enhance learning generally and motor learning specifically. First, expecting to teach may increase people's motivation by causing them to recognize that their learning affects another person's behavioral improvement, in addition to their own. To this point, some studies have observed expecting to teach enhances motivation (Benware & Deci, 1984; Fiorella & Mayer, 2014, Experiment 1), although others have not (Fiorella & Mayer, 2013; Fiorella & Mayer, 2014, Experiment 2). Second, given that another person is depending on the learner, expecting to teach may increase learners' anxiety, yielding adaptive arousal levels (Yerkes & Dodson, 1908). To this point, Fiorella and Mayer (2014, Experiment 1) observed expecting to teach led to increased stress accompanied by short-term test improvement, although they subsequently failed to replicate this effect (Fiorella & Mayer, 2014, Experiment 2). Third, expecting to teach may enhance identification of key concepts related to the skill/material being learned, because teaching involves summarizing critical points (Nestojko et al., 2014). Indeed, Nestojko et al. (2014) observed expecting to teach enhanced free recall of main points in a text. Related to these mechanisms, it is also important to consider whether expecting to teach enhances learning by increasing the quantity of studying and practice. For example, increases in motivation and anxiety could theoretically stimulate more studying and practice, consequently enhancing learning. Thus, in the current experiment, participants were given the option to study and practice beyond the experimentally defined minimum, and the quantity of studying and practice was controlled for in statistical analyses. Accordingly, we tested the following hypotheses: (H1) Expecting to teach enhances motor learning, and (H2) expecting to teach enhances motor learning after statistically controlling for quantity of skill study (time spent reviewing instructions, accruing declarative knowledge) and physical practice (number of putts practiced, accruing procedural knowledge). Motivation, anxiety, and identification of key concepts were examined as theoretical mechanisms explaining any learning effects, as suggested by previous literature.

Methods

Participants

Fifty-six right-handed, young adults (32 females and 24 males, M_{age} = 23.1, *SD* = 2.40 years; see Table 1 for detailed descriptive data) provided informed written 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 expecting to teach on motor learning, controlling for the quantity of time spent studying, repetitions of practice, and baseline (pretest) motor skill performance in a multiple regression model (Faul, Erdfelder, Lang, & Buchner, 2007). Participants were recruited from university courses and by word-of-mouth, and they were compensated with course credit and/or entry into a raffle for a monetary award.

Task

Participants used a standard, right-handed golf putter to putt a standard golf ball on an artificial grass surface to a target cross (+) comprised of two 10.8 cm pieces of white masking tape located 120 cm from participants. The objective was to have the ball stop as close to the center of the target as possible.

Procedure

Day 1. All participants completed the experiment individually. After consenting to participate, they completed a demographic questionnaire asking them their age, sex, and putting experience (anything from miniature golf to playing 18 holes on a standard golf course) over their lifetime and within the past year (only participants who had putted 30 times or fewer in their lifetime were recruited to participate in the experiment). Next, participants completed the pretest phase, which consisted of 10 putts. As the purpose of this phase was to determine their baseline skill level, we attempted to isolate performance and minimize on-line learning by having them wear a blindfold and earplugs while putting (Dyke et al., 2014). In so doing, they were not able to obtain visual or auditory feedback about the outcome of the putt, which is crucial for learning. This also meant participants were unable to use visual or auditory feedback during putt execution. However, putting is a discrete motor skill relying primarily on feedforward control. Prior to each putt, they were permitted to view the ball and target. Once the putter was placed behind the ball, vision was occluded. After the pretest, participants were read instructions according to the group to which they were randomly assigned. Participants in the Teach group were told “tomorrow you will teach another participant how to putt,” and participants in the Test group were told “tomorrow you will be tested on your putting.” Next, participants completed the acquisition phase. First, they were told they had 1 hour to practice and had to remain in the study laboratory for the duration of the hour. Next, they were told to start practice by studying a golf putting instruction booklet for at least 2 min, but to take as long to initially study the booklet as they liked. The instructions for the booklet were provided by an expert golfer (for information about the golfer and to view the booklet, see the appendix). After studying the booklet, participants were told that they would continue having the opportunity to learn by performing at least five sets of ten putts, but that they could take up to an additional five sets of putts if they liked, as long as they continued to perform the putts in sets of no more than ten. Between each set, participants were told they had a 1-min break during which they could study the booklet if they liked. Participants were told that after they finished practicing they could spend the remainder of the hour in the lab doing what they liked (e.g., browsing the Internet), but that, once they stopped practicing, they would not be allowed to resume. When participants stopped practicing, they completed an anxiety visual analog scale (Anxiety VAS) and a motivation visual analog scale (Motivation VAS). The Anxiety VAS asked participants “how anxious (nervous) [they felt] while putting,” and it was anchored by “not anxious at all” and “extremely anxious.” The Motivation VAS asked participants “how motivated [they were] to learn the putting task,” and it was anchored by “not motivated at all” and “extremely motivated.” On both scales, participants were instructed to respond by drawing a vertical line across a 10 cm horizontal line to indicate how motivated/anxious they felt.

Day 2. Approximately 24 h after completing Day 1, participants returned to complete the study. Participants in the Teach group were told “the participant who you were going to teach did not show up today, so you will actually be tested on your putting instead.” Next, participants completed the retention and transfer test phases in a counterbalanced order. As the purpose of these phases was to determine their skill level achieved due to the previous day’s practice, we attempted to isolate performance and minimize on-line learning (make them putt from memory) by having participants wear a blindfold and ear-plugs, but they were permitted to view the ball while placing their putter behind it prior to each putt (as in the pretest). The retention test was the same as the pretest, whereas the transfer test required participants to putt to a target 50 cm farther away from the pretest/acquisition/retention target. The transfer test was included because the ability to adapt a skill to novel parameters (e.g., putt to a farther target) is considered a hallmark of motor learning (e.g., Magill & Anderson, 2014). After completing retention and transfer, participants were asked to complete a free recall test asking them “to report, in as much detail as possible, any rules, methods, or techniques [they recalled] using to execute putts.” Finally, participants were debriefed regarding the purpose of the study (Teach group participants were told they were deceived—there was never another participant to teach—and asked if they still wanted to have their data included in the study (they all did) and dismissed.

Data Processing

Putting accuracy and precision were measured because they are separate and critical aspects of motor learning (Fischman, 2015). Specifically, accuracy was indexed by recording radial error (RE) and precision was indexed by recording bivariate variable error (BVE) as recommended by Hancock, Butler, and Fischman (1995). RE and BVE were calculated for the odd-numbered blocks in the acquisition phase to get a general assessment of improvement during practice without overly slowing data collection. RE and BVE were calculated for all blocks of the pretest, retention, and transfer test phases in order to assess motor learning. The Motivation and Anxiety VASs were scored by measuring how many millimeters from the "not anxious/motivated at all" end of the line participants placed their vertical line. The amount of time participants spent looking at the putting instruction booklet was recorded in order to quantify the amount of time they spent studying the skill, and the number of putts they took during the acquisition phase was recorded to quantify their practice repetitions. To index the identification of key concepts, the number of main points from the putting instruction booklet correctly recalled by participants was recorded. The main points were the most important concepts, as indicated by the expert golfer who provided the putting instructions: (1) "establish proper grip," (2) "place the putter head behind the ball and take a hip-width stance," (3) "place the eyes directly over the ball by hinging from the hips," and (4) "stroke the ball without breaking the wrists."

Statistical Analysis

Independent sample *t*-tests were conducted to verify that there were no group differences with respect to Age, Gender, Lifetime Putting Experience, or Past-Year Putting Experience. To assess practice performance differences between the groups, mixed-factor ANCOVAs were conducted for RE and BVE with Group (Test or Teach) serving as the between-subjects factor, Set (practice sets 1, 3, and 5) serving as the within-subjects factor, and Pretest RE or BVE (depending on the ANCOVA's dependent variable) serving as the covariate.

In order to measure learning, prior to the regression analyses testing the a priori hypotheses, separate mixed-factor ANCOVAs for RE and BVE were conducted with Group serving as the between-subjects factor, Posttest (Retention or Transfer) serving as the within-subjects factor, and Pretest RE/BVE, Studying (time spent studying the instruction booklet), and Putts (number of putts taken) serving as covariates. Significant Group x Posttest interactions would indicate that separate regressions should be conducted for the retention and transfer tests, whereas nonsignificant Group x Posttest interactions would indicate that RE and BVE could be collapsed across the retention and transfer tests, thus reducing the number of regressions conducted (Lohse, Buchanan, & Miller, in press).

Regressions were conducted for RE and BVE separately. The first-step of the regressions included Group and Pretest RE or BVE as predictors, and the second step added in Studying and Putts as predictors. An exploratory third step was also conducted by adding in Group x Studying and Group x Putts interaction terms as predictors, but the inclusion of these terms in the model was dependent upon their adding a significant proportion of explained variance (i.e., a significant change in *F*). To test potential mechanisms explaining any group effect revealed by the regressions, separate independent sample *t*-tests were conducted for Motivation VAS, Anxiety VAS, and Key Concepts (number of key concepts correctly recalled). Next, any mechanism that exhibited significant group differences was entered into a regression to determine if it predicted

RE or BVE, controlling for Group and Pretest RE or BVE. Alpha levels were set to .05 for all tests and all confidence intervals are set at 95%.

Results

Demographics, Time Spent Studying, and Putts Taken

Groups did not significantly differ with respect to Age, Gender, Lifetime Putting Experience, Past-Year Putting Experience, Studying, or Putts (ts ≤ 1.70; see Table 1).

Table 1. Descriptive data for each group.

Descriptive Data by Group	Test (n = 28; 13 females)		Teach (n = 28; 19 females)	
	M	*CI*	*M*	*CI*
Age (Years)	23.6	22.7 – 24.5	22.5	21.7 – 23.3
Lifetime Putting Experience[a]	1.11	0.770 – 1.45	1.18	0.820 – 1.54
Past-Year Putting Experience[a]	0.423	0.240 – 0.610	0.464	0.280 – 0.650
Studying (s)	278	240 – 317	309	271 – 347
Putts	71.1	64.4 – 77.8	68.5	61.5 – 75.5
Motivation VAS	78.9	74.3 – 83.5	79.1	73.8 – 84.4
Anxiety VAS	32.9	24.2 – 41.6	36.2	27.7 – 44.7
Free Recall	1.36	0.990 – 1.73	2.39	1.86 – 2.92

[a] 0 = Never putted; 1= Putted 1 – 10 times; 2 = Putted 11 – 20 times; 3 = Putted 21 – 30 times

Practice Performance

Figure 1A shows RE for the groups across all phases of the study. The ANCOVA revealed nonsignificant effects for Group, Set, and the Group x Set interaction (Fs ≤ 3.12).

Figure 1B shows BVE for the groups across all phases of the study. The ANCOVA revealed nonsignificant effects for Group, Set, and the Group x Set interaction (*F*s ≤ 1.42).

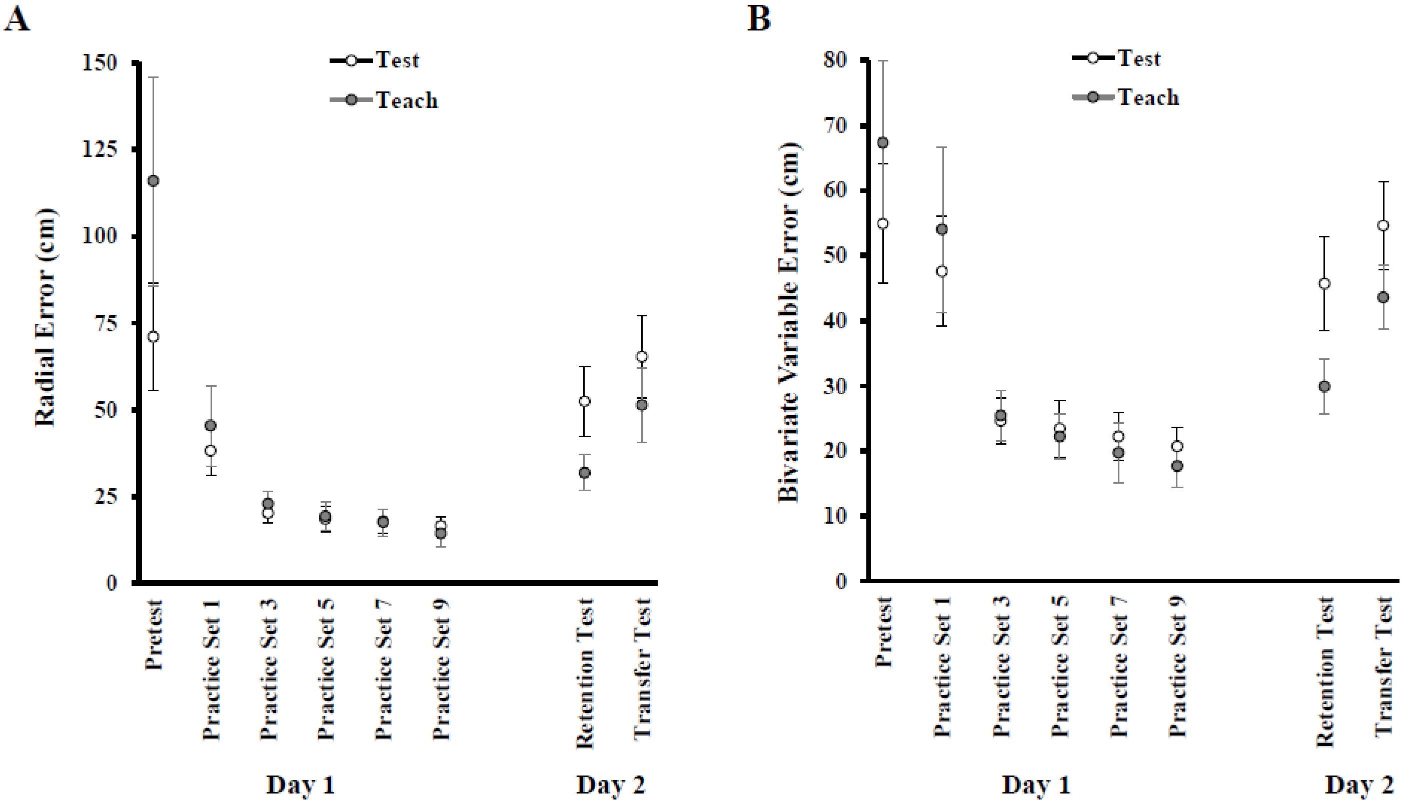


Fig. 1. A) Putting accuracy (lower RE indicates greater accuracy) as a function of study phase and group. Motor learning, as indexed by averaged retention and transfer test accuracy, is superior for the Teach group relative to the Test group. B) Putting precision (lower BVE indicates greater precision) as a function of study phase and group. Motor learning, as indexed by averaged retention and transfer test precision, is superior for the Teach group relative to the Test group. All error bars represent 95% CIs.

Motor Learning (Retention and Transfer Test Performance)

The ANCOVAs for RE and BVE revealed nonsignificant Group x Posttest interactions (*F*s ≤ 0.903), so RE and BVE were averaged across the retention and transfer tests in the regressions. The first step of the regression for RE revealed a significant effect of Group controlling for Pretest RE (β_{Group} = -22.2 cm, see Table 2). The second step of this regression revealed a significant effect of Group controlling for Pretest RE, Studying, and Putts (β_{Group} = -22.8 cm; see Table 2). Notably, the Group effect is also significant for both steps when not controlling for Pretest (βs ≤ -17.2, *p*s ≤ .011). The exploratory third step revealed that adding Group x Studying and Group x Putts to the model explained a nonsignificant proportion of explained variance (*F* = .055).

Table 2. Details of regression models testing the hypotheses that expecting to teach enhances motor learning (as indexed by superior averaged retention and transfer test accuracy) not controlling for the amount of time spent studying the instruction booklet and the number of putts taken (Model 1) and controlling for these factors (Model 2). Regression coefficients are not standardized and are thus interpretable in their natural units. For the Group variable, Test = ‘0’ and Teach = ‘1.’

Model 1: Avg. Retention and Transfer Test RE ~ Pretest + Group					
	SS	*Df*	*MS*	*F*	R^2 *Change*
Regression	6910	2	3455	6.19	.168
Residual	29608	53	559		
Coefficients	β	*CI*	*t*-value	*p*-value	
Intercept	51.1	39.6 – 62.5	8.96	< .001	
Pretest RE	0.111	0.011 – 0.211	2.22	.031	
Group	-22.2	-35.7 – -8.79	3.32	0.002	

Model 2: Avg. Retention and Transfer Test RE ~ Pretest + Group + Studying + Putts					
	SS	*Df*	*MS*	*F*	R^2 *Change*
Regression	7513	4	1878	3.30	.017
Residual	29004	51	569		
Coefficients	β	*CI*	*t*-value	*p*-value	
Intercept	63.2	33.6 – 92.7	4.29	< .001	
Pretest RE	0.109	0.006 – 0.211	2.13	.038	
Group	-22.8	-36.4 – -9.07	3.34	.002	
Studying	0.004	-0.062 – 0.70	0.124	.902	
Putts	-0.184	-0.547 – 0.178	1.02	.312	

The first step of the regression for BVE revealed a significant effect of Group controlling for Pretest BVE (β_{Group} = -14.3 cm, see Table 3). The second step of this regression revealed a significant effect of Group controlling for Pretest BVE, Studying, and Putts (β_{Group} = -14.8 cm). Notably, the Group effect is also significant for both steps when not controlling for Pretest (βs ≤ -13.4, *p*s < .001). The exploratory third step revealed that adding Group x Studying and Group x Putts to the model added a nonsignificant proportion of explained variance (*F* = .518).

Table 3. Details of regression models testing the hypotheses that expecting to teach enhances motor learning (as indexed by superior averaged retention and transfer test precision) not controlling for the amount of time spent studying the instruction booklet and the number of putts taken (Model 1) and controlling for these factors (Model 2). Regression coefficients are not standardized and are thus interpretable in their natural units. For the Group variable, Test = ‘0’ and Teach = ‘1.’

Model 1: Avg. Retention and Transfer Test BVE ~ Pretest + Group					
	SS	*Df*	*MS*	*F*	R^2 *Change*
Regression	2750	2	1375	8.14	.234
Residual	8957	53	169		
Coefficients	β	*CI*	*t*-value	*p*-value	
Intercept	46.4	38.2 – 54.6	11.3	< .001	
Pretest BVE	0.068	-0.052 – 0.189	1.14	.260	
Group	-14.3	-21.4 – -7.17	4.02	< .001	

Model 2: Avg. Retention and Transfer Test BVE ~ Pretest + Group + Studying + Putts					
	SS	*Df*	*MS*	*F*	R^2 *Change*
Regression	2920	4	730	4.24	.014
Residual	8787	51	172		
Coefficients	β	*CI*	*t*-value	*p*-value	
Intercept	44.9	28.2 – 61.7	5.39	< .001	
Pretest BVE	0.058	-0.066 – 0.182	0.937	.353	
Group	-14.8	-22.1 – -7.52	4.08	< .001	
Studying	0.018	-0.019 – 0.054	0.977	.333	
Putts	-0.040	-0.239 – 0.159	0.977	.687	

Potential Mechanisms Explaining Group Effect

The groups did not significantly differ with respect to Motivation VAS or Anxiety VAS (ts $\leq$ -0.573; see Table 1), but the Teach group (M = 2.39) recalled significantly more key concepts than the Test group (M = 1.36; t(48.2) = 3.16, p = .003, d = 0.863, corrected for unequal variances, see Table 1). A regression assessing whether Key Concepts predicted RE, controlling for Group and Pretest RE, revealed a nonsignificant result (t = 1.39). Similarly, a regression assessing whether Key Concepts predicted BVE, controlling for Group and Pretest BVE, revealed a nonsignificant result (t = 0.895).

Discussion

In the present experiment we investigated whether expecting to teach enhances motor learning. In particular, the following hypotheses were tested: (H1) Expecting to teach enhances motor learning, and (H2) expecting to teach enhances motor learning, after statistically controlling for the quantity of skill study and practice. Results supported both hypotheses. Specifically, expecting to teach enhanced retention and transfer test accuracy (lower RE) and precision (lower BVE), thus confirming H1. Additionally, expecting to teach enhanced retention and transfer test accuracy and precision after controlling for the quantity of skill study (time spent studying the instruction booklet) and practice (number of putts taken), thus confirming H2. Motivation, anxiety, and the identification of key concepts were examined as possible mechanisms explaining the learning effects. Results revealed expecting to teach did not increase motivation or anxiety, but did enhance the identification (free recall) of key concepts. However, the identification of key concepts was not correlated with motor learning, when controlling for the expectation of teaching. Thus, despite the plausible theoretical explanations tested, reasons why expecting to teach enhances motor learning remain unclear.

One hypothesis as to why expecting to teach enhances motor learning is that it may elicit more elaborate information processing prior to executing practice trials, which has been associated with superior motor learning (Cross, Schmitt, & Grafton, 2007). Future studies may index information processing prior to practice trials by measuring the time participants take prior to executing a trial. A second hypothesis as to why expecting to teach enhances motor learning is that it may increase motivation, although we found no evidence to support that supposition in the present experiment. Specifically, the single-item motivation (and anxiety) questionnaire may have poorly represented the construct. Future studies may need to employ more extensive questionnaires. For instance, we measured *general* motivation, when *intrinsic* motivation may have been more appropriate to assess. Indeed, the two studies reporting expecting to teach increases motivation focused on intrinsic motivation (Benware & Deci, 1984; Fiorella & Mayer, 2014, Experiment 1), which is strongly linked to motor learning (Lewthwaite & Wulf, 2012).

The results obtained in the present research are the first to reveal that expecting to teach enhances motor learning and, taken together with similar studies concerning academic information, the present work suggests expecting to teach yields a general learning benefit to different types of skills. Accordingly, creating the expectation of teaching while people study material or practice a skill may be a practical means to improve their behavior and mental processes. It is important to note that simply creating the expectation of teaching cannot practically be implemented in order to continually enhance learning, because learners would soon realize they are never actually going to have to teach. Furthermore, research suggests that having learners study academic information with the expectation of teaching and then actually having them teach elicits even greater learning than the expectation of teaching alone (Fiorella & Mayer, 2014, Experiment 2). Accordingly, it is likely having learners study and practice motor skills with the expectation of teaching and then having the learners actually teach the skills is a practical way to continually enhance learning. Future studies should test this hypothesis.

Chapter 2: Expecting to teach enhances motor learning and information processing during practice

Introduction

Determining practical ways to enhance motor learning is crucial to facilitate motor behavior. In light of this, Daou, Buchanan, Lindsey, Lohse, and Miller (2016) investigated a novel means to enhance motor learning: have learners study and practice a skill with the expectation of having to teach it. The impetus for this investigation was the small body of literature indicating that expecting to teach may enhance the learning of declarative knowledge, like academic information (Bargh & Schul, 1980; Benware & Deci, 1984; Nestojko, Bui, Kornell, & Bjork, 2014). However, no previous research had investigated the effects of expecting to teach on motor learning, which relies heavily on procedural knowledge. Thus, Daou et al. attempted to address this shortcoming.

Specifically, Daou, Buchanan, et al. (2016) had participants study and practice golf putting during an acquisition phase either with the expectation of having to teach the skill to another participant (Teach group) or being tested on the skill the next day (Test group). Participants' study time and practice repetitions were allowed to vary in order to test whether expecting to teach would have an indirect and/or direct effect on motor learning. Specifically, Daou et al. sought to determine whether expecting to teach would increase studying and practice, thereby *indirectly* enhancing learning, or whether expecting to teach would *directly* improve learning, after statistically controlling for studying and practice. Upon arriving for the second day of the experiment, Teach participants were told the participant they were supposed to teach could not come, and so the Teach participant would be tested on their putting instead. Thus, Teach and Test participants completed retention and transfer tests (posttests). Teach participants exhibited superior posttest putting accuracy and consistency, even after controlling for the amount of time participants spent studying and the number of putts they practiced during acquisition, which did not differ between the groups. Therefore, Daou et al. revealed that expecting to teach *directly* (controlling for quantity of skill study and practice) enhances motor learning. Further, Teach participants remembered more key concepts about golf putting on a free recall test. The purpose of the present experiment was to replicate and expand upon Daou et al.'s results. We attempted to replicate the null result for the indirect effect and the positive result for the direct effect.

In addition to revealing expecting to teach enhances motor learning, Daou, Buchanan, et al. (2016) also investigated possible mechanisms underlying their results. Specifically, Daou et al. examined motivation, which has been associated with both expecting to teach (Benware & Deci, 1984; Fiorella & Mayer, 2014, Experiment 1) and motor learning (for review, see Wulf & Lewthwaite, 2016). Daou et al. also assessed anxiety (pressure), which has been linked to expecting to teach (Fiorella & Mayer, 2014, Experiment 1) and may elicit adaptive levels of arousal (Yerkes & Dodson, 1908). However, Daou et al. observed no differences in motivation or anxiety between Teach and Test participants. The absence of differences might be related to the way in which motivation and anxiety were measured. Specifically, motivation and anxiety were indexed with single-item visual analog scales, which may have poorly represented the constructs. Additionally, Daou et al. measured general motivation, and expecting to teach has been shown to specifically enhance intrinsic motivation (Benware & Deci, 1984; Fiorella & Mayer, 2014, Experiment 1). Thus, we sought to overcome Daou et al.’s shortcomings in measuring motivation and pressure by indexing several types of motivation (intrinsic, internalized, and general) and pressure with the multi-item subscales of the Intrinsic Motivation Inventory (IMI; McAuley, Duncan, & Tammen, 1989).

Another shortcoming of Daou, Buchanan, et al. (2016) is that they limited their investigation to social-affective mechanisms, when information processing mechanisms could have explained the motor learning effect. For example, participants could have engaged in greater information processing prior to acquisition phase trials, which has been associated with motor learning (e.g., Cross, Schmitt, & Grafton, 2007). Information processing prior to acquisition trials may benefit motor learning in multiple ways. For example, information processing may enhance the elaborateness and distinctiveness of a generalized motor program’s representation, thereby improving its encoding (Shea & Zinny, 1983). As another example, information processing could facilitate learning the proper parameterization of a motor program given certain environmental conditions (e.g., distance to target), which could facilitate parameterization during program retrieval. In both cases, increased information processing prior to acquisition trials may elongate the preparation preceding each trial, as learners deliberately program their movement. Thus, we sought to index information processing during acquisition by quantifying the duration participants took preparing each putt.

A final shortcoming of Daou, Buchanan, et al. (2016) is the problem that the Teach participants had their expectations violated just before their posttests, when they were told that they wouldn’t be teaching. Conversely, the Test participants did not have their expectations violated, because they performed the posttests as anticipated. To address this confound, we added a second day of posttests, at which both Teach and Test participants knew they were going to be tested on their putting. This second day of posttests was one week after the acquisition phase, so these posttests also allowed examination of the relative durability of the motor learning effect.

We addressed Daou, Buchanan, et al. (2016)’s shortcomings and attempted to replicate the motor learning effects: superior accuracy and consistency on retention and transfer tests, controlling for pretest accuracy/consistency, skill studying, and skill practice. Finally, we also explored the nature of the motor learning effects. Specifically, we investigated whether Teach participants developed a more elaborate generalized motor program than their Test counterparts, or were implementing a similar motor program but parameterizing it better. To examine this question, reaction time to begin the putting movement was recorded at a pretest and at the posttests (based on work by Henry & Rogers, 1960). Although a longer reaction time to begin a movement can indicate a more elaborate motor program is being ‘opened’, this means of assessing motor program complexity is weak. Specifically, there are many other factors that could contribute to reaction time, such as the sense of urgency brought about the ‘go’ signal (see Section 2.3.1). As such, this dependent variable should be sensitive to response programming, but is certainly not specific to response programming.

Methods

Participants

Fifty-six right-handed, young adults (31 females, $M_{\text{age}} = 21.5$, $SD = 2.12$ years; see Table 1 for detailed descriptive data) completed the experiment after providing informed written consent to an institution-approved research protocol. Two participants did not show up for Day 3, so their data were excluded from all analyses, and all information in the manuscript reflects the *exclusion* of these participants. 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 expecting to teach on motor learning, controlling for the quantity of time spent studying, repetitions of practice, and baseline (pretest) motor skill performance in a multiple regression model (Faul, Erdfelder, Lang, & Buchner, 2007). Participants were recruited from university courses and by word-of-mouth, and they were compensated with course credit and/or entry into a raffle for a monetary award.

Task

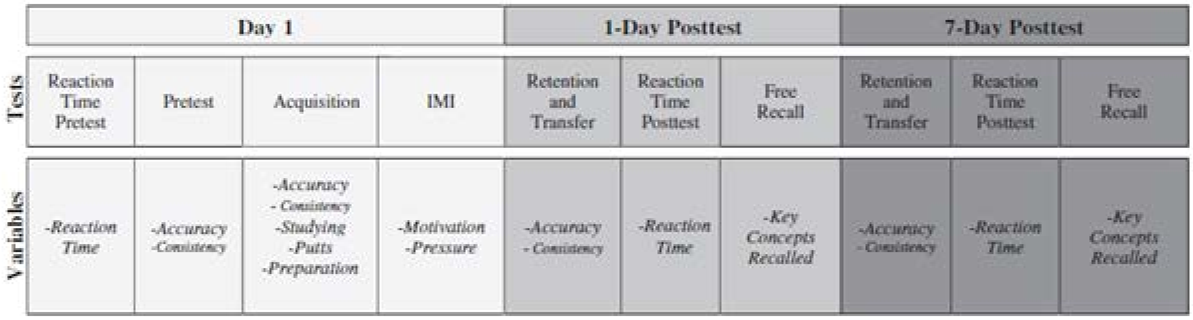
Participants used a standard, right-handed golf putter to putt a standard golf ball on an artificial grass surface to a target cross (+) comprised of two 10.8 cm pieces of white masking tape located 120 cm from a starting position, which was indicated by a 5 cm piece of white masking tape next to participants. The objective was to have the ball stop as close to the center of the target as possible.

Procedure

All participants completed the experiment individually. After consenting to the experiment, they completed a demographic questionnaire asking them their age, gender, and putting experience (anything from miniature golf to playing 18 holes on a standard golf course) over their lifetime and within the past year (only two participants putted more than 30 times in their lifetime, but their pretest and posttest putting results were within 1.72 *SD* of the *M*, so these participants were retained in all analyses).

Pretest

Participants completed the pretest phase, which consisted of two blocks of ten putts with 45 s between each putt (see Figure 1 for chart describing timeline of procedure). As the purpose of this phase was to determine participants' baseline skill level, we attempted to isolate performance and minimize on-line learning by having them wear a blindfold and earplugs while putting (Dyke et al., 2014). In so doing, they were not able to obtain visual or auditory feedback about the outcome of the putt, which is crucial for learning. Of course, this also meant participants were unable to use visual or auditory feedback during putt execution. However, putting is a discrete motor skill relying primarily on feedforward control. Prior to each putt, they were permitted to view the ball and target. Once the putter was placed behind the ball, vision was occluded.



	Day 1				1-Day Posttest			7-Day Posttest		
Tests	Reaction Time Pretest	Pretest	Acquisition	IMI	Retention and Transfer	Reaction Time Posttest	Free Recall	Retention and Transfer	Reaction Time Posttest	Free Recall
Variables	*-Reaction Time*	*-Accuracy* *-Consistency*	*-Accuracy* *- Consistency* *-Studying* *-Putts* *-Preparation*	*-Motivation* *-Pressure*	*-Accuracy* *- Consistency*	*-Reaction Time*	*-Key Concepts Recalled*	*-Accuracy* *- Consistency*	*-Reaction Time*	*-Key Concepts Recalled*

Fig. 1. Timeline of procedure, including tests conducted and variables collected each day.

The sole purpose of the first pretest block was to index the elaborateness of the motor program being utilized for the putting movement. (We were not interested in the accuracy or consistency of these putts). Accordingly, the amount of time participants took to begin their putting movement was recorded. Specifically, participants were told "a tone will be played before each putt. Please putt when you hear the tone." It is critical to note that participants may have interpreted this instruction differently, which could have affected their reaction time. For example, some participants may have felt a greater sense of urgency to putt quickly upon hearing the tone, which would have decreased their reaction time. After the experimenter placed the ball in the starting position, he asked participants to "affirm being ready to putt as accurately as possible." Upon participant affirmation, the experimenter pressed a computer key, which elicited a tone (750 Hz and 90 dB, loud enough to be heard through earplugs) at a random interval 4 – 8 s after the keypress. The tone sent a marker into an amplifier (BrainAmp DC amplifier, Brain

Products GmbH, Munich, Germany) linked to BrainVision Recorder software (Brain Product, GmbH, Munich, Germany). The amplifier was set to record markers at a sampling rate of 5000 Hz. When participants began putter movement, they broke a light beam aligned with the head of their putter. The breaking of the light beam activated a photosensor attached to a BrainVision StimTrak device (BrainProducts GmbH, Munich, Germany), which sent a marker into the amplifier. Participants performed one warm-up trial to ensure the amplifier was capturing the markers representing the tone and putter movement, then participants proceeded on with the block of ten trials.

Next, participants completed 10 putts like they did in Block 1, except the putts were ‘self-paced’ (i.e., there was no tone indicating when participants should begin putting). This second block of putts was used to determine baseline skill level, so putting accuracy and consistency were analyzed.

Acquisition

After the pretest, participants were read instructions according to the group to which they were randomly assigned. Participants in the Teach group were told “tomorrow you will teach another participant how to putt,” and participants in the Test group were told “tomorrow you will be tested on your putting.” Next, participants completed the acquisition phase. First, they were told they had 1 hour to practice and had to remain in the experiment laboratory for the duration of the hour. Next, they were told to start practice by studying a golf putting instruction booklet for at least 2 min, but to take as long to initially study the booklet as they liked. Participants were *not* told they would be tested on information contained in the instruction booklet, so any learning of the material can be considered somewhat incidental. The instructions for the booklet were provided by an expert golfer (for booklet, see the appendix). After studying the booklet, participants were told that they would be performing at least five blocks of ten putts, but that they could take up to an additional five blocks of putts if they liked. Between each block, participants were told they had a 1-min break during which they could study the booklet if they chose to do so. A video camera (Logitech C930e, Logitech International, Newark, CA) recorded the putting starting position area at 30 frames/s during odd-numbered blocks. The purpose of this recording was to index information processing during acquisition by quantifying the duration participants took preparing each putt (preparation). Participants were told that after they finished practicing they could spend the remainder of the hour in the lab doing what they liked (e.g., browsing the Internet), but that, once they stopped practicing, they would not be allowed to resume. When participants stopped practicing, they completed the IMI (McAuley et al., 1989). The subscales of interest were as follows: interest/enjoyment (intrinsic motivation), value/usefulness (internalized motivation), effort/importance (general motivation), and pressure/tension (pressure). All responses were made on a seven-point Likert scale, anchored by “not true at all” and “very true.” Participants completed the perceived competence and perceived choice subscales as well, but we did not analyze them, as we deemed them irrelevant for the experiment.

Retention and transfer

One day after completing Day 1, participants returned to complete the experiment. Participants in the Teach group were told “the participant who you were going to teach did not show up today, so you will actually be tested on your putting instead.” Next, participants completed the retention and transfer test phases in a counterbalanced order. As the purpose of these phases was to determine their skill level achieved due to the previous day’s acquisition phase, we attempted to isolate performance and minimize on-line learning (make them putt from memory) by having participants wear a blindfold and ear-plugs, but they were permitted to view the ball while placing their putter behind it prior to each putt (as in the pretest phase). Also like the pretest, participants were given 45 s between each putt. The retention test was the same as the pretest, whereas the transfer test required participants to putt to a target 170 cm from participants (50 cm farther away from the pretest/acquisition/retention target). The transfer test was included because the ability to adapt a skill to novel parameters (e.g., putt to a farther target) is considered a hallmark of motor learning (e.g., Magill & Anderson, 2014).

Next, participants completed one warm-up putt and ten additional putts to index the elaborateness of the motor program being utilized for the putting movement. Accordingly, the amount of time participants took to begin their putting movement was recorded just like during Day 1.

After participants completed the 10 reaction time putts, we assessed their declarative knowledge by asking them to complete a free recall test wherein they were told “to report, in as much detail as possible, any rules, methods, or techniques [they recalled] using to execute putts." Finally, participants were debriefed about the purpose of the experiment, and Teach participants were told they were deceived into expecting to teach another participant and asked if they still wanted to have their data included in the experiment (they all did). All participants were asked to return for a third day of the experiment, which would follow the same procedures as the one they had just completed (i.e., all participants expected to be tested on Day 3). Approximately seven days after Day 1, participants completed the same procedure as in Day 2, except they were not debriefed.

Data Processing

Putting accuracy and consistency

Putting accuracy was indexed by recording radial error, and consistency was indexed by recording bivariate variable error as recommended by Hancock, Butler, and Fischman (1995). During the acquisition phase, accuracy and consistency were calculated for the odd-numbered blocks to get a general assessment of improvement during acquisition (without overly slowing data collection). Accuracy and consistency were calculated for all self-paced blocks of the pretest and posttest phases in order to assess motor learning.

Reaction Time

To index reaction time, the time between each tone marker and the corresponding putter movement marker was determined with BrainVision Analyzer 2.1 (BrainProducts GmbH, Munich, Germany). The reaction times were then averaged separately for pretest, 1-day posttest, and 7-day posttest.

Free recall

To measure free recall, the number of key concepts from the putting instruction booklet correctly recalled by participants was recorded (Daou et al., in press). The key concepts were the most important concepts, as indicated by the expert golfer who provided the putting instructions: (1) “establish proper grip,” (2) “place the putter head behind the ball and take a hip-width stance,” (3) “place the eyes directly over the ball by hinging from the hips,” and (4) “stroke the ball without breaking the wrists.”

Preparation

To index preparation, the duration from when the experimenter placed the ball in the starting position to when the participant began the putting movement was determined via frame-by-frame analysis of the video recorded during the first, third, and fifth acquisition blocks. The first putt of each of these blocks was excluded from analysis, because participants were often reorienting themselves to the task, asking the experimenter a question, etc.

Motivation and pressure

Items were averaged within the intrinsic, internalized, generalized motivation, and pressure IMI subscales to create a single score for each subscale.

Quantity of studying and practicing

The amount of time participants spent looking at the putting instruction booklet was recorded in order to quantify the amount of time they spent studying the skill (studying) during the acquisition phase. Specifically, during the initial study phase of at least 2 min, a researcher controlled a chronometer on a computer located less than 2 min from the participant. The researcher used the chronometer to record the time the participant was looking at the booklet, as opposed to looking elsewhere. After the initial 2 min, the researcher told the participant that the "two minutes time is over, but you may study the booklet for as long as you want." When participants told the researcher that they were done studying, the researcher recorded the final study time for the initial study phase. During the 1 min breaks between acquisition blocks, the researcher used the chronometer to record the amount of time the participant spent looking at the instruction booklet. The initial study time was then combined with the between-blocks study time to yield the 'studying' variable. The number of putts they took during the acquisition phase (putts) was recorded to quantify their practice repetitions. Time spent studying and number of putts was collected for between-group analyses and to be used as covariates (a priori) in the analysis of accuracy and consistency.

Statistical Analysis

To measure motor learning, one hierarchical multiple regression was conducted for accuracy, averaged across all four posttests (1-day retention, 1-day transfer, 7-day retention, and 7-day transfer), and another regression was conducted for consistency, averaged across all posttests. The first step of the regressions included group (test or Teach) and pretest accuracy or consistency (depending on the dependent variable) as predictors, and the second step added studying and putts as predictors.

In order to justify averaging across posttests for accuracy and consistency, separate mixed-factor ANCOVAs for accuracy and consistency were conducted with group serving as the between-subjects factor, posttest type (retention or transfer) as well as posttest day (1-day or 7-day) serving as the within-subjects factors, and pretest accuracy or consistency, studying, and putts serving as covariates. Significant Group x Posttest Type, Group x Posttest Day, or Group x Posttest Type x Posttest Day interactions would indicate that separate regressions should be conducted for the posttests, whereas nonsignificant interactions would indicate that accuracy and consistency could be collapsed across the posttests, thus reducing the number of regressions conducted (Lohse, Buchanan, & Miller, in press).

To assess reaction time, a mixed-factor ANCOVA was conducted with group serving as the between-subjects factor, posttest day serving as the within-subjects factor, and pretest reaction time serving as the covariate. Due to equipment malfunction, four participants (all in the Test group) did not have data for one or more of the reaction time assessments, so these participants' data were excluded from the reaction time analysis. To assess free recall, a mixed-factor ANOVA was conducted with group serving as the between-subjects factor and posttest day serving as the within-subjects factor.

To assess group differences during the acquisition phase, mixed-factor ANCOVAs were conducted for accuracy and consistency with group serving as the between-subjects factor, block (Acquisition Blocks 1, 3, and 5) serving as the within-subjects factor, and pretest accuracy or consistency serving as the covariate. Additionally, independent sample *t*-tests (group) were conducted for studying and putts.

To assess possible mechanisms underlying learning effects, independent sample *t*-tests (group) were conducted for the following dependent variables: intrinsic motivation, internalized motivation, general motivation, pressure, and preparation. Next, any mechanism that exhibited significant group differences was entered into a regression to determine if it predicted posttest accuracy or consistency, controlling for group and pretest accuracy or consistency. Alpha levels were set to .05 for all tests, and the Greenhouse-Geisser correction is provided when sphericity was violated. The *p*-values reported are based on the corrected degrees of freedom.

Independent sample *t*-tests (group) were conducted for age, gender, lifetime putting experience, and past-year putting experience.

Results

Demographics

Analyses revealed no group differences with respect to age, lifetime putting experience, or past-year putting experience (*p*s ≥ .328), but a significant difference with respect to gender (*p* = .003, see Table 1). Thus, we conducted supplementary analyses to determine whether gender significantly affected any other variables and, when it did, we controlled for gender when assessing the affected variables. The results of these analyses were in exact correspondence (in terms of statistical significance) with the results presented below, which did not control for gender (see Supplementary Online Material).

Table 1. Descriptive data for each group. *CI* is 95%.

Descriptive Data by Group				
	Test (*n* = 28; 21 females)		Teach (*n* = 28; 10 females)	
	M	*CI*	*M*	*CI*
Age (Years)	21.3	20.5 – 22.1	21.8	21.0 – 22.6
Lifetime Putting Experience[a]	1.64	0.700 – 2.58	1.14	0.830 – 1.45
Past-Year Putting Experience[a]	0.429	0.120 – 0.740	0.429	0.240 – 0.620
Studying (s)	181	157 – 205	318	268 – 368
Putts	53.6	51.2 – 56.1	60.3	54.2 – 66.4
Intrinsic Motivation	5.59	5.27 – 5.91	5.49	5.16 – 5.82
Internalized Motivation	5.38	5.00 – 5.76	5.61	5.28 – 5.94
General Motivation	5.89	5.58 – 6.20	6.17	5.89 – 6.45
Pressure	2.18	1.76 – 2.60	2.76	2.37 – 3.15
Preparation (s)	5.41	4.69 – 6.13	8.85	7.82 – 9.88
Reaction Time Pretest (s)	0.412	0.369 – 0.455	0.504	0.440 – 0.568
Reaction Time Posttest 1 (s)	0.429	0.379 – 0.479	0.524	0.448 – 0.600
Reaction Time Posttest 2 (s)	0.391	0.337 – 0.448	0.483	0.419 – 0.547
Free Recall	2.23	1.81 – 2.65	2.96	2.69 – 3.23

[a] 0 = Never putted; 1= Putted 1 – 10 times; 2 = Putted 11 – 20 times; 3 = Putted 21 – 30 times

Acquisition Accuracy, Consistency, Studying, and Putts

Figure 2A and 2B show accuracy and consistency, respectively, for the groups across all phases of the experiment. Analyses of accuracy and consistency during the acquisition phase revealed no significant effects for group, block, or Group x Block interaction (*p*s ≥ .163), controlling for pretest accuracy and consistency. Additionally, the groups did not differ with respect to putts (*p* = .054), but the Teach group (*M* = 318 s) exhibited significantly more studying than the Test group (*M* = 181 s, *p* < .001, *d* = 2.79, see Table 1). Notably, studying did not predict posttest accuracy or consistency, when controlling for pretest accuracy or consistency (*p*s ≥ .141).

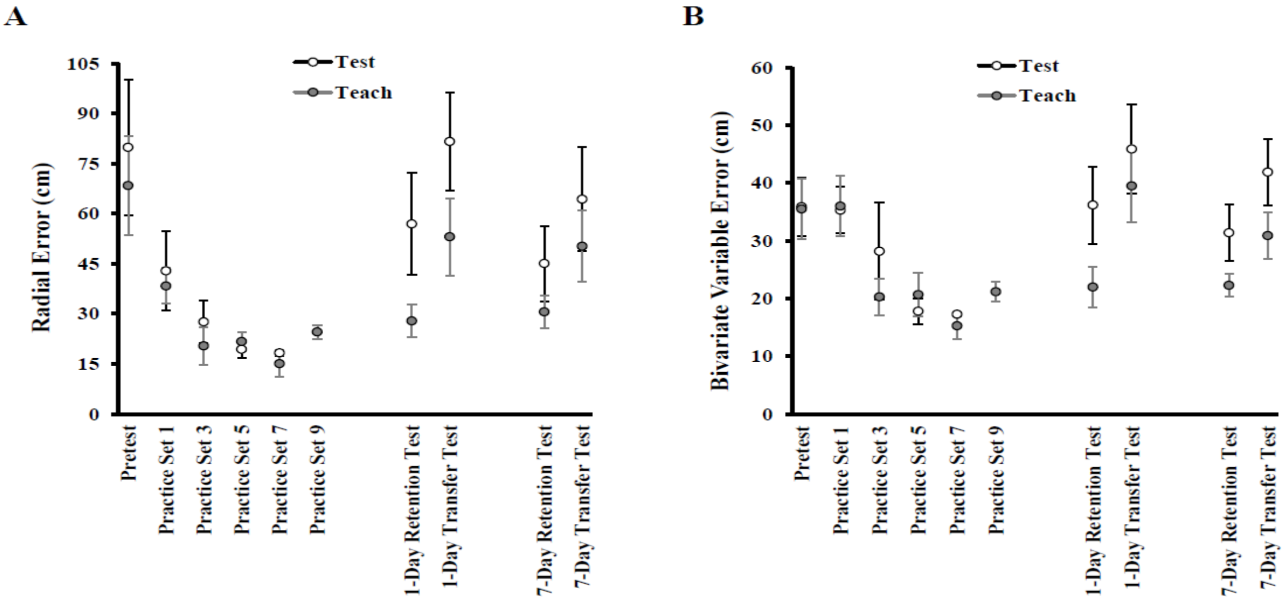


Figure 2. A) Accuracy (lower error indicates greater accuracy) as a function of experimental phase and group. B) Consistency (lower error indicates greater consistency) as a function of experiment phase and group. All error bars represent 95% CIs.

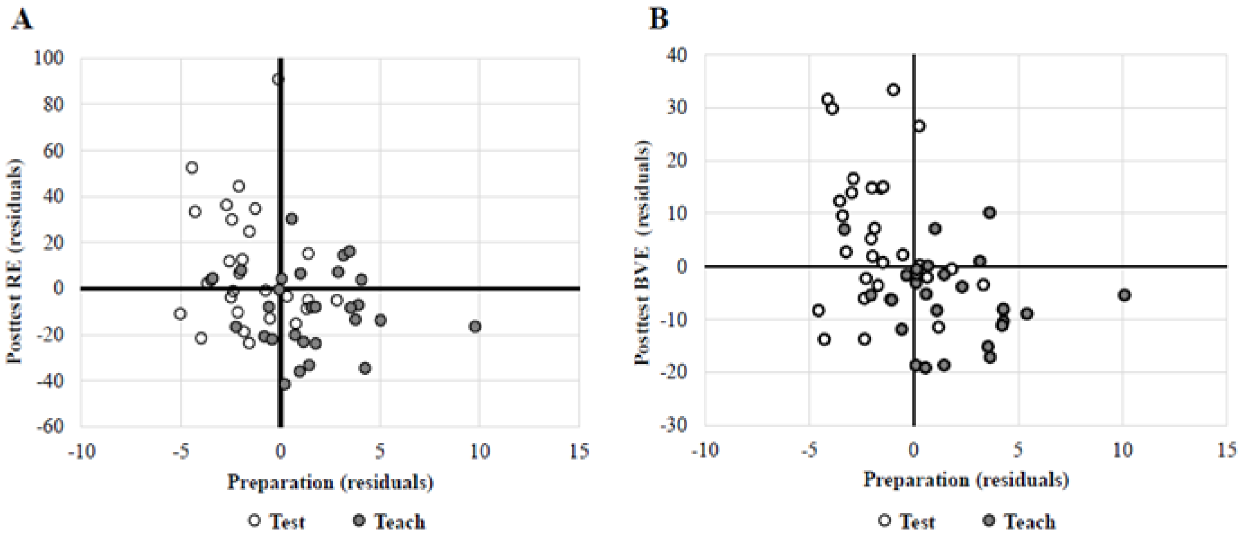


Figure 3. A) Posttest accuracy as a function of preparation controlling for pretest accuracy. Higher values on the x-axis represent longer preparation, and lower values on the y-axis represent less error (greater accuracy). Cases are identified by group (teach/test). B) Posttest consistency as a function of preparation controlling for pretest accuracy. Higher values on the x-axis represent longer preparation, and lower values on the y-axis represent less error (greater consistency). Cases are identified by group (teach/test).

Analyses of posttest accuracy and consistency revealed nonsignificant Group x Posttest Type, Group x Posttest Day, and Group x Posttest Type x Posttest Day interactions (*p*s ≥ .075), so accuracy (or consistency) was averaged across all posttests for the regressions. The first step of the regression for accuracy revealed a significant effect of group controlling for pretest accuracy (β_{group} = -18.8 cm, p = .003; see Table 2). The second step of this regression revealed a significant effect of group controlling for pretest accuracy, studying, and putts (β_{group} = -19.5 cm, p = .011).

The first step of the regression for consistency revealed a significant effect of group controlling for pretest consistency β_{group} = -12.3 cm, $p < .001$; see Table 3). The second step of this regression revealed a significant effect of group controlling for pretest consistency, studying, and putts (β_{group} = -15.9 cm, $p < .001$).

Posttest Reaction Time and Free Recall

Analyses revealed that reaction time was not affected by group and no Group x Posttest Day interaction was present (ps ≥ .309). However, group did have a main effect on free recall ($F(1, 54) = 8.24$, $p = .006$, $\eta_p^2 = .132$), and no Group x Posttest Day interaction was observed (p = .259). The main effect indicated the Teach group (M = 2.96) recalled more key concepts than the Test group (M = 2.23, see Table 1).

Mechanistic Variables

Analyses revealed that the groups did not differ with respect to intrinsic, internalized, or general motivation (ps ≥ .197), nor did they differ with respect to pressure (p = .051). Conversely, the groups did differ with respect to preparation ($t(54) = 5.34$, $p < .001$, $d = 1.45$). Specifically, the Teach group spent longer (M = 8.85 s) preparing their putts during the acquisition phase than did the Test group (M = 5.41 s). However, preparation did not predict posttest accuracy when controlling for group and pretest accuracy (*p* = .557), nor did preparation predict posttest consistency when controlling for group and pretest consistency (*p* = .245). Nonetheless, we conducted exploratory regression analyses to determine whether preparation predicted posttest accuracy or consistency, when controlling for just pretest accuracy or consistency (not controlling for group). We observed preparation did indeed predict posttest accuracy ($\beta_{preparation}$ = -2.44 cm, *p* = .032; Figure 3A) and consistency ($\beta_{preparation}$ = -1.74 cm, *p* = .002; Figure 3B).

Table 2. Details of regression models testing the hypotheses that expecting to teach enhances motor learning (as indexed by superior posttest accuracy) not controlling for the amount of time spent studying the instruction booklet and the number of putts taken (Model 1) and controlling for these factors (Model 2). Regression coefficients are not standardized and are thus interpretable in their natural units. For the Group variable, Test = '0' and Teach = '1.' *CI* is 95%.

Model 1: Posttest Accuracy ~ Pretest + Group					
	SS	*Df*	*MS*	*F*	R^2 *Change*
Regression	13,830	2	6915	13.8	.121
Residual	26,625	53	502		
Coefficients	β	*CI*	*t*-value	*p*-value	
Intercept	42.7	29.5 – 55.9	6.48	< .001	
Pretest Accuracy	0.242	0.115 – 0.369	3.82	< .001	
Group	-18.8	-30.9 – -6.70	3.12	.003	

Model 2: Posttest Accuracy ~ Pretest + Group + Studying + Putts					
	SS	*Df*	*MS*	*F*	R^2 *Change*
Regression	13,858	4	3465	6.64	.001
Residual	26,597	51	522		
Coefficients	β	*CI*	*t*-value	*p*-value	
Intercept	39.8	10.6 – 69.0	2.74	.009	

Pretest Accuracy	0.240	0.110 – 0.371	3.70	.001	
Group	-19.5	-34.3 – -4.66	2.64	.011	
Studying	0.003	-0.059 – 0.064	0.087	.931	
Putts	0.047	-0.475 – 0.570	0.182	.856	

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Model 1: Posttest Consistency ~ Pretest + Group					
	SS	*Df*	*MS*	*F*	R^2 *Change*
Regression	2567	2	1288	10.8	.237
Residual	6351	53	120		
Coefficients	β	*CI*	*t*-value	*p*-value	
Intercept	39.3	29.9 – 48.7	8.42	< .001	
Pretest Consistency	0.188	-.008 – 0.384	1.92	.060	
Group	-12.3	-18.2 – -6.44	4.22	< .001	

Model 2: Posttest Consistency ~ Pretest + Group + Studying + Putts					
	SS	*Df*	*MS*	*F*	R^2 *Change*
Regression	3013	4	753	6.49	.049
Residual	5915	51	116		
Coefficients	β	*CI*	*t*-value	*p*-value	
Intercept	37.6	23.6 – 51.6	5.38	< .001	
Pretest Consistency	0.161	-0.044 – 0.366	1.58	.121	
Group	-15.9	-22.9 – -8.88	4.55	< .001	
Studying	0.028	-0.001 – 0.057	1.94	.059	
Putts	-0.041	-0.297 – 0.214	0.326	.746	

Discussion

Results reveal expecting to teach enhances posttest accuracy and consistency, even after controlling for quantity of skill study and practice. Thus, results suggest expecting to teach directly enhances motor learning. This learning enhancement was accompanied by superior free recall of key concepts, indicative of greater declarative knowledge. The learning enhancement was not associated with longer reaction times to begin movement during posttests, which could indicate that expecting to teach yields better motor program parameterization rather than a more elaborate motor program (Henry & Rogers, 1960). However, this inference is very tenuous given the other factors that could have affected reaction time, including participants' interpretation of timed test instructions. Regarding the mechanisms underlying the learning enhancement, results revealed that expecting to teach did not increase motivation or pressure, but it did increase the duration participants took preparing each putt. Additionally, this increased preparation predicted superior posttest accuracy and consistency (although not when controlling for group). Thus, results provide modest evidence that expecting to teach enhances motor learning by increasing information processing.

The result that expecting to teach *directly* enhances motor learning replicates Daou et al. (in press). Notably, the magnitudes of the expecting to teach effects in the two experiments were quite similar (accuracy: β_{group} = -22.8 and -19.5 cm in Daou et al. and present experiment, respectively; consistency: β_{group} = -14.8 and -15.9 cm in Daou et al. and present experiment, respectively). (The comparison of unstandardized β coefficients is warranted because the task and regression models in both experiments were exactly the same). Additionally, present results suggest the learning benefit is relatively durable, as both 1- and 7-day posttests were employed, as opposed to Daou et al., who only used a 1-day posttest. The present experiment's second day of posttests also permits the conclusion that neither Daou et al.'s results nor the present results were confounded by Teach participants having their expectations violated prior to testing. This follows because Teach participants did not have their expectations violated at second day of posttests in the present experiment.

The present result that expecting to teach enhances free recall also replicates Daou et al. (in press), as well as some other studies assessing declarative knowledge (Bargh & Schul, 1980; Benware & Deci, 1984; Nestojko, Bui, Kornell, & Bjork; 2014). Notably, present results differ from Daou et al. in that expecting to teach increased the *quantity* of skill study in addition to augmenting the *quality* of skill study and practice, as reflected by superior motor learning and free recall. Since Daou et al. did not observe expecting to teach enhanced the amount of time spent studying, our conflicting results require future investigation.

Present results replicate Daou et al. (in press) in that neither self-reported motivation nor pressure explained the effects of expecting to teach on motor learning. Crucially, however, the present results suggest the learning benefit of expecting to teach may be explained by greater information processing, as reflected by longer motor preparation. Yet, it is unclear what type of information was being processed during preparation. Notably, if participants were thinking about movement production (adopting an internal focus of attention), it could be argued that learning should have been hindered, whereas if they were thinking about movement effects (adopting an external focus of attention), learning should have been enhanced (Wulf & Prinz, 2001). (It is worth noting that not all research suggests focus of attention strongly affects learning, showing its strongest effects on performance [Lohse, Sherwood, & Healy, 2014]). The ‘black box’ nature of the preparation measure limits its qualitative interpretation. Future research could employ electroencephalography to shed light upon the type of information being processed during preparation (e.g., Zhu, Poolton, Wilson, Maxwell, & Masters, 2011). Additionally, to provide stronger evidence that information processing does indeed explain the learning benefit of expecting to teach, future work could attempt to experimentally control information processing, by limiting Teach participants’ preparation, and observing learning outcomes. Finally, information processing at other times outside of motor preparation, such as feedback processing (e.g., Grand et al., 2015), could be assessed.

Conclusions

In conclusion, present results replicate those of Daou et al. (in press), giving greater credence that the benefit of expecting to teach on motor learning is a real phenomenon. Further, present results suggest enhanced information processing may underlie the phenomenon. As future research attempts to test this possible mechanism, other studies should have participants actually engage in teaching, which is critical from a practical standpoint. Another factor limiting the external validity of the present study is that participants’ had their vision and hearing occluded during testing, which wasn’t the case during acquisition. In future studies, we recommend participants be tested under both ecologically-valid conditions that match task constraints during acquisition and with vision/audition occluded to control for online learning.

Recent evidence suggests that having learners study with the expectation of teaching and then actually having them teach increases declarative knowledge relative to the expectation of teaching alone (Fiorella & Mayer, 2014, Experiment 2). Accordingly, it is likely having learners study and practice motor skills with the expectation of teaching and then having the learners actually teach the skills is a practical way to continually enhance learning.

Chapter 3: Does practicing a skill with the expectation of teaching alter motor preparatory cortical dynamics?

Introduction

Determining practical ways to enhance people's learning is a challenge in the field of motor behavior. One way might be having people study and practice a skill with the expectation of teaching it to another person. There are several mechanisms whereby expecting to teach could enhance motor learning. First, expecting to teach may cause a learner to recognize their learning affects another person's learning. This recognition might increase the learner's motivation (Benware & Deci, 1984; Fiorella & Mayer, 2014, Experiment 1), which has been positively linked to motor learning (Wulf & Lewthwaite, 2016). Second, the learner could have elevated anxiety (pressure) by identifying their responsibility in facilitating another person's learning. This elevated anxiety may yield arousal levels that are adaptive for learning (Yerks, & Dodson, 1908). Third, expecting to teach could enhance information processing. Specifically, learners expecting to teach could engage in greater information processing while practicing, which has been positively associated with motor learning (Cross, Schmitt, & Grafton, 2007). For example, knowing that they have to teach another person, a learner might use working memory and verbal-analytic processes (e.g., instructional self-talk) to implement proper skill technique, more elaborate programming and parameterizing for their movements, and more attentional monitoring of their movements, all of which could lead to better skill retention.

Daou, Buchanan, Lindsey, Lohse & Miller (2016) conducted the first experiment testing the hypothesis that studying and practicing a skill with the expectation of teaching it enhances motor learning. Specifically, the authors examined the skill of golf putting and tested motivation and pressure as possible mechanisms related to an effect of expecting to teach. Results from Daou, Buchanan et al. revealed that participants who were expecting to teach exhibited superior posttest putting accuracy and precision (enhanced motor learning), but not increased motivation or pressure. Further, participants who were expecting to teach remembered more key concepts about golf putting on a free recall test, in accord with some literature indicating expecting to teach enhances declarative memory (Bargh & Schul, 1980; Benware & Deci, 1984; Nestojko et al., 2014).

As Daou, Buchanan et al. (2016) observed expecting to teach does indeed enhance motor learning, but motivation and pressure did not explain the expecting to teach effect, Daou, Lohse & Miller (2016) investigated whether information processing could explain the effect. Specifically, Daou, Lohse et al. sought to index information processing during practice by quantifying the amount of time participants took preparing each putt. Results revealed that expecting to teach increased the duration participants took preparing each putt and improved motor learning, the latter replicating Daou, Buchanan et al.'s findings. Additionally, the increased putt preparation time during practice predicted superior posttest accuracy and precision (although not when controlling for group [i.e., whether participants were expecting to teach or not]). Thus, Daou, Lohse et al.'s results provide modest evidence that expecting to teach enhances motor learning by increasing information processing during motor preparation.

Daou, Lohse et al. (2016) revealed motor preparatory processing during practice may explain the expecting to teach effect, however the authors did not investigate the specific preparatory processes. Therefore, the present study sought to examine particular motor preparatory processes reflected by cortical dynamics while participants prepared to putt during practice. To assess motor preparatory cortical dynamics, electroencephalography (EEG) was employed. A number of experiments have used EEG to investigate cortical dynamics related to motor preparatory processes (for reviews, see Cooke, 2013; Hatfield, Haufler, Hung, & Spalding, 2004). For instance, spectral power in the theta frequency bandwidth (4 – 7 Hz) at the frontal midline is a variable positively associated with attention employed for working memory and action monitoring while people are performing a task (Doppelmayr, Finkenzeller, & Sauseng, 2008; Dyke et al., 2014; Gevins, Smith, McEvoy, & Yu, 1997; Kao, Huang & Hung, 2013; Weber & Doppelmayer, 2016). For example, Weber and Doppelmayer (2016) observed participants exhibited increased frontal midline theta power during motor preparation for a dart throw after 15 sessions of mental and physical dart throwing practice, presumably because the practice required participants to engage working memory and action monitoring processes.

Another important variable is spectral power in the upper-alpha bandwidth (10 – 12 Hz) overlying motor cortex, which is *negatively* associated with cortical resource allocation to accurate motor programming (Babiloni et al., 2008; Cooke et al., 2014; Cooke et al., 2015). For instance, Cooke et al. (2014) observed participants exhibited decreased upper-alpha power over motor cortex during motor preparation for successful (holed) versus unsuccessful (missed) putts, suggesting decreased upper-alpha was associated with more accurate motor programming.

Further, another variable related to motor preparatory processes is upper-alpha T7-Fz coherence, which is positively associated with the degree of communication between left temporal lobe and premotor cortex, with more communication indicating greater verbal-analytic information being processed in order to translate the information into motor planning (Buszard, Farrow, Zhu, & Masters, 2016; Cheng et al., 2017; Deeny, Hillman, Janelle, & Hatfield, 2003; Deeny, Haufler, Saffer, & Hatfield, 2009; Gallicchio, Cooke, & Ring, 2016; Gallicchio, Cooke, & Ring, 2017; Gentili et al., 2015; Rietschel et al., 2012; Zhu et al., 2010; Zhu et al., 2011; Zhu, Poolton, Wilson, Maxwell, & Masters, 2011). For example, Zhu et al. (2011) observed participants predisposed to use verbal-analytic processes during movement exhibited elevated left temporal-premotor coherence during motor preparation for golf putts relative to counterparts not predisposed to use verbal-analytic processes during movement, suggesting the coherence differences were due to variation in participants' tendency to use verbal-analytic processes during movement.

Finally, another variable related to motor preparatory processes is the readiness potential (RP). Unlike the aforementioned variables, the RP is a time-domain variable, in particular a negative-going wave with a central scalp distribution that precedes movement execution (Brunia, Van Boxtel, Bocker, 2012). The RP can be divided into early (~ between 2000 ms and 1500 ms preceding movement) and late (~between 500 ms and 400 ms preceding movement) subcomponents. According to Brunia et al. (2012), the early-RP reflects motor program selection, with greater early-RP amplitude reflecting more cortical resources devoted to program selection. Conversely, the late-RP reflects the specification of movement parameters required for accuracy and precision, with greater late-RP amplitude indicating more cortical resources allocated to parameterization. Brunia et al.'s opinion is based on a review of studies showing early-RP is modulated by factors such as movement selection, whereas late-RP is altered by variables such as movement precision (also see Shibasaki & Hallett (2006)'s review). Notably, Daou, Lohse et al. concluded that expecting to teach did not increase the elaborateness of the motor program used for putting, but this conclusion was inferred from reaction times recorded during pretest and posttest (Henry & Rogers, 1960). Thus, Daou, Lohse et al. concluded expecting to teach likely improves the specification of motor program parameters, but does not affect motor program selection.

Based on Daou, Lohse et al. (2016), we predicted participants expecting to teach would exhibit greater motor preparatory processing while practicing a skill, and this increased processing would be reflected in the aforementioned EEG variables. Specifically, we predicted participants expecting to teach would exhibit: (1) greater frontal midline theta (attempt to keep more skill information in mind and monitor their actions to greater extent); (2) less motor upper-alpha (allocate more cortical resources to motor programming); (3) higher T7-Fz coherence (show more verbal-analytic information being translated into motor planning); and (4) greater late-RP amplitude (engaging in more deliberate movement parameter specification).

Methods

Prior to beginning data collection, the experimental design and analyses were registered and made public on AsPredicted.org (https://aspredicted.org/dt4gj.pdf).

Participants

Sixty right-handed young adults (28 females), ages between 18 and 35 years (M_{age} = 21.1 years, *SD* = 1.53 years), participated in the study after consenting to a protocol approved by the Auburn University Institutional Review Board (#16-484 EP 1612). Participants were recruited from university courses and by word-of-mouth, and were compensated with course credit and/or entry into a raffle for a monetary award. Sample size was determined with an a priori power calculation providing 80% power ($\alpha \leq .05$) to detect a moderate-sized effect ($f^2 = .15$) when adding a practice phase EEG variable (e.g., frontal midline theta or motor-upper alpha) to the multiple regression model predicting posttest performance (accuracy/precision) controlling for group, pretest performance, and the pretest EEG variable (i.e., one variable being tested with four total predictors; Faul, Erdfelder, Lang, & Buchner, 2007). The power calculation yielded *N* of 55, but it was decided to include 60 participants because past studies in our lab recording EEG from participants putting excluded about 8% of participants due to poor EEG recording (Dyke et al., 2014).

Task

All participants used a standard, right-handed golf putter to putt a standard golf ball from a starting position indicated by a 5 cm line painted in white washable paint on an artificial grass surface to a target cross (+) comprised of two 10.8 cm lines painted in white washable paint. Participants’ objective was to make the ball stop as close to the center of the target as possible.

Procedure

All participants completed the experiment individually. After consenting to the experiment, they completed a demographic questionnaire asking their age, sex, and putting experience (anything from miniature golf to playing 18 holes on a standard golf course) over their lifetime and within the past year.

Pretest. After completing the demographic questionnaire, participants were asked to perform the pretest phase, which consisted of one block of ten putts. The target was located 300 cm away from the starting position.

Practice. After pretest, participants were quasi-randomly assigned to teach or test group. Quasi-randomization was based on pretest accuracy score. Specifically, participants' pretest accuracy placed them in one of three categories (< 24 cm, 24 - 49 cm, > 49 cm),[1] within which they were randomly assigned to the teach group or test group. After quasi-randomization, participants in the teach group were told “tomorrow you will teach another participant how to putt,” and participants in the test group were told “tomorrow you will be tested on your putting.” Next, participants were asked to complete the practice phase. First, they were asked to study a golf putting instruction booklet for 2 min. The instructions for the booklet were provided by an expert golfer (for booklet, see the appendix). After studying the booklet, participants were told to perform six blocks of ten putts to the same target as pretest, taking a 1-min break between each block (participants were asked to sit in a chair for this rest period). When participants stopped

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[1] Category range was based on pilot data (*N* = 12)

practicing, they completed the Intrinsic Motivation Questionnaire (IMI) (McAuley, Duncan, & Tammen, 1989). The subscales of interest were as follows: interest/enjoyment (intrinsic motivation), value/usefulness (internalized motivation), effort/importance (general motivation), and pressure/tension (pressure). All responses were made on a seven-point Likert scale, anchored by ‘‘not true at all” and ‘‘very true.” Participants completed the perceived competence and perceived choice subscales as well, but they weren’t analyzed, because they are simply correlates of intrinsic motivation, which is more directly linked to motor learning (Wulf & Lewthwaite, 2016).

Posttests (retention and transfer). One day after completing Day 1 (pretest and practice phase), participants returned to complete the experiment. Participants in the teach group were told “the participant who you were going to teach did not show up today, so you will actually be tested on your putting instead.” Next, participants completed the retention and transfer test phases in a counterbalanced order. The target for the retention test was located at the same distance as the pretest and practice phase, whereas the transfer test required participants to putt to a target located 100 cm farther away from the pretest/practice/retention target (400 cm away of starting position). Transfer tests are included in posttests to investigate the generalizability concept of transferring a skill to novel parameters (i.e., reparameterizing to putt to a farther target; Magill & Anderson, 2014). After completing retention and transfer tests, participants were asked to complete a free recall test which asked participants “to report, in as much detail as possible, any rules, methods, or techniques [they recalled] using to execute putts.” Next, we asked participants whether they did anything to prepare (e.g., mental rehearsal, watching golf putting videos) for day 2 either (a) on day 1 after practicing and/or (b) on day 2 before completing the experiment. Finally, participants were debriefed regarding the purpose of the study and dismissed.

EEG recording. EEG was recorded during pretest and practice phases. Scalp EEG was collected from 20 channels of an EEG cap housing a 64 channel BrainVision actiCAP system (Brain Products GmbH, Munich, Germany) labeled in accord with an extended international 10-20 system (Oostenveld & Praamstra, 2001). EEG data was sampled at 250 Hz. EEG data was online-referenced to the left earlobe, and a common ground was employed at the FPz electrode site. Electrode impedances were maintained below 25 kΩ throughout the study and a high-pass filter was set at 0.016 Hz. The EEG signal was amplified and digitized with a BrainAmp DC amplifier (Brain Products GmbH) linked to BrainVision Recorder software (Brain Products GmbH).

This study aimed to investigate cortical dynamics during motor preparation. Specifically, we were interested in the time window from 3 s prior until 1 s after the start of the backswing on each putt. The beginning of the time window was chosen based on Daou, Lohse et al (2016)'s results in which 55 out of 56 participants took at least 3 s preparing to putt during practice phase. For analysis, this time window was subdivided into the following epochs: 3s to -2s, -2s to -1s, -1s to 0s, and 0s to +1s, where 0 s represents the onset of putter movement (e.g. Cooke et al, 2014; Cooke et al, 2015). To capture the onset of putter movement, a photosensor was placed opposite a flashlight on either side of the putter in its starting position. Thus, the putter blocked the flashlight's beam from activating the photosensor until participants moved the putter to initiate their backswing. When participants initiated their backswing, the light beam activated the photosensor, which was attached to a BrainVision StimTrak device (BrainProducts GmbH). This device then sent a marker into the EEG signal (see Figure 1). As the photosensor is a sensitive device and may capture any movement that breaks the light beam (e.g., movements other than the backswing), a manual marker was also inputted when participants began their backswing. This manual marker denoted which photosensor marker was associated with the backswing. For four participants, the photosensor failed to send the marker, so their EEG data were excluded from analysis.

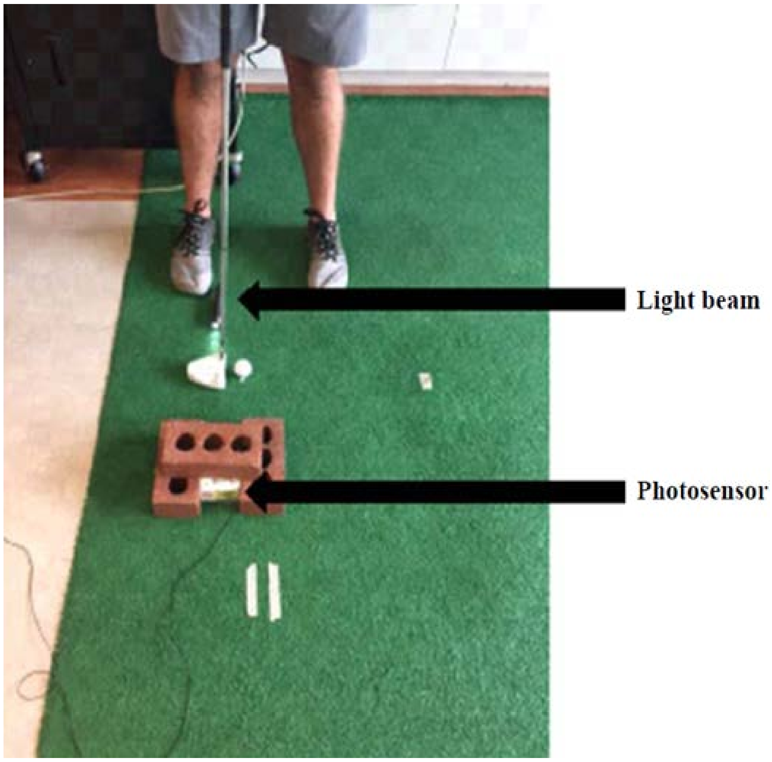

Fig. 1. Pictorial description of method to record initiation of backswing. When the participant began backswing, the putter head moved, allowing the light beam to stimulate the photosensor, which sent a marker into the EEG signal.

Data Processing

Performance and learning. Putting accuracy and precision were measured because they are separate and critical aspects of motor learning (Fischman, 2015). Specifically, accuracy was indexed by recording radial error as recommended by Hancock, Butler, and Fischman (1995): $Radial\ Error\ (RE) = (x^2 + y^2)^{1/2}$, where x and y represent the magnitude of error along the respective axes (i.e., how far away from the target cross the ball stopped in the horizontal and vertical directions). Precision was indexed by recording bivariate variable error as recommended by Hancock et al.: $Bivariate\ Variable\ Error\ (BVE) = [(\frac{1}{k})\sum_{i=1}^{k}[(x_i - x_c)^2 + (y_i - y_c)^2]\}^{1/2}$, where k = trials in a block and $_c$ = centroid along the given axis (x or y) for that block. RE and BVE were calculated over pretest (10 putts) to get a baseline skill level, as well as during the six blocks (60 putts) of the practice phase to get an assessment of improvement in performance. To assess motor learning, RE and BVE were calculated for the retention and transfer tests during the 1-day-delayed posttest phase.

EEG processing for frequency domain. All EEG data processing was conducted with BrainVision Analyzer 2.1 software (BrainProducts GmbH). First, photosensor markers corresponding to putt initiation were identified based on the manual markers. Next, data were re-referenced to an averaged ears montage, band-passed filtered between 1 and 45 Hz with 4th-order rolloffs and a 60 Hz notch employing a zero-phase shift Butterworth filter. Next, eye-blinks were reduced employing the ICA-based ocular artifact correction function within the BrainVision Analyzer software (electrode FP2 served as the vertical and horizontal electrooculography channel; BrainProducts, 2013). This function searches for an ocular artifact template in channel FP2, and then finds ICA-derived components that account for a user specified (70%) amount of variance in the template-matched portion of the signal from FP2. These components were removed from the EEG signal, which then was reconstructed for further processing. Next, EEG was segmented into the aforementioned motor preparatory time window (3s prior to and 1s after putter movement onset) for each putting trial. Then, trials with visually obvious artifact from non-brain sources were manually rejected and excluded from subsequent analysis. For pretest, an average of 19.3% of trials per participant were rejected (each participant had at least 5 trials). For practice, an average of 14.7% of trials per participant were rejected (each participant had at least 33 trials). Next, a fast Fourier transformation was employed using 0.977 Hz bins and a Hamming window (50% taper) for spectral power. Then, trials were averaged separately for the pretest and practice phases. For frontal midline theta, spectral power was averaged from 4 – 7 Hz for the Fz electrode and natural log transformed. For motor upper-alpha, spectral power was averaged from 10 – 12 Hz, natural log transformed, and then averaged across the following electrodes overlying motor regions: FC3, FCz, FC4, C3, Cz, and C4. T7-Fz coherence in the upper-alpha bandwidth was calculated using the following formula: $Cxy(f)|^2$. T8-Fz coherence was also calculated to examine visuospatial information being translated into motor planning (Buszard et al., 2016; Deeny et al., 2003; Deeny et al., 2009; Gallicchio et al., 2016; Zhu et al., 2010; Zhu, Poolton, Wilson, Hu et al., 2011; Zhu, Poolton, Wilson, Maxwell et al., 2011). Both coherence values were subjected to Fisher z transformation.

EEG processing for readiness potential. Since RP requires at least 30 trials to obtain a reliable average, it was only quantified for the practice phase (Brunia et al., 2012). EEG data were re-referenced to an averaged ears montage and low-pass filtered at 10 Hz with a 4th-order rolloff employing a zero-phase shift Butterworth filter. Then, ocular artifact correction and signal reconstruction were implemented as with the other EEG variables. Next, EEG was segmented into a time window from -2200 – 100 ms, where 0 ms represents the initiation of the putter movement. Next, trials with visually obvious artifact from non-brain sources were manually rejected and excluded from subsequent analysis. An average of 14.7% of trials per participant were rejected (each participant had at least 33 trials). Next, a baseline correction was implemented from -2200 – -2000 ms, and trials were averaged across the practice phase. Early-RP was quantified by centering a 50 ms time window around the peak negative amplitude between -2000 – -1500 ms at electrode C4, where the early-RP was most negative in the grand average waveform. This resulted in a time window of -1665 – -1615 ms, in which mean amplitude was derived for electrodes C3, Cz, and C4, and then averaged across these electrodes. Late-RP was quantified in the same way as the early-RP, except the peak was identified between -500 – 0 ms at electrode C3, where amplitude was maximal. This resulted in a time window of -65 – -15 ms.

Free recall. To measure declarative knowledge (free recall), the number of key concepts from the putting instruction booklet correctly recalled by participants was recorded. The key concepts are the most important concepts, as indicated by the expert golfer who provided the putting instructions: (1) “establish proper grip,” (2) “place the putter head behind the ball and take a hip-width stance," (3) "place the eyes directly over the ball by hinging from the hips," and (4) "stroke the ball without breaking the wrists."

IMI processing. To process the IMI, its scores were averaged within subscales: interest/enjoyment, value/usefulness, effort/importance, and pressure/tension.

Statistical Analysis

To assess learning, separate 2 (Group: teach/test) x 2 (Test: retention/transfer) ANCOVAs with pretest RE/BVE serving as the covariate were employed for RE and BVE. Based on Daou, Buchanan et al. (2016) and Daou, Lohse et al. (2016), we predicted a main effect of group for both RE and BVE. To assess practice performance, separate 2 (Group: teach/test) x 6 (Blocks 1 – 6) ANCOVAs with pretest RE/BVE serving as the covariate were employed for RE and BVE. Based on Daou, Buchanan et al. and Daou, Lohse et al, we predicted no significant differences between groups during practice. For these analyses and others, the Greenhouse-Geisser correction is applied when sphericity is violated.

For frontal midline theta**,** a 2 (Group: teach/test) x 2 (Phase: pretest/practice) x 4 (Epoch: -3 – -2, -2 – -1, -1 – 0, 0 – +1) ANOVA was employed. For motor upper-alpha, a 2 (Group: teach/test) x 2 (Phase: pretest/practice) x 4 (Epoch: -3 – -2, -2 – -1, -1 – 0, 0 – +1) ANOVA was employed. Following the rationale set forth in the Introduction, we predicted a Group x Phase interaction for frontal midline theta and motor upper-alpha, such that both variables would be higher for the teach group in the practice phase but not the pretest phase. To assess upper-alpha T7/T8-Fz coherence, a 2 (Group: teach/test) x 2 (Phase: pretest/practice) x 2 (Electrode Pair: T7-

Fz/T8-Fz) ANOVA was employed. Following the rationale set forth in the Introduction, we predicted a Group x Phase x Electrode Pair interaction, such that the teach group would exhibit greater coherence than the test group exclusively for T7-Fz and exclusively during practice. To assess the RP, a 2 (Group: teach/test) x 2 (Sub-Component: early/late) ANOVA was employed (since 30 trials are required to establish a reliable RP (Brunia et al., 2012), it was not computed for pretest, and thus there is no factor of phase). Based on Daou, Lohse et al. (2016), we predicted a Group x Sub-Component interaction, such that the teach group would exhibit greater RP amplitude exclusively for the late component.

To analyze motivation (IMI), the motivation subscale scores (interest/enjoyment, value/usefulness, effort/importance) were subjected to a MANOVA with group serving as the independent variable (Grand, Daou, Lohse, & Miller, 2017). Based on Daou, Buchanan et al. (2016) and Daou, Lohse et al. (2016), we predicted the MANOVA would yield a nonsignificant result. The pressure/tension subscale was subjected to an independent sample *t*-test (group) separate from the MANOVA, since pressure/tension is not a measure of motivation. Based on Daou, Buchanan et al. and Daou, Lohse et al., we predicted the *t*-test would yield a nonsignificant result. To assess the free recall**,** an independent sample *t*-test (group) was conducted for key concepts correctly recalled. Based on Daou, Buchanan et al. and Daou, Lohse et al., we predicted the teach group would recall significantly more concepts than the test group. To explore whether expecting to teach influenced participants’ likelihood of engaging in activities to prepare for day 2, we conducted chi-square tests with group serving as the independent variable and day 1 as well as day 2 preparation activities serving as the dependent variables.

Results

Descriptive Data

Table 1 shows descriptive data for each group.

Table 1. Descriptive data for each group.

Descriptive Data by Group	Test (*n* = 30; 15 females)		Teach (*n* = 29; 13 females)	
	M	95% *CI*	*M*	95% *CI*
Age (Years)	20.9	20.3 – 21.5	21.2	20.7 – 21.7
Lifetime Putting Experience[a]	1.13	0.82 – 1.43	1.48	1.09 – 1.87
Past-Year Putting Experience[a]	0.33	0.15 – 0.51	0.48	0.26 – 0.70
Pretest RE	57.2	48.0 – 66.4	50.7	43.7 – 57.7
Pretest BVE	64.3	57.2 – 71.4	61.8	52.1 – 71.5
Intrinsic Motivation	6.08	5.82 – 6.34	6.06	5.77 – 6.34
Internalized Motivation	6.13	5.80 – 6.46	5.91	5.60 – 6.21
General Motivation	5.68	5.34 – 6.02	5.85	5.51 – 6.18
Pressure	2.52	2.18 – 2.85	2.39	2.02 – 2.76
Free Recall	2.13	1.76 – 2.58	3.17	2.86 – 3.47
Preparation Activities Day 1	0.133	0.004 – 0.262	0.310	0.131 – 0.489
Preparation Activities Day 2	0.200	0.048 – 0.352	0.276	0.103 – 0.449
Early RP	0.776	-0.525 – 2.08	0.318	-1.88 – 2.52
Late RP	0.361	-0.149 – 0.871	-0.673	-1.58 – 0.237

[a] 0 = Never putted; 1= Putted 1 – 10 times; 2 = Putted 11 – 20 times; 3 = Putted 21 – 30 times

Performance and Motor Learning

Figure 2A shows accuracy (RE) for the groups across all phases of the study. ANCOVA for practice RE revealed no main effect for group ($F(1, 56) = .471$, $p = .495$, $\eta^2_p = .008$), block ($F(3.70, 207) = 1.57$, $p = .188$, $\eta^2_p = .027$, $\varepsilon = .740$)[2], or the Group x Block interaction ($F(5, 280) = .476$, $p = .794$, $\eta^2_p = .008$), controlling for pretest RE. In addition, Figure 2B shows precision (BVE) for the groups across all phases of the study. ANCOVA revealed no main effect for group ($F(1, 56) = 1.30$, $p = .258$, $\eta^2_p = .023$), block ($F(5, 280) = .554$, $p = .735$, $\eta^2_p = .010$) or Group x Block interaction ($F(5, 280) = 1.51$, $p = .186$, $\eta^2_p = .026$), controlling for pretest BVE.

For motor learning, ANCOVA revealed a main effect of group for posttest RE ($F(1, 56) = 4.96$, $p = .030$, $\eta^2_p = .081$), controlling for pretest RE[3]. Specifically, the teach group exhibited lower RE ($M_{\text{adjusted}} = 48.1$, $CI = 43.6 – 52.5$ cm) than the test group ($M_{\text{adjusted}} = 55.0$, $CI = 50.7 – 59.4$ cm) indicating that expecting to teach enhanced accuracy. No main effect for test ($F(1, 56) = 2.64$, $p = .110$, $\eta^2_p = .045$), and no Group x Posttest interaction ($F(1, 56) = .963$, $p = .331$, $\eta^2_p = .017$) was observed, controlling for pretest RE.

In addition, ANCOVA revealed a main effect of group for posttest BVE ($F(1, 56) = 5.78$, $p = .02$, $\eta^2_p = .094$), controlling for pretest BVE. Specifically, the teach group exhibited lower

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[2] Despite the null result for block, participants improved on Day 1. However, most of their improvement occurred from pretest to block 1, which reflects a typical power law of practice (Crossman, 1959). Indeed, if pretest is considered a ‘block’ in Day 1, the effect of block is significant for RE and BVE (RE: $F(4.49, 256) = 23.5$, $p < .001$, $\eta^2_p = .292$, $\varepsilon = .748$; BVE: $F(6, 342) = 25.4$, $p < .001$, $\eta^2_p = .308$).

[3] We removed one participant in the teach group from all analyses because he was a univariate outlier for RE at pretest (*z*-score = 4.40 *SD*s above *M*) and was an influential data point in the ANCOVA for posttest RE (Cook’s distance = 2.21). Including this participant in analyses does not change results in terms of statistical significance.

BVE (M_{adjusted} = 54.8, *CI* = 49.2 – 60.4 cm) than the test group (M_{adjusted} = 64.2, *CI* = 58.7 – 69.7 cm) indicating that expecting to teach enhanced precision. No main effect for test (*F*(1, 56) = .791, *p* = .378, η^2_p = .014), and no Group x Posttest interaction (*F*(1, 56) = 1.02, *p* = .316, η^2_p = .018) was observed, controlling for pretest BVE.

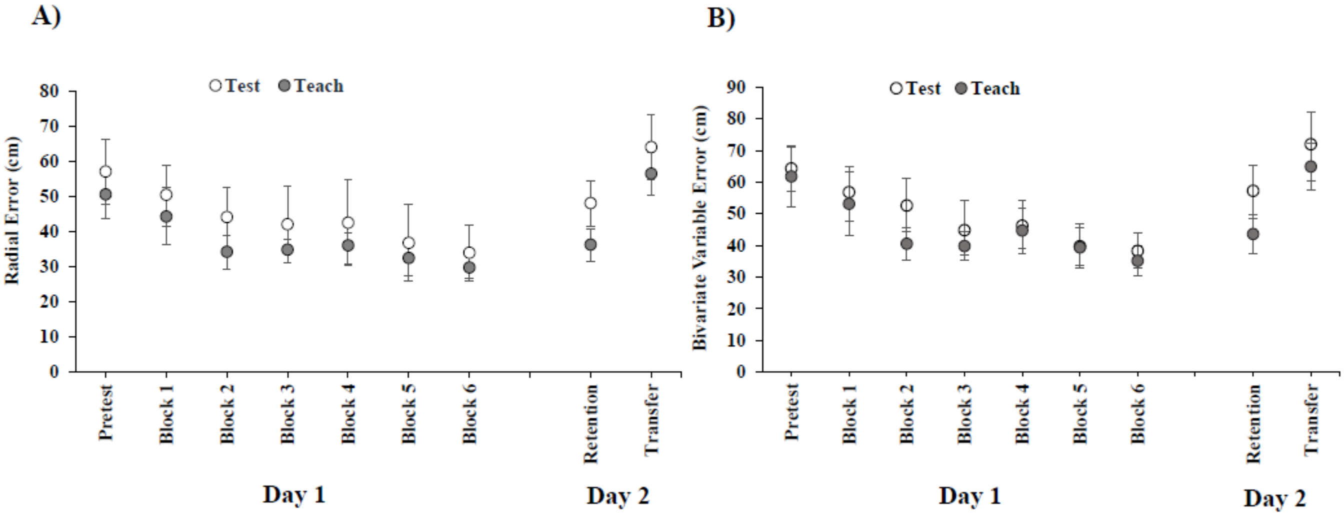

Fig. 2. A) Putting accuracy (lower RE indicates greater accuracy) as a function of study phase and group. Motor learning, as indexed by retention and transfer test accuracy, is superior for the teach group relative to the test group. B) Putting precision (lower BVE indicates greater precision) as a function of study phase and group. Motor learning, as indexed by retention and transfer test precision, is superior for the teach group relative to the test group. All error bars represent 95% *CI*s.

Motivation and Pressure

MANOVA of the motivation measures revealed that groups did not differ with respect to intrinsic, internalized, or general motivation (*F*(3, 55) = .768, *p* = .517, Wilk's Λ = .960, η^2_p =

.040). *T*-test of pressure/tension revealed a nonsignificant difference ($t(57) = .521$, $p = .605$, $d = 0.139$).

Free Recall

T-test revealed participants in the teach group recalled significantly more rules than participants in the test group, see Table 1 ($t(57)= 4.12$, $p < .001$, $d = 1.09$).

Preparation for Day 2 Activities

Chi-square revealed group assignment did not influence participants' likelihood of engaging in day 2 preparation activities either on day 1 (X^2 (1, $N = 59$) = 2.69, $p = .101$) or day 2 (X^2 (1, $N = 59$) = 0.469, $p = .493$).

EEG Results

Frontal midline theta. ANOVA revealed a main effect of phase ($F(1, 53) = 22.9$, $p < .001$, , $\eta^2_p = .301$), represented by greater frontal midline theta during practice ($M = 0.381$, CI = 0.209 - 0.552) in comparison to pretest ($M = -0.147$, CI = -0.347 - 0.054). Additionally, a main effect of epoch was observed ($F(3, 159) = 9.74$, $p < .001$, $\eta^2_p = .155$). This effect was driven by a significant linear effect ($F(1, 53) = 22.8$, $p < .001$, $\eta^2_p = .301$), such that frontal midline theta progressively decreased preceding putt initiation. On the other hand, results revealed no main effect of group ($F(1, 53) = 0.293$, $p = .590$, $\eta^2_p = .006$), no Group x Phase interaction ($F(1, 53) = 1.06$, $p = .308$, $\eta^2_p = .020$), no Group x Epoch interaction ($F(3, 159) = 1.46$, $p = .228$, $\eta^2_p = .027$), no Phase x Epoch interaction ($F(3, 159) = 0.469$, $p = .704$, $\eta^2_p = .009$), and no Group x Phase x Epoch interaction ($F(3, 159) = 0.549$, $p = .650$, $\eta^2_p = .010$) (see Figure 3).

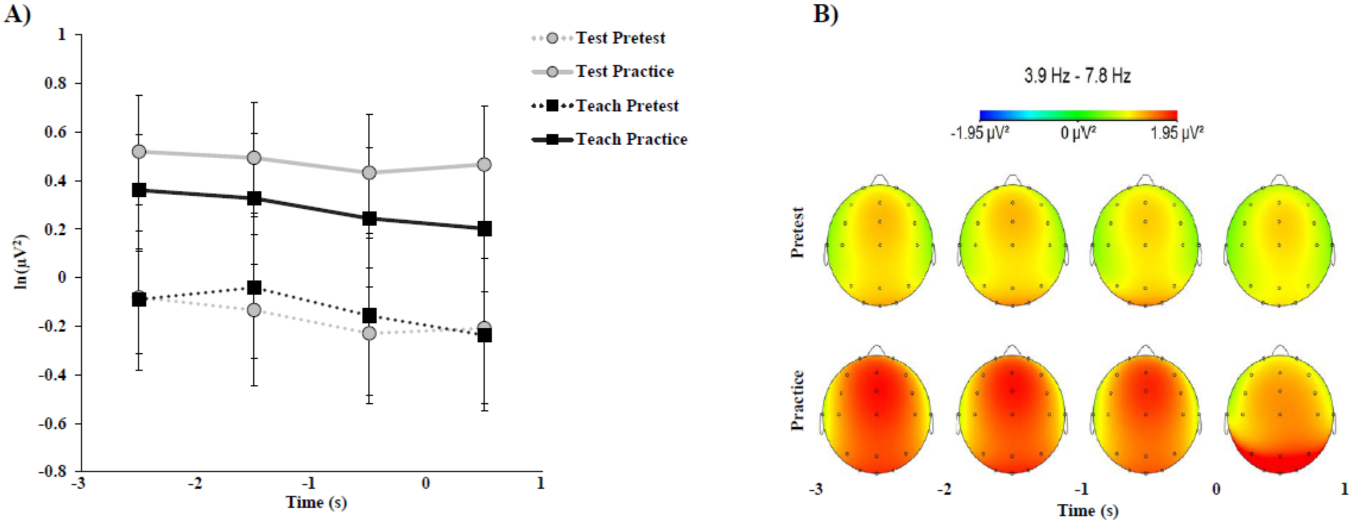

Fig. 3. A) Frontal midline theta as a function of group, phase, and epoch. There was a main effect of phase on frontal midline theta represented by greater power during practice relative to pretest, and a main effect of epoch represented by a decrease in power preceding putt initiation. There was no main effect or interaction involving group. B) Frontal midline theta topographies as a function of phase and epoch, averaged across groups.

Motor upper-alpha. ANOVA revealed a main effect of phase ($F(1, 53) = 56.3$, $p < .001$, $\eta^2_p = .515$), represented by greater motor upper-alpha during practice ($M = -0.314$, $CI = -0.472$ - -0.156), in comparison with pretest ($M = -0.960$, $CI = -1.12$ - -0.798). Additionally, a main effect of epoch ($F(2.63, 139) = 6.82$, $p < .001$, $\eta^2_p = .114$, $\varepsilon = .875$). This effect was driven by a significant linear effect ($F(1, 53) = 15.7$, $p < .001$, $\eta^2_p = .229$), such that motor upper-alpha progressively decreased preceding putt initiation. However, results revealed no main effect of group ($F(1, 53) = 0.181$, $p = .672$, $\eta^2_p = .003$), no Group x Phase interaction ($F(1, 53) = 0.166$, p

= .685, η^2_p = .003), no Group x Epoch interaction (*F*(3, 159) = 0.730, *p* = .536, η^2_p = .014), no Phase x Epoch interaction (*F*(3, 159) = 0.786, *p* = .503, η^2_p = .015), and a nonsignificant Group x Phase x Epoch interaction (*F*(3, 159) = 0.713, *p* = .545, η^2_p = .013) (See Figure 4).

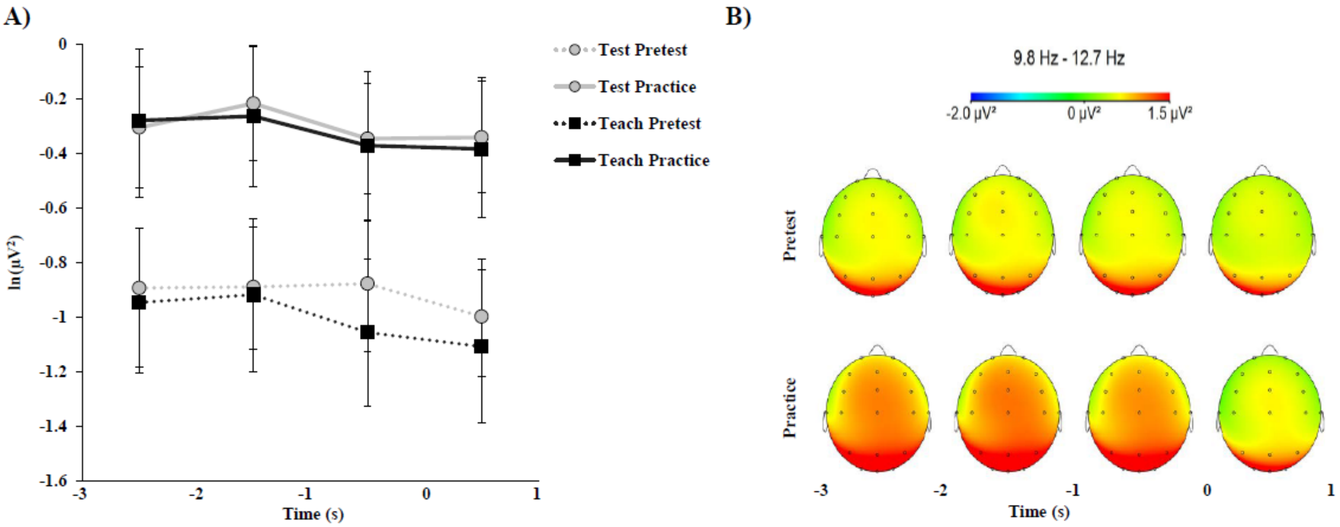

Fig. 4. A) Motor upper-alpha as a function of group, phase, and epoch. There was a main effect of phase on motor upper-alpha represented by greater power during practice relative to pretest, and a main effect of epoch represented by a decrease in power preceding putt initiation. There was no main effect or interaction involving group. B) Motor upper-alpha topographies as a function of phase and epoch, averaged across groups.

T7-Fz and T8-Fz coherence. ANOVA revealed a main effect of phase (*F*(1, 53) = 30.9, $p <$ .001, η^2_p = .368), represented by higher coherence during practice (*M*= 0.169, *CI* = 0.133 - 0.206) than pretest (*M* = 0.284, *CI* = 0.247 - 0.320). Additionally, a main effect for Electrode Pair was found (*F*(1, 53) = 11.1, *p* = .002, η^2_p = .173), represented by a higher coherence for T8-Fz (*M* = 0.257, *CI* = 0.220 - 0.295), in comparison with T7-Fz (*M* = 0.195, *CI* = 0.162 - 0.229). On the other hand, results revealed no main effect of group ($F(1, 53) = 0.212$, $p = .647$, $\eta^2_p = .004$), of epoch ($F(3, 159) = 1.8$, $p = .150$, $\eta^2_{p\,=}\ .033$), of Group x Phase interaction ($F(1, 53) = 1.74$, $p = .193$, $\eta^2_p = .032$), of Group x Epoch interaction ($F(3, 159) = 0.401$, $p = .752$, $\eta^2_p = .008$), of Group x Electrode Pair interaction ($F(1, 53) = 2.98$, $p = .090$, $\eta^2_p = .053$), of Phase x Epoch interaction ($F(3, 159) = 1.37$, $p = .253$, $\eta^2_p = .025$), of Phase x Electrode pair interaction ($F(1, 53) = 1.25$, $p = .269$, $\eta^2_p = .023$), of Epoch x Electrode Pair interaction ($F(3, 159) = 2.39$, $p = .071$, $\eta^2_p = .043$), of Phase x Epoch x Group interaction ($F(3, 159) = 0.561$, $p = .641$, $\eta^2_p = .010$), of Phase x Electrode Pair x Group interaction ($F(1, 53) = 2.77$, $p = .102$, $\eta^2_p = .050$), of Epoch x Electrode Pair x Group interaction ($F(3, 159) = 2.36$, $p = .074$, $\eta^2_p = .043$), of Phase x Epoch x Electrode Pair interaction ($F(3, 159) = 1.89$, $p = .133$, $\eta^2_p = .034$), and of Phase x Epoch x Electrode Pair x Group interaction ($F(3, 159) = 0.281$, $p = .839$, $\eta^2_p = .005$) (See Figure 5).

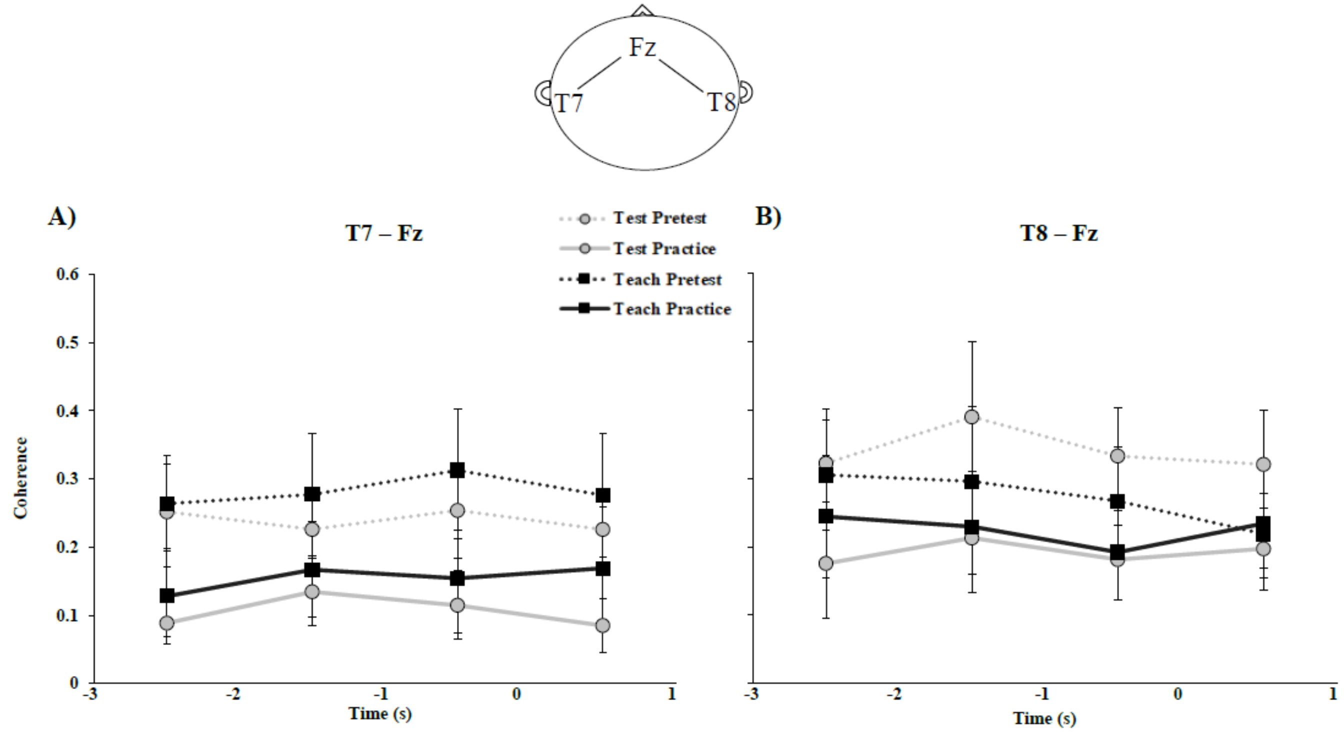

Fig. 5. A) T7-Fz coherence as a function of group, phase, and epoch. There was a main effect of phase represented by greater coherence during pretest relative to practice. There was no main effect or interaction involving group. B) T8-Fz coherence as a function of group, phase, and epoch. There was a main effect of phase represented by greater coherence during pretest relative to practice. There was no main effect or interaction involving group. Notably, a main effect of electrode pair was found such that T8-Fz coherence was higher than T7-Fz coherence.

Readiness potential. Results revealed a main effect of subcomponent (*F*(1, 53) = 21.2, $p > .001$, $\eta^2_p = .286$), with late RP being more negative than early RP. However, no main effect of group (*F*(1, 53) = 0.594, $p = .444$, $\eta^2_p = .011$) or Group x Subcomponent interaction (*F*(1, 53) = 0.832, $p = .366$, $\eta^2_p = .015$) was found (see Figure 6).

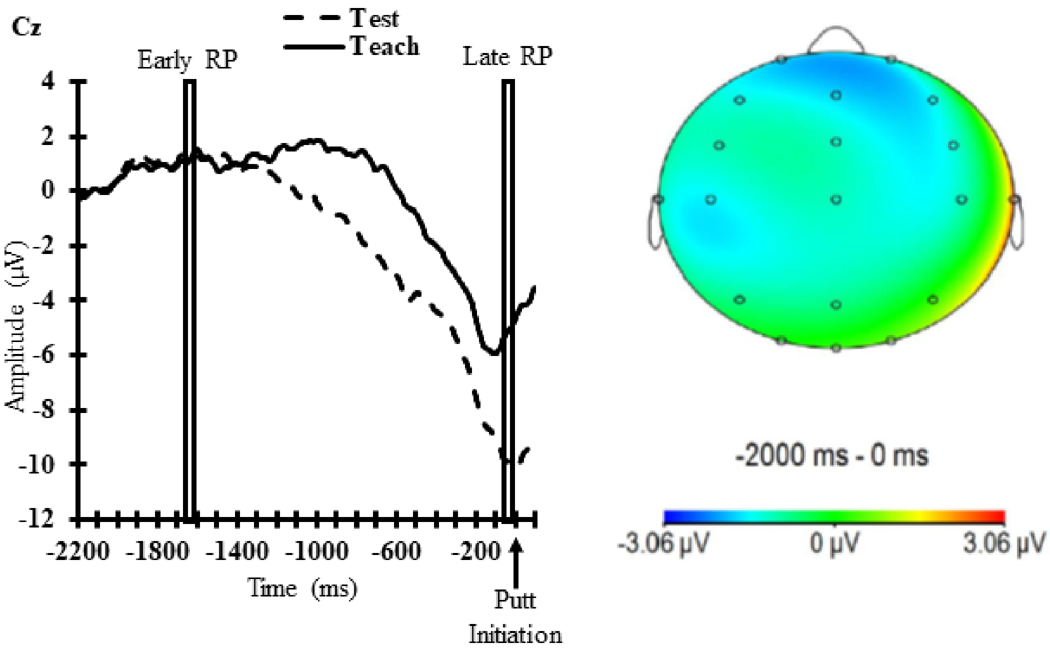

Fig. 6. A) RP as a function of group and subcomponent. There was no main effect or interaction involving group. B) Scalp topography of early RP averaged across groups.

Discussion

Results reveal expecting to teach enhanced motor learning (posttest accuracy and precision), but not practice performance, replicating previous findings (Daou, Buchanan, et al., 2016; Daou, Lohse et al., 2016). Motor learning enhancement was accompanied by superior free recall of key concepts, indicative of greater declarative knowledge, as found in previous research (Bargh & Schul, 1980; Benware & Deci, 1984; Daou, Buchanan et al. 2016; Daou, Lohse et al., 2016; Nestojko et al., 2014). Taken together, these results give us confidence that the expecting to teach effect represents a real phenomenon in motor learning, for both procedural knowledge (motor memory about the skill) and declarative knowledge (verbalized rules and techniques about the skill).

Turning to potential mechanisms explaining the expecting to teach effect, results indicated expecting to teach does not influence one's likelihood of engaging in post-practice preparatory activities for teaching/testing. Similarly, results related to social-affective mechanisms, specifically motivation and pressure, were not affected by expecting to teach, as found in previous research (Daou, Buchanan, et al., 2016; Daou, Lohse et al., 2016). Likewise, results revealed expecting to teach did not affect motor preparatory processing, as indexed with EEG.

However, several interesting effects of experimental phase and motor preparatory epoch emerged from the EEG analysis. Specifically, frontal midline theta results revealed a main effect of phase represented by greater theta during practice relative to pretest, and a main effect of epoch represented by a decrease in theta preceding putt initiation. In the context of motor preparation, frontal midline theta is related to working memory and action monitoring (Doppelmayr et al., 2008; Dyke et al., 2014; Gevins et al., 1997; Kao et al., 2013; Weber & Doppelmayer, 2016). In particular, frontal midline theta during motor preparation should be greater if participants are holding more information in mind and monitoring their action to a greater extent. It follows then that the main effect of phase on frontal midline theta could be explained by participants attempting to hold information from the golf instruction booklet in mind during practice, but not during pretest, since they had not yet studied the booklet and had minimal prior information about putting. Similarly, participants may have been monitoring their actions while preparing to putt and initiating their putts during practice more so than during pretest, because participants may have only been aware of the proper actions to monitor during practice, after exposure to the instruction booklet. The main effect of epoch, whereby frontal midline theta exhibited a linear decrease preceding putt initiation, also has a reasonable explanation. Specifically, participants held putting information in working memory as they planned their movement, and then gradually released the information as they got closer to initiating their putt, after which the putting information could not be used. Additionally, participants may have been monitoring their actions while preparing to putt (e.g., establishing a proper stance and grip on the club), but they may have stopped monitoring their actions as they got closer to initiating their putt, since little action adjustments can be made once the swing has begun.

As with frontal midline theta, motor upper-alpha results revealed a main effect of phase represented by greater upper-alpha during practice relative to pretest, and a main effect of epoch represented by a linear decrease in upper-alpha preceding putt initiation. Motor upper-alpha is inversely related to the degree of cortical resource allocation to accurate motor programing (Babiloni et al., 2008; Cooke et al., 2014; Cooke et al., 2015). It follows then that the elevated motor upper-alpha during practice concomitant with improved accuracy and precision relative to pretest may represent increased ‘psychomotor efficiency’ (Gallicchio, Cooke, & Ring, 2017; Gentili, Bradberry, Oh, Hatfield, & Contreras-Vidal, 2011; Kerick, Douglass, & Hatfield, 2004). That is, participants were able to achieve superior performance with fewer cortical resources as they gained more experience with the skill. The main effect of epoch whereby motor upper-alpha exhibited a linear decrease preceding putt initiation also has a reasonable explanation. This effect indicates that participants were progressively allocating cortical resources to motor programing as they prepared to execute the motor program (i.e., initiate the putt). Notably, this result is similar to those reported in the extant literature (Babiloni et al., 2008; Cooke et al., 2014; Cooke et al., 2015; Gallicchio et al., 2017).

Upper-alpha T7/T8-Fz coherence results revealed a main effect of phase, represented by greater coherence during pretest relative to practice, and a main effect of electrode pair, characterized by greater T8-Fz coherence than T7-Fz coherence. T7-Fz coherence shows the communication between left temporal lobe and premotor cortex, and is sensitive to verbal-analytic information being translated into motor planning (Buszard et al., 2016; Cheng et al., 2017; Deeny et al., 2003; Deeny et al., 2009; Hatfield et al., 2013; Gallicchio et al., 2016; Gallicchio et al., 2017; Gentili et al., 2015; Rietschel et al., 2012; Zhu et al., 2010; Zhu, Poolton, Wilson, Hu et al., 2011; Zhu, Poolton, Wilson, Maxwell et al., 2011). On the other hand, T8-Fz coherence reflects the communication between right temporal lobe and premotor cortex, indicating visuospatial information being translated into motor planning. It follows then that the reduced T7-Fz and T8-Fz coherence during practice concomitant with improved accuracy and precision relative to pretest may represent increased ‘psychomotor efficiency’, similar to the modulations in motor upper-alpha (Cheng et al., 2017; Deeny et al., 2003; Deeny et al., 2009; Hatfield et al., 2013; Gallicchio et al., 2016; Gallicchio et al., 2017; Gentili et al., 2015; Rietschel et al., 2012; Zhu, Poolton, Wilson, Hu et al., 2011; Zhu, Poolton, Wilson, Maxwell et al., 2011). That is, participants were able to achieve superior performance with less verbal-analytic and visuospatial involvement in motor planning as they gained more experience with the skill. The result that coherence was higher between T8 and Fz relative to T7 and Fz also is reasonable, considering that participants were performing a task relying more heavily on the visuospatial processes represented by T8-Fz coherence than the verbal-analytic processes reflected by T7-Fz coherence.

Daou, Lohse et al. (2016) suggested expecting to teach may modulate motor preparatory processing during practice. Thus, the present experiment investigated whether expecting to teach enhanced specific motor preparatory processes, as indexed by EEG. Results indicate expecting to teach does not affect motor preparatory processes during the 3-s prior to movement execution in practice. Nonetheless, the present experiment replicated Daou, Buchanan et al. (2016) and Daou, Lohse et al. (2016)'s result that expecting to teach does indeed enhance motor learning, and that the expecting to teach effect is not related to social-affective mechanisms, such as motivation and pressure. This begs the question of what could explain the expecting to teach effect. One possibility is that expecting to teach may not affect cerebral cortical motor preparatory processes recordable by EEG, but rather sub-cortical processes that are not captured by EEG. For example, expecting to teach may increase a learner's interest in skill acquisition, which could increase connectivity between midbrain and hippocampal regions (Gruber, Gelman, & Ranganath, 2014), which are implicated in motor learning (Doyon et al., 2009). To investigate this possibility, future research could adapt the expecting to teach paradigm for functional magnetic resonance imaging, thus avoiding the limitation of EEG's inability to examine sub-cortical regions that could mediate the relationship between expecting to teach and motor learning. Alternatively, the means by which EEG data is quantified (averaging across trials) could have masked expecting to teach effects at the level of cerebral cortex. However, analyzing single trials of a task involving as much movement as putting is difficult due to the potential for noise induced by movement artifact.

Another possibility is that expecting to teach does not affect the quality of motor preparatory processing, but rather the duration of the processing. For example, expecting to teach may cause learners to spend more time holding information in working memory, allocating cortical resources to motor programming, translating verbal-analytic information into motor planning, and specifying movement parameters, without actually increasing the magnitude of these processes during the 3-s preceding movement initiation. As such, expecting to teach would increase the duration of preparatory processing (as observed by Daou, Lohse et al., 2016), but not the values of the EEG variables reflecting preparatory processes during the final 3-s preceding movement initiation (as observed in the present experiment). An alternative approach to investigate motor preparatory processing without limiting analysis to the 3-s preceding movement initiation would be to begin each practice trial with a cue, and examine EEG time-locked to the cue. This approach could shed light upon motor preparatory processes beginning prior to the 3-s preceding movement initiation and possibly elucidate both the duration and magnitude of motor preparatory processing. For example, teach participants may demonstrate elevated cortical activity immediately following the cue, whereas test participants may not exhibit such activity until shortly (~3-s) prior to movement initiation. Further, teach participants could exhibit increased processing of the cue (e.g., higher amplitude of the P3a event-related potential component), suggesting greater allocation of attentional resources to the task (Frömer, Stürmer, & Sommer, 2016a).

Finally, expecting to teach may affect neurocognitive processes linked to motor learning, but not occurring during motor preparation. For instance, expecting to teach may affect feedback processing. This follows because learners may process their errors to a greater extent if they expect to have to teach another person how to correct that person's errors. Thus, future studies may examine feedback processing in the expecting to teach paradigm by recording learners' event-related potentials time-locked to intrinsic or augmented feedback (Frömer, Stürmer, Sommer, 2016b; Grand et al., 2015; Grand et al., 2017; Joch, Hegele, Maurer, Müller, & Maurer, 2017a; Joch, Hegele, Maurer, Müller, & Maurer, 2017b; Maurer, Maurer, & Müller, 2015). Such investigations would overcome the present experiment's limited focus on motor preparation.

In conclusion, expecting to teach appears to enhance motor learning, but the mechanisms underlying this effect remain elusive. The present experiment's inability to shed light upon the mechanisms could be due to several study limitations. These limitations include the use of EEG, which does not capture sub-cortical processes that could mediate the expecting to teach effect, and the averaging across EEG trials, which could have masked expecting to teach effects. An additional limitation is the focus on motor preparatory processing time-locked to movement onset, which did not allow examination of cue-locked motor preparatory processing (occurring more than 3-s prior to movement onset) nor feedback processing, both of which could mediate the expecting to teach effect. While future research may address the limitations of the present experiment in order to elucidate the mechanisms of the expecting to teach effect, it is important to reiterate that the current results provide further evidence for the effect, which has implications for practitioners in the field of motor learning (e.g., sport coaches and physical therapists). Further, the present experiment revealed effects of experimental phase and motor preparatory epoch on EEG activity, thus shedding light upon learners' cerebral cortical dynamics as they perform motor skills, and how these dynamics change from initial performance to a practice session.

Chapter 4: Does limiting pre-movement time during practice eliminate the benefit of practicing while expecting to teach?

Introduction

Determining practical ways to enhance motor learning is critical to improve motor behavior. Past research has shown that practicing and studying a motor skill with the expectation of teaching it to another person enhances skill learning relative to practicing and studying with the expectation of being tested (Daou, Buchanan, Lindsey, Lohse, & Miller, 2016; Daou, Lohse, & Miller, 2016; Daou, Lohse, & Miller, 2018). Specifically, participants expecting to teach have demonstrated superior skill accuracy and precision, as well as, declarative knowledge about the skill, when assessed during posttests 1-day and 7-days after practice. Initially, researchers believed the benefit of expecting to teach could be attributed to heightened motivation and/or pressure during practice (Daou, Buchanan et al., 2016). In particular, it was reasoned that expecting to teach should cause a learner to recognize their own learning may affect another person's learning, thereby increasing their drive and pressure to learn. Elevated motivation and pressure (stress) while practicing and studying, in turn, could yield psychological and physiological states conducive for learning (e.g., increased dopaminergic activity [Arnsten, 2009; Wulf & Lewthwaite, 2016]); and/or prompt learners to engage in additional practice trials or study time, consequently, enhancing learning. However, neither motivation nor pressure have been demonstrated to increase as a function of expecting to teach (Daou, Buchanan et al., 2016; Daou, Lohse et al., 2016; Daou, Lohse et al., 2018).[4]

Beyond motivation and pressure, Daou, Lohse et al. (2016) examined whether enhanced information processing during practice and studying could explain the benefit of expecting to teach. The authors reasoned that expecting to teach may cause a learner to process more information about the skill they are practicing and studying, knowing that they will have to communicate this information to another person, and that greater information processing should improve learning (Cross, Schmitt, & Grafton, 2007). The authors used the amount of time spent in motor preparation before each practice trial (pre-movement time) as a proxy for information processing. Results revealed expecting to teach lengthened pre-movement time and predicted posttest accuracy and precision. However, pre-movement time did not predict posttest accuracy and precision when controlling for whether participants expected to teach, thus casting doubt on whether increased motor preparation explains the expecting to teach effect or merely coincides with it. In a follow-up experiment, Daou, Lohse et al. (2018) used electroencephalography (EEG) to examine cerebral cortical dynamics during the final 3 s of motor preparation before each practice trial and did not observe any effects of expecting to teach. The authors concluded the benefit of expecting to teach might be explained by the duration of motor preparation, but not the cortical dynamics during preparation.

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[4] As for additional practice trials or study time, Daou, Buchanan et al. (2016) did not observe an effect of expecting to teach, and Daou, Lohse et al. (2016) only observed expecting to teach increased study time. In both experiments, the number of putts and study time failed to predict posttest accuracy or precision.

The present experiment aimed to test whether motor preparation does indeed explain the advantage of expecting to teach. Specifically, we had participants practice golf putting with the expectation of teaching the skill to another participant the following day or the expectation of being tested on the skill the following day. We limited the motor preparation time for half of the participants who expected to teach and half of the participants who expected to test, and allowed the remaining participants to take as much motor preparation time as they deemed necessary. All participants were tested on their putting the next day. We predicted main effects of expectation and motor preparation on posttest accuracy and precision, such that, participants who had the expectation of teaching and participants who had unlimited motor preparation would exhibit superior accuracy and precision. However, we hypothesized these main effects would be superseded by an expectation by motor preparation interaction revealing that the benefit of expecting to teach on posttest accuracy and precision would be exclusive to those participants who also practiced with unlimited motor preparation.

Methods

Prior to beginning data collection, the experimental design and analyses were registered and made public on AsPredicted.org (https://aspredicted.org/dt4gj.pdf).

Participants

Eighty right-handed young adults (54 females, $M_{\text{age}} = 21.8$, $SD = 2.52$ years; see Table 1 for descriptive data) completed the experiment after providing informed written consent to a research protocol (Protocol #14-534 EP 1412) approved by Auburn University's Institutional Review Board. Participants were recruited from university courses and by word-of-mouth. They were compensated with course credit and/or entry into a raffle for a monetary award. Sample size was determined with an a priori power calculation using G*Power 3.1 (Faul, Erdfelder, Lang, & Buchner, 2007). The power calculation sought 80% power ($\alpha \leq .05$) to detect moderate-sized effects ($f^2 = .15$) of expectation, motor preparation, and an Expectation x Motor Preparation interaction on posttest accuracy/precision in a multiple linear regression model. The model included three other predictors: pretest accuracy/precision, number of putts taken during practice, and amount of time spent studying during practice.

Task

Participants used a standard, right-handed golf putter to putt a standard golf ball on an artificial grass surface to a target cross (+) comprised of two 10.8 cm pieces of white masking tape located 120 cm from a starting position, which was indicated by a 5 cm piece of white masking tape next to participants. The objective was to have the ball stop as close to the center of the target as possible.

Procedure

All participants completed the experiment individually. After consenting to the experiment, they completed a demographic questionnaire asking their age, sex, and putting experience (anything from miniature golf to playing 18 holes on a standard golf course) over their lifetime and within the past year.

Pretest. After completing the demographic questionnaire, participants were asked to perform the pretest phase. Pretest consisted of one block of ten putts. As the purpose of this phase was to determine participants' baseline skill level, we attempted to isolate performance and minimize on-line learning by preventing participants from using feedback about the outcome of their putts. To this end, participants wore a blindfold and earplugs while putting (Dyke et al., 2014). Participants were permitted to view the ball and the target after the experimenter reset the ball in the starting position. Once the participant placed the putter behind the ball, they pulled down their blindfold before putting. Further, the experimenter waited 45 s after each putt to reset the ball in the starting position. In so doing, participants could not determine their accuracy by how quickly the experimenter reset the ball. (High accuracy could be inferred from a quick reset, since the experimenter manually measured the distance of the ball to the target before manually resetting the ball in the starting position, making measurement quicker for putts closer to the target.)

Practice. After pretest, participants were read instructions according to the groups to which they were randomly assigned. Participants in the teach groups (*n* = 39) were told “tomorrow you will teach another participant how to putt,” and participants in the test groups (*n* = 41) were told “tomorrow you will be tested on your putting.” Next, participants were told they had 1 hour to learn how to putt, and they had to remain in the experiment laboratory for the duration of the hour. To initiate the practice phase, participants studied a golf putting instruction booklet for at least 2 min; however, it was emphasized that they could take as long as they liked. The instructions for the booklet were provided by an expert golfer (for booklet, see Daou, Buchanan et al. [2016]). After studying the booklet, participants were told they would be performing at least five blocks of ten putts, but they could take additional putts if they wanted. Between each block, participants were told they had a 1 min break during which they could study the booklet if they wanted. Participants were cut off from taking additional putts (and study time) after their tenth block (if they chose to complete a tenth block).

During the practice phase, 20 of the participants in the teach group and 20 of the participants in the test group had unlimited motor preparation time before each putt (the time from when the experimenter reset the ball until the participant began their next putt), yielding a teach unlimited and test unlimited group. The remaining participants had their motor preparation time limited during practice, yielding a teach limited (*n* = 19) and test limited (*n* = 21) group. Specifically, the experimenter told participants in these groups:

> “After I put the ball on the ground, at the starting position, you will only have a few seconds to putt before a tone is played. If you don’t begin your backswing before the tone, then I will pick up the ball, and you will start the trial over again.

> So you must putt the ball before the tone is played. Your goal is to make the ball stop as close to the target as you can."

On each trial of the practice phase, the experimenter placed the ball at the starting position and asked participants to affirm being ready to putt. Upon participant affirmation, the experimenter pressed a button on a keyboard for a computer located ~150 cm behind participants, which elicited a tone (750 Hz and 90 dB) from speakers adjacent to the computer at a random interval 4 – 5 s after the keypress. The 4 – 5 s motor preparation time limit was based on Daou, Lohse et al. (2016), who observed 27 out of 28 participants in the teach group took at least 5 s to putt. Thus, imposing this motor preparation time limit should have caused teach participants to take less time preparing their movements than they otherwise would. Pre-movement time was recorded (30 frames/s) with a video camera (Logitech C930e, Logitech International, Newark, CA) focused on the starting position during odd-numbered blocks for all participants, but we were only interested in the preparation time for the teach/test unlimited groups, which we expected to significantly differ as in Daou, Lohse et al.

Immediately after participants reported that they were done putting or completed 100 putts, they filled out the Intrinsic Motivation Inventory (IMI) (McAuley, Duncan, & Tammen, 1989) to assess whether expectation and/or preparation time influenced motivation and pressure. The subscales of interest were as follows: interest/enjoyment, value/usefulness, effort/importance, and pressure/tension. Examples of each subscale of interest are as follows: interest/enjoyment item, "I enjoyed doing this activity very much"; value/usefulness item, "I think this is an important activity"; effort/importance item, "I put a lot of effort into this activity”; pressure/tension item, “I was anxious while working on this activity”. The questionnaire was scored on a seven-point Likert scale with “not true at all” and “very true” as the anchors. (The perceived competence and perceived choice subscales were also collected, but they were not analyzed, as these subscales are merely correlates of motivation, and it is motivation that is more directly linked to motor learning [Wulf & Lewthwaite, 2016]).

Posttests (retention and transfer). Approximately 24 hours after completing Day 1, participants returned to complete the experiment. Participants in the teach groups were told, “the participant who you were going to teach did not show up today, so you will actually be tested on your putting instead.” Next, all participants completed the retention test and transfer test in a counterbalanced order. For the retention test, participants putted to the same target as during pretest and practice, whereas for the transfer test they putted to a target located 170 cm away. As with the pretest, we sought to minimize online learning during the posttest, so participants were blindfolded and ear-plugged and the amount of time between putts was fixed at 45 s. After the retention and transfer tests, we assessed participants’ declarative knowledge by asking them to complete a free recall test wherein they were asked to “…report, in as much detail as possible, any rules, methods, or techniques [they recalled] using to execute putts during practice on the first day of the study” (Daou, Buchanan et al., 2016; Daou, Lohse et al., 2016; Daou, Lohse et al., 2018). Finally, participants were debriefed about the purpose of the experiment, and teach participants were told that they were deceived into expecting to teach another participant and asked if they still wanted to have their data included in the experiment. (They all did.)

Data Processing

Performance and learning. Putting accuracy and precision were measured because they are separate and critical aspects of motor learning (Fischman, 2015). Specifically, accuracy was indexed by recording radial error as recommended by Hancock, Butler, and Fischman (1995): $Radial\ Error\ (RE) = (x^2 + y^2)^{1/2}$, where x and y represent the magnitude of error along the respective axes (i.e., how far away from the target cross the ball stopped in the horizontal and vertical directions). Precision was indexed by recording bivariate variable error as recommended by Hancock et al.: $Bivariate\ Variable\ Error\ (BVE) = \{(\frac{1}{k}) \sum_{i=1}^{k} [(x_i - x_c)^2 + (y_i - y_c)^2]\}^{1/2}$, where *k* = trials in a block and $_c$ = centroid along the given axis (x or y) for that block. RE and BVE were calculated for pretest to assess baseline skill level, odd-numbered blocks of the practice phase for a glimpse into practice performance without overly slowing data collection, and retention and transfer tests to measure learning.

Pre-movement time. The duration from when the experimenter placed the ball in the starting position to when the participant began the putting movement was determined via frame-by-frame analysis of the video recorded during the first, third, and fifth acquisition blocks. The first putt of each of these blocks was excluded from analysis, because participants were often reorienting themselves to the task, asking the experimenter a question, etc. (Daou, Lohse et al., 2016).

Free recall. To measure declarative knowledge (free recall), the number of key concepts from the putting instruction booklet correctly recalled by participants was recorded. The key concepts are the most important concepts, as indicated by the expert golfer who provided the putting instructions: (1) “establish proper grip,” (2) “place the putter head behind the ball and take a hip-width stance,” (3) “place the eyes directly over the ball by hinging from the hips,” and (4) “stroke the ball without breaking the wrists” (Daou, Buchanan et al., 2016; Daou, Lohse et al., 2016; Daou, Lohse et al., 2018).

IMI processing. IMI subscales exhibited good reliability: interest/enjoyment (Chronbach’s α = .917), value/usefulness (Chronbach’s α = .877), effort/importance (Chronbach’s α = .849), and pressure/tension (Chronbach’s α = .739). Thus, scores were averaged within subscales.

Skill study and practice putts. The amount of time participants spent looking at the putting instruction booklet was recorded in order to quantify the amount of time they spent studying the skill during the practice phase. Specifically, during the initial study phase of at least 2 min, a researcher controlled a chronometer on a computer located less than 2 m from the participant. The researcher used the chronometer to record the time the participant was looking at the booklet, as opposed to looking elsewhere. After the initial 2 min, the researcher told the participant that the “two minutes time is over, but you may study the booklet for as long as you want.” When participants told the researcher that they were done studying, the researcher recorded the final study time for the initial study phase. During the 1 min breaks between acquisition blocks, the researcher used the chronometer to record the amount of time the participant spent looking at the instruction booklet. The initial study time was then combined with the between-blocks study time to yield the ‘studying’ variable. The number of putts they took during the practice phase was recorded to quantify their practice repetitions; this variable is referred to as ‘putts’.

Statistical Analysis

To measure motor learning, multiple linear regressions for RE and BVE, each averaged across retention and transfer tests, were conducted[5]. The first step in each regression included the following variables: pretest RE or BVE, putts, and studying. The second step of each regression included the following variables: expectation (test/teach) and motor preparation (limited/unlimited). The third step of each regression included the interaction term: Expectation x Motor Preparation.

To measure practice performance, mixed-factor ANCOVAs were conducted for RE and BVE: 2 (Expectation) x 2 (Motor Preparation) x 3 (Practice Block: 1/3/5), with repeated measures on the last factor and pretest RE or BVE serving as the covariate.

IMI subscales, studying, putts, and free recall were submitted to 2 (Expectation) x 2 (Motor Preparation) between-subjects ANOVAs. Pre-movement time was submitted to an independent sample *t*-test (Expectation) for the groups with unlimited motor preparation.

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[5] To justify averaging across retention and transfer tests for RE and BVE, we preceded the regressions with mixed-factor ANCOVAs: 2 (Expectation) x 2 (Motor Preparation) x 2 (Posttest: retention/transfer), with repeated-measures on the last factor. Pretest RE or BVE, studying, and putts served as covariates, and RE or BVE served as the dependent variable. Since posttest did not interact with either expectation and motor preparation (ps $\geq .257$), we were justified in averaging across the retention and transfer tests in order to reduce the number of regressions we conducted (Lohse, Buchanan, & Miller, 2016).

Alpha levels were set to .05 for all tests, and the Greenhouse-Geisser correction is provided when sphericity was violated. The *p*-values reported are based on the corrected degrees of freedom. Results will be reported beginning with mechanistic variables and free recall, followed by practice performance, and then posttest performance.

Results

Studying, Putts, Motivation, Pressure, Pre-Movement Time, and Free Recall

No effects of expectation ($ps \geq .106$), motor preparation ($ps \geq .333$), or Expectation x Motor Preparation ($ps \geq .605$) were found for studying or putts (see Table 1). Similarly, no effects of expectation ($ps \geq .207$), motor preparation ($ps \geq .218$), or Expectation x Motor Preparation ($ps \geq .569$) were found for measures of motivation or pressure. For free recall, a main effect of expectation was observed f ($F(1, 76) = 10.9$, $p = .004$, $\eta^2_p = .102$), but there were no effects of motor preparation or Expectation x Motor Preparation ($ps \geq .105$). There was no effect of expectation on pre-movement time ($p = .877$). In sum, these results replicate prior work in showing a strong effect of expecting to teach on free recall, but not on studying, putts, motivation, or pressure (Daou, Buchanan et al., 2016; Daou, Lohse et al., 2016; Daou, Lohse et al., 2018). Notably, that expecting to teach did not increase pre-movement time is in contrast to Daou, Lohse et al. (2016).

Table 1. Descriptive data for each group.

Descriptive Data by Group

Group:	Test Limited (*n* = 21; 14 females)		Teach Limited (*n* = 19; 14 females)		Test Unlimited (*n* = 20; 13 females)		Teach Unlimited (*n* = 20; 13 females)	
	M	*CI* (95%)	*M*	*CI* (95%)	*M*	*CI (95%)*	*M*	*CI (95%)*
Age (years)	20.9	20.3 – 21.4	22.3	20.5 – 24.2	21.1	20.3 – 22.0	22.9	20.9 – 24.8
Lifetime Putting Experience[a]	1.38	0.98 – 1.77	1.63	1.17 – 2.09	1.40	0.98 – 1.81	1.15	0.80 – 1.49
Past-Year Putting Experience[a]	0.476	0.243 – 0.708	0.368	0.130 – 0.606	0.500	0.260 – 0.740	0.450	0.211 – 0.688
Studying (s)	203	162 – 243	227	183 – 270	212	169 – 254	258	205 – 310
Putts	54.2	49.1 – 59.3	55.4	50.5 – 60.2	57.1	50.1 – 64.2	58.7	50.1 – 67.3
Intrinsic Motivation	5.38	4.93 – 5.82	5.37	4.96 – 5.77	5.37	4.85 – 5.88	5.49	4.97 – 6.00
Internalized Motivation	6.00	5.60 – 6.39	5.69	5.21 – 6.16	5.95	5.52 – 6.37	5.73	5.29 – 6.16
General Motivation	5.40	4.90 – 5.89	5.09	4.53 – 5.64	5.57	5.13 – 6.00	5.50	5.03 – 5.96
Pressure	2.37	1.86 – 2.87	2.40	2.02 – 2.77	2.33	1.81 – 2.84	2.11	1.73 – 2.48
Free Recall	2.52	2.12 – 2.91	3.05	2.55 – 3.54	1.90	1.29 – 2.50	2.85	2.26 – 3.43
Pre-Movement Time (s)	-	-	-	-	6.05	4.28 – 7.81	5.88	4.49 – 7.27

[a] 0 = Never putted; 1= Putted 1 – 10 times; 2 = Putted 11 – 20 times; 3 = Putted 21 – 30 times

Practice Performance

Figure 1A shows accuracy (RE) for the groups across all phases of the study. During practice, there were no effects of expectation ($p = .575$), motor preparation ($p = .210$), block ($p = .657$), Expectation x Motor Preparation ($p = 0.990$), Expectation x Block ($p = .630$), Motor Preparation x Block ($p = .180$), or Expectation x Motor Preparation x Block ($p = .880$) on RE, controlling for pretest RE. Figure 1B depicts precision (BVE) for groups across all phases of the study. During practice, there were no effects of expectation (p = .580), block (p = .855), Expectation x Motor Preparation (p =.580), Expectation x Block (p = .606), or Expectation x Motor Preparation x Block (p = .470) on BVE, controlling for pretest BVE. However, there were significant effects of motor preparation ($F(1, 75) = 5.42$, $p = .023$, $\eta^2_p = .067$) and Motor Preparation x Block ($F(1.16, 87.1) = 3.82$, $p = .048$, $\eta^2_p = .048$). Follow-up univariate ANCOVAs (motor preparation) for each block (controlling for pretest BVE) revealed participants with limited motor preparation exhibited superior precision for block 1 (M_{adjusted} = 31.6 cm, $CI_{95\%}$ = 22.1 – 41.1 cm) relative to their counterparts with unlimited motor preparation (M_{adjusted} = 46.6 cm, $CI_{95\%}$ =37.2 – 56.1 cm), but motor preparation time did not affect precision for blocks 3 (p = .313) or 5 (p = .221). In summary, practice performance results replicate prior findings that expecting to teach does not influence practice accuracy or precision (Daou, Buchanan et al., 2016; Daou, Lohse et al., 2016; Daou, Lohse et al., 2018), and indicate limiting motor preparation time may have a benefit to precision on early practice trials.

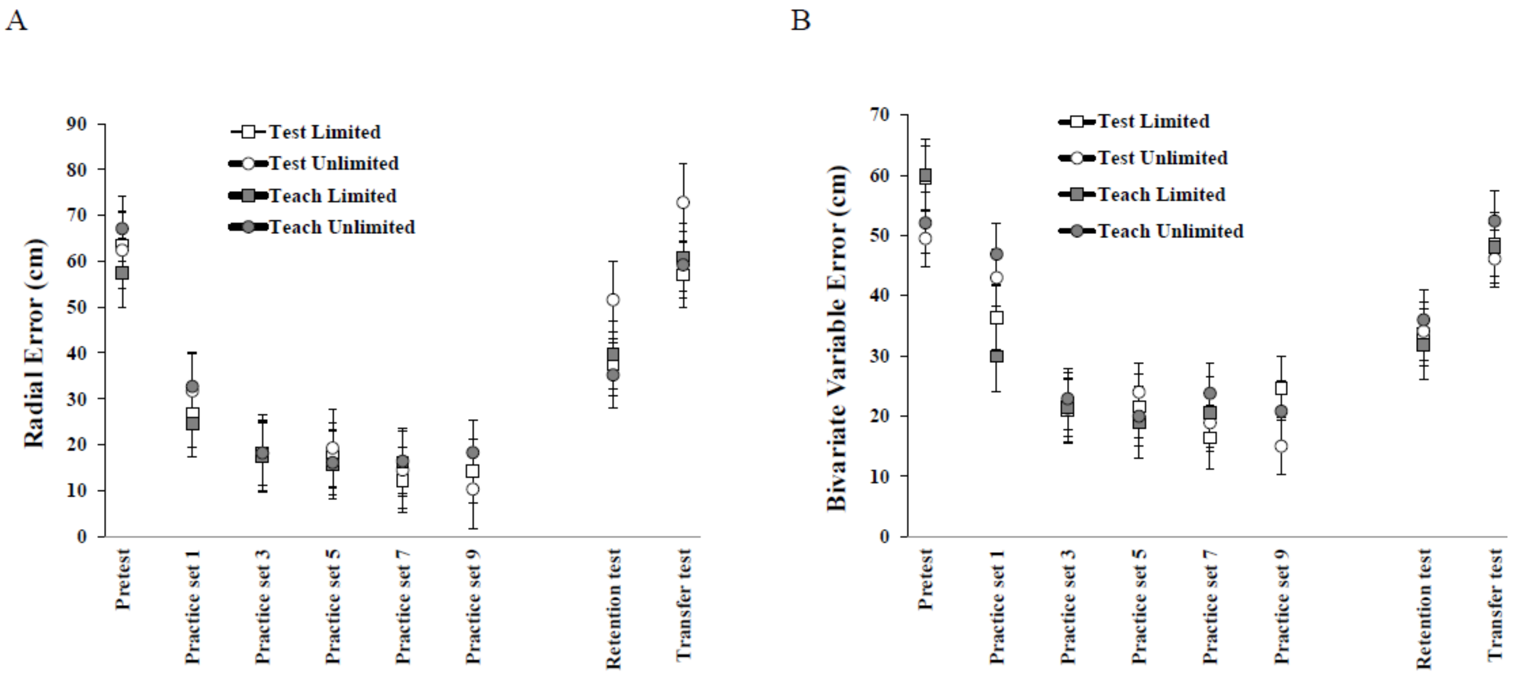

Figure 1. A) Putting accuracy (lower RE indicates greater accuracy) as a function of study phase and group. B) Putting precision (lower BVE indicates greater precision) as a function of study phase and group. All error bars represent 95% *CI*s.

Posttest Performance

The first step of the regression predicting posttest RE revealed pretest RE to be a significant predictor (ß$_{\text{pretest}}$ = 0.284 cm, p = .004), but neither studying (p = .605) or putts (p = .693) were significant predictors (see Table 2). The second step of the regression added expectation and motor preparation, neither of which were significant predictors (ps = .469 and .417, respectively). The final step of the regression added Expectation x Motor Preparation, but this interaction term was not a significant predictor (p = .139).

Table 2. Details of regression models testing the hypotheses that expectation, motor preparation, and Expectation x Motor Preparation predict posttest RE.[a]

Model 1: Avg. Retention and Transfer Test RE ~ Pretest + Studying + Putts

	SS	*Df*	*MS*	*F*	R^2 Change
Regression	9452	3	3151	3.31	.116
Residual	72,292	76	951		
Coefficients	β	*CI*	*t*-value	*p*-value	
Intercept	44.3	11.77 – 76.8	2.71	.008	
Pretest RE	0.284	0.095 – 0.474	2.99	.004	
Studying	-0.020	-0.098 – 0.058	0.520	.605	
Putts	-0.104	-0.626 – 0.418	0.396	.693	

Model 2: Avg. Retention and Transfer Test RE ~ Pretest + Studying + Putts + Expectation + Motor Preparation

	SS	*Df*	*MS*	*F*	R^2 *Change*
Regression	10,594	5	2119	2.20	.014
Residual	71,150	74	961		
Coefficients	β	*CI*	*t*-value	*p*-value	
Intercept	45.2	12.0 – 78.3	2.72	.008	
Pretest RE	0.277	0.086 – 0.468	2.89	.005	
Studying	-0.017	-0.097 – 0.063	0.421	.675	
Putts	-0.125	-0.653 – 0.402	0.473	.638	
Expectation	-2.57	-9.62 – 4.47	0.727	.469	
Motor Preparation	2.86	-4.12 – 9.84	0.817	.417	

Model 3: Avg. Retention and Transfer Test RE ~ Pretest + Studying + Putts + Expectation + Motor Preparation + Expectation x Motor Preparation

	SS	*Df*	*MS*	*F*	R^2 *Change*
Regression	12,709	6	2118	2.24	.026
Residual	69,036	73	946		
Coefficients	β	*CI*	*t*-value	*p*-value	
Intercept	44.2	11.3 – 77.1	2.68	.009	
Pretest RE	0.285	0.095 – 0.474	2.99	.004	
Studying	-0.015	-0.094 – 0.065	0.366	.715	
Putts	-0.124	-0.647 – 0.400	0.471	.639	
Expectation	-2.61	-9.60 – 4.38	0.743	.460	
Motor Preparation	2.69	-4.24 – 9.62	0.774	.441	
Expectation x Motor Preparation	-5.16	-12.0 – 1.72	1.50	.139	

[a]Regression coefficients are not standardized and are thus interpretable in their natural units. For expectation, test is coded as '-1' and teach as '1'. For motor preparation, limited is coded as '-1' and unlimited as '1'. *CI* is 95%.

The first step of the regression predicting posttest BVE revealed pretest BVE to be a significant predictor (ß$_{\text{pretest}}$ = 0.184 cm, *p* = .001), but neither studying (*p* = .860) nor putts (*p* = .703) were significant predictors (see Table 3). The second step of the regression added expectation and motor preparation, neither of which were significant predictors (*p*s = .712 and .307, respectively). The final step of the regression added Expectation x Motor Preparation, but this interaction term was not a significant predictor (*p* = .479).

Table 3. Details of regression models testing the hypotheses that expectation, motor preparation, and Expectation x Motor Preparation predict posttest BVE.[a]

Model 1: Avg. Retention and Transfer Test BVE ~ Pretest + Studying + Putts					
	SS	*Df*	*MS*	*F*	*R*^{2} Change
Regression	3093	3	1031	4.67	.156
Residual	16,773	76	221		
Coefficients	β	*CI*	*t*-value	*p*-value	
Intercept	33.1	17.7 – 48.6	4.27	< .001	
Pretest BVE	0.184	0.080 – 0.289	3.50	.001	
Studying	0.003	-0.034 – 0.040	0.177	.860	
Putts	-0.048	-0.297 – 0.201	0.383	.703	

Model 2: Avg. Retention and Transfer Test BVE ~ Pretest + Studying + Putts + Expectation + Motor Preparation					
	SS	*Df*	*MS*	*F*	*R*^{2} *Change*
Regression	3359	5	672	3.01	.013
Residual	16,507	74	223		
Coefficients	β	*CI*	*t*-value	*p*-value	
Intercept	34.0	18.3 – 49.6	4.33	<.001	
Pretest BVE	0.194	0.087 – 0.301	3.61	.001	
Studying	0.000	-0.039 – 0.038	0.021	.984	

Putts	-0.057	-0.308 – 0.194	0.454	.652
Expectation	0.631	-2.76 – 4.02	0.371	.712
Motor Preparation	1.76	-1.65 – 5.16	1.03	.307

Model 3: Avg. Retention and Transfer Test BVE ~ Pretest + Studying + Putts + Expectation + Motor Preparation + Expectation x Motor Preparation

	SS	*Df*	*MS*	*F*	R^2 *Change*
Regression	3473	6	579	2.58	.006
Residual	16,393	73	225		
Coefficients	β	*CI*	*t*-value	*p*-value	
Intercept	34.1	18.4 – 49.8	4.33	<.001	
Pretest BVE	0.194	0.087 – 0.301	3.60	.001	
Studying	-0.001	-0.040 – 0.037	0.061	.951	
Putts	-0.056	-0.308 – 0.195	0.447	.656	
Expectation	0.643	-2.76 – 4.04	0.377	.707	
Motor Preparation	1.79	-1.63 – 5.21	1.05	.299	
Expectation x Motor Preparation	1.20	-2.15 – 4.54	0.711	.479	

[a]Regression coefficients are not standardized and are thus interpretable in their natural units. For expectation, test is coded as ‘-1’ and teach as ‘1’. For motor preparation, limited is coded as ‘-1’ and unlimited as ‘1’. *CI* is 95%.

In summary, expectation, motor preparation, and Expectation x Motor Preparation did not predict posttest accuracy or precision, neither of which were predicted by studying or putts. Expectation not predicting posttest accuracy and precision is inconsistent with past studies (Daou, Buchanan et al., 2016; Daou, Lohse et al., 2016; Daou, Lohse et al., 2018). Given this null result, we conducted a cumulative analysis of the current data combined with past work.

Cumulative Analysis of Past and Present Expecting to Teach Experiments

The present results differ in a few ways from other results our lab has published (Daou, Buchanan et al., 2016; Daou, Lohse et al., 2016; Daou, Lohse et al., 2018). Specifically, present results did not reveal a main effect of expecting to teach on RE and BVE (Daou, Buchanan et al., 2016; Daou, Lohse et al., 2016; Daou, Lohse et al., 2018) nor did present results reveal a main effect of expecting to teach on pre-movement time (Daou, Lohse et al., 2016). Notably, these experiments sampled from similar populations in terms of age, sex, and putting experience (college-age males and females with little putting experience). To determine whether differences in results can be attributed to sampling variability or failed replication, we combined the present data from participants whose motor preparation time was unlimited with data from Daou, Buchanan et al. (2016) and Daou, Lohse et al. (2016). Both of these prior experiments used the same task, blindfolded/ear-plugged pretests and 1 day posttests, allowed the amount of study time and putts to vary, and permitted unlimited motor preparation time. (Daou, Lohse et al. (2018) used longer putt distances, did not use a blindfold/earplugs, and fixed the amount of study time and putts.)

For RE and BVE, we conducted 3 (Experiment: Daou, Buchanan et al./Daou, Lohse et al./Present Experiment) x 2 (Expectation) x 2 (Posttest) mixed factor ANCOVAs, with the final factor serving as a repeated-measure. We controlled for pretest RE or BVE, studying, and putts. For RE, results showed a main effect of expectation ($F(1, 143) = 21.8$, $p < .001$, $\eta^2_p = .132$), with the teach group exhibiting lower RE ($M_{\text{adjusted}} = 41.6$ cm, $CI_{95\%} = 34.9 – 48.4$ cm) than the test group ($M_{\text{adjusted}} = 64.6$ cm, $CI_{95\%} = 57.8 – 71.4$ cm). Notably, the Experiment x Expectation interaction was not significant ($F(2, 143) = 0.502$, $p = .607$, $\eta^2_p = .007$). Thus, these results suggest the effect of expecting to teach on RE did not significantly differ among the present experiment and prior experiments, which, together, reveal a significant effect of expecting to teach (see Figure 2A). Accordingly, the nonsignificant effect in the present experiment is likely due to sampling variability.

For BVE, results showed a main effect of expectation ($F(1, 143) = 17.4$, $p < .001$, $\eta^2_p = .108$), with the teach group exhibiting lower BVE ($M_{\text{adjusted}} = 37.8$ cm, $CI_{95\%} = 34.4 – 41.2$ cm) than the test group ($M_{\text{adjusted}} = 48.0$ cm, $CI_{95\%} = 44.6 – 51.4$ cm). However, this effect was superseded by a significant Experiment x Expectation interaction ($F(2, 143) = 6.52$, $p = .002$, $\eta^2_p = .084$) (see Figure 2B). Thus, these results suggest either (a) the significant effect of expecting to teach on BVE in the prior experiments were false positive results, or (b) the nonsignificant effect of expecting to teach on BVE in the present experiment is a false negative result. Given that significant effects were observed in the two experiments included in this cumulative analysis as well as Daou, Lohse et al. (2018), it seems more likely that the nonsignificant effect of expecting to teach on BVE in the present experiment is a false negative.

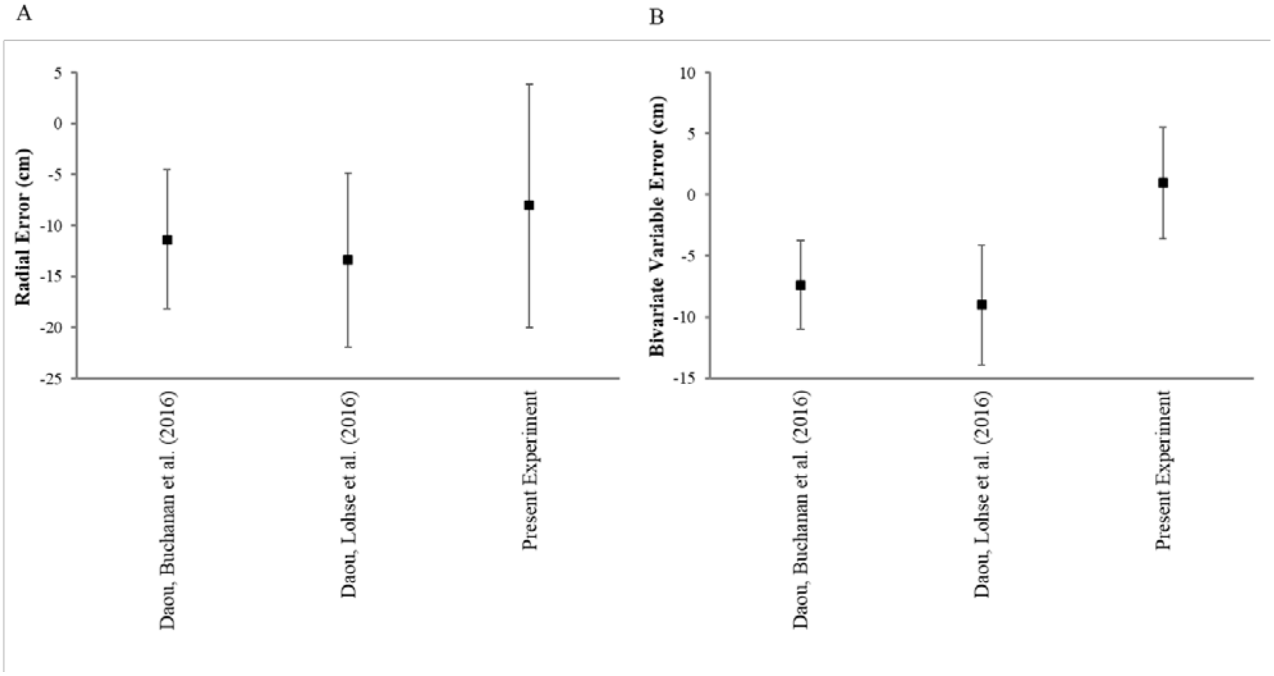

Figure 2. Unstandardized beta coefficient of the effect of expecting to teach on posttest (avg. retention and transfer test) RE (Panel A) and BVE (Panel B) in three experiments. Coefficients are based on a model accounting for pretest RE or BVE, studying, and putts. Negative numbers represent positive effects (reduced error) in favor of the expecting to teach group (contrast coded as '1' relative to the expecting to test group, which is coded as '-1'), and error bars represent 95% *CI*s.

When taken together, results from the present experiment and three others published by our laboratory suggest expecting to teach improves posttest RE as well as BVE. However, the mechanisms underlying these effects remain unclear. Daou, Lohse et al. (2016) proposed that pre-movement time may be an underlying mechanism based on their observation that expecting to teach increased pre-movement time, which predicted posttest RE and BVE. However, the present experiment did not show this effect. To address this inconsistency, we combined data from Daou, Lohse et al. and the present experiment, and conducted a 2 (Experiment) x 2 (Expectation) between-subjects ANOVA with pre-movement time serving as the dependent variable. (Again, we only included participants from the present experiment whose preparation time was unlimited.) Results revealed a significant effect of expectation ($F(1, 91) = 7.73$, $p = .007$, $\eta^2_p = .078$), such that participants who expected to teach exhibited longer pre-movement time ($M = 7.37$ s, $CI_{95\%} = 6.55 – 8.19$ s) than participants who expected to test ($M = 5.73$ s, $CI_{95\%} = 4.90 – 6.57$ s). However, this main effect was superseded by an Experiment x Expectation interaction ($F(1, 91) = 9.38$, $p = .003$, $\eta^2_p = .093$) (driven by the significant effect of expectation in Daou, Lohse et al. and nonsignificant effect of expectation in the present experiment). Thus, there is no consistent effect of expectation on pre-movement time.

Next, we investigated whether increased pre-movement time is even a candidate for explaining the learning benefit of expecting to teach. In order to be a candidate, pre-movement time should predict learning, irrespective of experimental manipulation (expectation to teach/test). Thus, we used the data from Daou, Lohse et al. (2016) and the present experiment to address this issue. (Again, we only included participants from the present experiment whose preparation time was unlimited.) Specifically, we conducted a multiple linear regression to predict posttest RE averaged across retention and transfer tests. The first step of the regression included the following variables: pretest RE, expectation, studying, and putts; and the second step of the regression added motor preparation time. Results showed pre-movement time did not predict posttest RE ($p = .843$). A similar regression predicting posttest BVE also showed a null effect for pre-movement time ($p = .139$). Thus, pre-movement time in our golf putting paradigm does not seem to predict posttest RE or BVE. This suggests the significant relationship between pre-movement time and RE as well as BVE observed by Daou, Lohse et al. was simply due to participants who expected to teach exhibiting long pre-movement time *coincident* to low RE and BVE. Taken together, results from the cumulative pre-movement time analyses suggest that even if expecting to teach does increase pre-movement time, this increase does not explain the learning benefit of expecting to teach.

Discussion

The present experiment tested the hypothesis that increased motor preparation time preceding practice trials explains the motor learning benefit of practicing a skill with the expectation of teaching it to another person. Specifically, motor preparation time during practice was limited for approximately half of the participants who practiced with the expectation of teaching, and statistical tests examined whether the benefit of expecting to teach on posttest accuracy (RE) and precision (BVE) was eliminated for these participants.

Results showed limiting motor preparation time did not affect posttest accuracy or precision, but neither of these learning indices were improved by expecting to teach in the present experiment. However, we conducted cumulative analyses based on datasets from two other published experiments in our laboratory (Daou, Buchanan et al., 2016; Daou, Lohse et al., 2016). Results from this analysis suggest the effect of expecting to teach on posttest accuracy was within the range of sampling variability, albeit non-significant. There are multiple ways to judge replication success, beyond statistical significance, including whether the confidence interval of the present effect includes the effect obtained in past experiments (Open Science Collaboration, 2015). As such, we do not consider this a failed replication, but the underlying effect-size is likely smaller than originally estimated.

Results from the cumulative analysis suggest the null effect of expecting to teach on posttest precision in the present experiment is indeed a failed replication: The effect was not significant in the present experiment, and the confidence interval of the effect does not include the effects observed in prior experiments. However, we believe the present result is likely a false-negative given that the only three other published experiments investigating the expecting to teach effect have shown a benefit to posttest precision (Daou, Buchanan et al., 2016; Daou, Lohse et al., 2016; Daou, Lohse et al., 2018). Cumulatively then, we believe evidence suggests expecting to teach enhances motor learning, as measured by posttest accuracy and precision. Notably, results demonstrated expecting to teach enhanced free recall of putting techniques at posttest, replicating prior experiments and indicating expecting to teach enhances both procedural knowledge, reflected by posttest accuracy and precision, as well as declarative knowledge, represented by free recall (Daou, Buchanan et al., 2016; Daou, Lohse et al., 2016; Daou, Lohse et al., 2018).

Taken together, past experiments and the present experiment suggest expecting to teach enhances motor learning, but these experiments provide little insight into the mechanisms underlying this effect. Specifically, the present experiment revealed that expecting to teach does not enhance motivation or pressure during practice, which is consistent with past experiments (Daou, Buchanan et al., 2016; Daou, Lohse et al., 2016; Daou, Lohse et al., 2018). Additionally, the present experiment failed to reveal expecting to teach enhances the duration of motor preparation during practice, which is inconsistent with Daou, Lohse et al. (2016). Importantly, a cumulative analysis of these two datasets suggests motor preparation time does not predict posttest accuracy or precision, thereby making it unlikely that motor preparation time explains the learning benefit of expecting to teach. Further, when one considers Daou, Lohse et al. (2018) observed expecting to teach does not influence cerebral cortical dynamics during motor preparation while practicing, it appears expecting to teach does not affect information processing during motor preparation, at least in the ways by which we have measured motor preparatory information processing.

Thus, the question of what mechanisms underlie the motor learning benefit of expecting to teach remains open. One possibility is that expecting to teach affects neurocognitive processes at the end of each practice trial rather than at the beginning (during motor preparation). Specifically, successful practice trials may be more rewarding to participants who practice with the expectation of teaching relative to their counterparts who practice with the expectation of being tested. This could be the case if participants who expect to teach place more emphasis on successful outcomes knowing that they will have to teach another participant how to achieve such outcomes. If participants who expect to teach find successful practice trials highly rewarding, then they will experience large positive reward-prediction errors (RPEs) and phasic increases in dopaminergic activity after successful practice trials (Wulf & Lewthwaite, 2016). These enhanced RPEs and dopaminergic responses can facilitate consolidation of the movement patterns that produced the successful trial. Future research may investigate the possibility that expecting to teach enhances learning by amplifying RPEs, specifically by indexing RPEs to augmented feedback about practice trial outcome with the EEG-derived reward positivity component of the event-related potential waveform (Grand, Daou, Lohse, & Miller, 2017). Additionally (or alternatively), participants who expect to teach may pay closer attention to trial outcomes knowing that they will have to teach another participant how to achieve successful outcomes. This enhanced attention to trial outcomes could also be investigated by indexing processing of augmented feedback with EEG (Grand et al., 2015).

In conclusion, practicing a motor skill with the expectation of teaching it to another person appears to enhance learning, as indexed by skill accuracy and precision on delayed posttests as well as declarative knowledge about the skill. The learning effects on skill accuracy and precision do not appear to be related to motor preparatory processes, motivation, or pressure during practice, leaving the mechanisms underlying the benefit of practicing with the expectation of teaching unknown. Thus, we suggest instructors have learners practice with the expectation of teaching, and researchers attempt to elucidate mechanisms underlying the beneficial effects of such practice.

Chapter 5: Learning a skill with the expectation of teaching it impairs the skill's execution under psychological pressure

Introduction

Recent studies suggest that practicing and studying a motor skill with the expectation of teaching it enhances learning in comparison to practicing and studying with the expectation of being tested. Specifically, expecting to teach improves skill accuracy and precision, suggesting enhanced procedural knowledge, while also increasing the ability to recall key concepts related to the skill, indicating greater declarative knowledge (Daou, Buchanan, Lindsey, Lohse, & Miller, 2016; Daou, Lohse, & Miller, 2016; Daou, Lohse, & Miller, 2018). A few different mechanisms potentially underlying the expecting to teach effect have been investigated. It was initially thought that motivation and pressure would explain the effect. This followed from the reasoning that expecting to teach should cause a learner to recognize that their own learning might affect another person's learning, thereby increasing their drive and pressure to learn. Heightened motivation and pressure while practicing and studying, in turn, could yield psychological and physiological states adaptive for learning. However, neither motivation nor pressure were found to differ as a function of expecting to teach (Daou, Buchanan, et al, 2016; Daou, Lohse et al., 2016; Daou, Lohse et al., 2018).

Turning from these affective-motivational mechanisms, Daou, Lohse et al. (2016) examined whether enhanced information processing during practice and studying could explain the expecting to teach effect. The authors reasoned that expecting to teach may cause a learner to process more information about the skill they are practicing and studying, knowing that they will have to transmit this information to another person, and that greater information processing should improve learning. The authors used the amount of time spent in motor preparation before each practice trial as a proxy for information processing and observed that motor preparation time was lengthened by expecting to teach and predicted learning (although not when controlling for whether participants expected to teach). In a follow-up experiment, Daou, Lohse et al. (2018) employed electroencephalography (EEG) to examine cerebral cortical dynamics during the final 3-s of motor preparation before each practice trial and did not observe any effects of expecting to teach. In summary, expecting to teach appears to improve motor learning, possibly by increasing the duration of information processing during motor preparation, but not by altering cortical dynamics during the final seconds of preparation.

Although expecting to teach has been shown to be an effective technique to enhance learning, it is possible that the expecting to teach benefit may be eliminated when a learner is asked to perform the acquired skill under certain conditions, in particular under psychological pressure. This follows because expecting to teach enhances declarative knowledge (explicit facts) about the skill in addition to procedural knowledge, manifested as improved skill accuracy and precision (Daou, Buchanan et al., 2016; Daou, Lohse et al., 2016; Daou, Lohse et al., 2018). The increase in declarative knowledge may be particularly caused by expecting to teach prompting learners to attend to explicit facts that they can disseminate to another person. A potential downside of this, however, is that motor skills acquired concomitant to large gains in declarative knowledge are more susceptible to decrement under psychological pressure than skills learned relatively implicitly (Hardy, Mullen, & Jones, 1996; Koedijker, Oudejans, & Beek, 2007; Lam, Maxwell, & Masters, 2009a, 2009b; Liao & Masters, 2001; Masters, 1992).

The phenomenon that motor skills acquired with large gains in declarative knowledge are highly susceptible to deterioration under pressure is consistent with Reinvestment Theory (Masters & Maxwell, 2008). This theory contends that dispositional and situational factors, such as psychological pressure, trigger individuals to use declarative knowledge acquired earlier in learning to attempt to consciously monitor and control practiced movements. This focus of attention on movement, paradoxically, impairs performance (Wulf & Su, 2007), likely due to inefficient muscle activation as well as invariable and uncorrelated effector movement (Lohse, Jones, Healy, & Sherwood, 2014; Lohse & Sherwood, 2012; Lohse, Sherwood, & Healy, 2010; Lohse, Sherwood, & Healy, 2011). Crucially, learners who accrue more declarative knowledge during skill practice are more likely to exhibit performance decrement under pressure, because they have more declarative knowledge to 'reinvest' in motor control. Notably, Reinvestment Theory is similar to other with explanations describing motor skill decrement under high psychological pressure (choking under pressure). Specifically, 'explicit monitoring' theories of choking argue that pressure causes individuals to closely attend to their movements, consequently worsening performance on motor skills largely relying on procedural knowledge (Baumeister, 1984; DeCaro, Thomas, Albert, & Beilock, 2011).

In light of Reinvestment Theory, the aim of the present study was to investigate whether the learning benefit of expecting to teach is eliminated when the acquired skill is performed under high pressure. It was predicted that participants who practice and study with the expectation of teaching would exhibit superior learning, but that this advantage would be moderated by the condition under which learning was assessed. Specifically, participants who expected to teach were hypothesized to exhibit superior performance on a low pressure posttest, but not on a high pressure posttest, due to a decrease in performance (choking) under high pressure. Further, it was predicted that this choking effect would be mediated by the amount of declarative knowledge participants used during posttests, which was hypothesized to be higher for those expecting to teach and correlated with the magnitude of choking.

Methods

Prior to beginning data collection, the experimental design and analyses were registered and made public on AsPredicted.org (https://aspredicted.org/zb44r.pdf).

Participants

Eighty-two right-handed young adults (56 females), ages between 18 and 27 years (M_{age} = 20.8 years, *SD* = 1.14 years), participated in the study after consenting to a protocol approved by the Auburn University Institutional Review Board (#16-484 EP 1612). Participants were recruited from university courses and by word-of-mouth, and were compensated with course credit. In addition, the five best performers during the high pressure posttest received between $10 (fifth place) and $50 (first place) (see more details in Posttest section). Sample size was determined with an a priori power calculation providing 95% power ($\alpha = .05$) to detect a moderate-sized ($\eta^2_p = .09$) Between-Subject x Within-Subject interaction (groups = 2; measurements = 3; nonsphericity correction = 1).

Task

All participants used a standard (89 cm), right-handed golf putter to putt a standard golf ball from a starting position indicated by a 5 cm line painted in white washable paint on an artificial grass surface to a target cross (+) comprised of two 10.8 cm lines painted in white washable paint and located 300 cm away from the starting position. Participants' objective was to make the ball stop as close to the center of the target as possible.

Procedure

All participants completed the experiment individually. After consenting to the experiment, participants completed a demographic questionnaire asking their age, sex, and putting experience (anything from miniature golf to playing 18 holes on a standard golf course) over their lifetime and within the past year. Then, participants put a physiological monitoring device around their chest (BioHarness 3.0, Zephyr Technology, Annapolis, MD) in order to get used to wearing it, which they would be asked to do the following day as well.

Pretest. After completing the demographic questionnaire, participants performed the pretest phase, which consisted of one block of ten putts.

Practice. After pretest, participants were quasi-randomly assigned to teach or test group. Quasi-randomization was based on pretest accuracy score (distance from target). Specifically, participants' pretest accuracy placed them in one of three categories (< 24 cm, 24 - 49 cm, > 49 cm),[6] within which they were randomly assigned to the teach group or test group. After quasi-randomization, the expecting to teach/expecting to test manipulation occurred. Participants in the teach group were told, “Tomorrow you will teach another participant how to putt,” and participants in the test group were told, “Tomorrow you will be tested on your putting skills.” Next, participants completed the practice phase. First, participants studied a golf putting instruction booklet for 2 min. The booklet consisted of written and pictorial descriptions of proper putting technique, as described by an expert golfer (for booklet, see the appendix). Next, participants performed six blocks of ten putts, taking a 1 min break between each block (participants sat in a chair during the breaks). When participants stopped practicing, they completed the Intrinsic Motivation Inventory (IMI) (Ryan, 1982) for possible exploratory analyses related to motivation and pressure during practice.

Posttests (Low Pressure and High Pressure). One day after completing pretest and practice, participants returned to complete the experiment. Participants in the teach group were told, “The participant who you were going to teach did not show up today, so you will actually be tested on your putting instead.” Then, participants put on the Bioharness, which was used to provide a physiological measure of anxiety (heart rate) during the posttests. Next, they completed low pressure and high pressure tests in counterbalanced order. For the low pressure test, the experimenter told participants, “In this set of ten putts, your goal is to make the ball stop as close

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[6] Category range was based on pilot data ($N = 12$).

to the center of the target as possible. Please, try to do the best you can." For the high pressure test, the experimenter told participants, "In the next set of ten putts, you will be recorded and critically analyzed by a golf expert who will give you a grade." The experimenter took an iPad pro 9.7 (240 x 169 x 6.1 mm) from a cabinet and affixed it to the edge of a 73 cm high table, approximately 45° to the right and 225 cm in front of participants. The iPad's screen faced participants so that they could see themselves being recorded. After the iPad was set-up, the experimenter told participants, "The combination of the golf expert grade and your performance during this set will allow you to compete against the rest of the participants for the 1st prize of $50, 2nd prize of $40, 3rd prize of $30, 4th prize of $20, and 5th prize of $10. In summary, you will be putting for money." As the experimenter explained the rewards, he took an envelope from a cabinet, pulled money from it, and displayed the potential monetary rewards to participants, after which he placed the money on a 91 cm high countertop, approximately 30° to the left and 100 cm in front of participants.

After each pressure manipulation (but before actually starting each posttest), participants completed the Revised Competitive State Anxiety Inventory-2 (CSAI-2R) (Cox, Martens, & Russell, 2003) in order to determine manipulation efficacy. The CSAI-2R is frequently used to assess anxiety in motor skill studies (Allsop & Gray, 2014; Elliot, Polman, & Taylor, 2014; Kinrade, Jackson, & Ashford, 2015; Kuan, Morris, Kueh, & Terry, 2018; Mullen, Jones, Oliver, & Hardy, 2016) and possesses good psychometric properties (Cox et al., 2003). The cognitive and somatic anxiety subscales were of interest, since the pressure manipulation was intended to modulate anxiety (nonetheless, participants did complete the self-confidence subscale as well)

(Jackson, Ashford, & Norsworthy, 2006). The cognitive and somatic anxiety subscale items ask participants to report how much they are currently feeling various indicators of anxiety. All responses were made by reporting a number between 0 and 100 on a scale with “not at all” corresponding to 0, followed by “somewhat”, then “moderately so”, and finally “very much so”, which corresponded to 100.

After finishing posttests, participants completed a free recall test to measure declarative knowledge use. Specifically, participants were asked to report, in as much detail as possible, any rules, methods, or techniques they recalled using to putt during the posttests. This type of free recall test is frequently used to assess declarative knowledge in motor skill studies (Daou, Buchanan et al., 2016; Daou, Lohse et al., 2016; Daou, Lohse et al., 2018; Maxwell, Masters, & Eves, 2000; Maxwell, Masters, Kerr, & Weedon, 2001; Zhu, Poolton, Wilson, Maxwell, & Masters, 2011).

Next, participants completed the Movement Specific Reinvestment Scale (MSRS) (Masters, Eves, & Maxwell, 2005). The MSRS is frequently used to examine individual tendencies to reinvest in motor control (Huffman, Horslen, Carpenter, & Adkin, 2009; Kal, van der Kamp, Houdijk, Groet, Bennekom, & Sherder, 2015; Klämpfl, Lobinger, & Raab, 2013; Malhorta, Poolton, Wilson, Ngo, & Masters, 2012; Vine, Moore, Cooke, Ring, & Wilson, 2013) and possesses good psychometric properties (Masters et al., 2005). The MSRS consists of the conscious motor processing and movement self-consciousness subscales, which ask participants to indicate how strongly they agree with statements related to their tendency to attempt to control their movements and monitor their movements, respectively. Participants respond on a 6-point scale anchored by strongly disagree and strongly agree. MSRS data was used to explore whether individual tendencies toward reinvestment would significantly moderate any effects of expecting to teach on choking under pressure.

Next, participants completed the shortened operation span task (OSPAN), which indexes working memory capacity (Foster et al., 2014). OSPAN data were intended to be used to examine whether individual differences in working memory capacity would significantly moderate any effects of expecting to teach on choking, given that working memory capacity is associated with one's likelihood of choking (Beilock & Carr, 2005; Wood, Vine, & Wilson, 2016). (Whether high working memory capacity increases or decreases the likelihood of choking is debatable.) Unfortunately, problems with OSPAN data collection led to removal of this data. Finally, participants were debriefed regarding the purpose of the study and dismissed.

Data Processing

Self-reported anxiety and heart rate. Good reliability was found among CSAI-2R subscale items reported prior to the low pressure (cognitive anxiety Chronbach's $\alpha = .781$; somatic anxiety Chronbach's $\alpha = .798$) and high pressure posttest (cognitive anxiety Chronbach's $\alpha = .846$; somatic anxiety Chronbach's $\alpha = .899$). Thus, items were averaged within each subscale for each posttest. Next, the cognitive and anxiety subscales were averaged together separately for each posttest, since the subscales were strongly correlated for each posttest (low pressure: $r = .549$, $p < .001$; high pressure: $r = .588$, $p < .001$). Thus, there was one self-reported anxiety score for the low pressure posttest and one score for the high pressure posttest.

Bioharness data was extracted and analyzed using Omnisense software (Zephyr Technology, Annapolis, MD). Specifically, heart rate was averaged from the time participants were read test instructions until they completed the test for the low and high pressure posttest.[7]

Putting. Putting accuracy was indexed by recording radial error as recommended by Hancock, Butler, and Fischman (1995): $Radial\ Error\ (RE) = (x^2 + y^2)^{1/2}$, where x and y represent the magnitude of error along the respective axes (i.e., how far away from the target cross the ball stopped in the horizontal and vertical directions). Precision was indexed by recording bivariate variable error as recommended by Hancock et al.: $Bivariate\ Variable\ Error\ (BVE) = \{(\frac{1}{k})\sum_{i=1}^{k}[(x_i - x_c)^2 + (y_i - y_c)^2]\}^{1/2}$, where k = trials in a block and $_c$ = centroid along the given axis (x or y) for that block. RE and BVE were calculated over pretest (10 putts) to get a baseline skill level, as well as for each of the six blocks (6 x 10 putts) of the practice phase to get an assessment of improvement in performance. To assess motor learning and choking under pressure, RE and BVE were calculated for the low and high pressure posttests.

Free recall. Three indices of declarative knowledge use were extracted from participants' responses on the free recall test.[8] First, 'all concepts' referred to the number of statements about a concept (rule) (e.g., "I held my left hand over above my right"), ignoring statements irrelevant to technical performance (e.g., "I was told to putt ten times to the target"). Second, 'key concepts' referred to the four most important rules in the golf putting instruction booklet: (1) establish proper grip, (2) place the putter head behind the ball and take a hip-width stance, (3)

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[7] Bioharness data was not successfully recorded for six participants.

[8] One participant's free recall data was lost.

place the eyes directly over the ball by hinging from the hips, and (4) stroke the ball without breaking the wrists (Daou, Buchanan et al., 2016). Third, hypothesis testing referred to statements indicating that the participant had tested hypotheses related to their putting stroke (e.g., "I adjusted the swing path of the putter after each missed ball" or "I tried to keep my head still throughout my putting stroke") (Maxwell et al., 2001). We ignored retrospective statements (e.g., "I held my left hand above my right" or "My feet were shoulder width apart") that may not have been used or thought about while putting, and we also ignored statements irrelevant to technical performance. Two researchers blind to participants' group assignment scored the declarative knowledge use measures. Next, their scores were correlated to examine interrater consistency. The correlation coefficients were strong and significant: all concepts (r = .788, $p \leq$ .001), key concepts (r = .685, $p \leq$ .001), and hypothesis testing (r = .815, $p \leq$ .001). Thus, the raters' scores for each measure were averaged. The hypothesis testing score was of greatest interest, as it has been most closely linked with reinvestment (Maxwell et al., 2001).

MSRS. Good reliability was found among movement self-consciousness items (Chronbach's α = .715), but not among conscious motor processing items (Chronbach's α = .452). Notably, the highest reliability was found when all items were considered (Chronbach's α = .750), so all items were summed into a single score (Malhorta et al., 2012). Next, we created a group of low-reinvestors (n = 36, M score = 24.2, 95% CI = 22.5 – 25.9) and high-reinvestors (n = 39, M score = 36.4, 95% CI = 35.1 – 37.7) by using a median split and excluding participants (n = 7) with the median score of 30.

Statistical Analysis

Paired sample *t*-tests (posttest: low pressure/high pressure) were conducted for self-reported anxiety and heart rate. To assess practice performance, 2 (Group: teach/test) x 6 (Practice Block: 1/2/3/4/5/6) ANCOVAs were conducted for RE and BVE, with pretest RE and BVE serving as the respective covariate. Prior to analysis, we conducted Mauchly's test for sphericity and the Greenhouse-Geisser correction was applied when sphericity was violated. To assess motor learning, 2 (Group) x 2 (Posttest) mixed-factor ANCOVAs (with repeated measures on the second factor) were conducted for RE and BVE, with pretest RE and BVE serving as the respective covariate. MSRS scores were added as a between-subjects factor to the 2 (Group) x 2 (Posttest) ANCOVAs. Independent sample *t*-tests (group) were conducted for free recall scores.

Results

Descriptive Data

Table 1 shows descriptive data for each group.

Table 1. Descriptive data for each group.

Descriptive Data by Group

	Test (*n* = 42; 32 females)		Teach (*n* = 40; 24 females)	
	M	95% *CI*	*M*	95% *CI*
Age (Years)	20.9	20.5 – 21.3	20.9	20.4 – 21.4

Lifetime Putting Experience[a]	1.24	0.97 – 1.51	1.30	1.03 – 1.57
Past-Year Putting Experience[a]	0.405	0.245 – 0.565	0.450	0.280 – 0.620
Low Pressure Self-Reported Anxiety	10.1	6.90 – 10.1	15.1	11.3 – 18.9
High Pressure Self-Reported Anxiety	20.3	15.3 – 25.3	24.6	19.0 – 30.2
Low Pressure Heart Rate (beats/min)	87.6	83.8 – 91.4	92.4	89.2 – 95.6
High Pressure Heart Rate (beats/min)	89.4	85.6 – 93.2	94.5	91.1 – 97.9
Free Recall All Concepts	5.76	5.09 – 6.43	7.03	6.37 – 7.70
Free Recall Key Concepts	1.88	1.61 – 2.15	2.17	1.92 – 2.42
Free Recall Hypothesis Testing	0.357	0.087 – 0.627	0.333	0.103 – 0.563
MSRS Score	30.0	27.9 – 32.1	31.0	28.7– 33.3

[a] 0 = Never putted; 1= Putted 1 – 10 times; 2 = Putted 11 – 20 times; 3 = Putted 21 – 30 times

Self-Reported Anxiety and Heart Rate

Both self-reported anxiety and heart rate were significantly elevated for the high pressure posttest relative to low pressure posttest (self-reported anxiety: $t(81) = 7.37$, $p < .001$, $d = 0.606$; heart rate: $t(75) = 4.67$, $p < .001$, $d = 0.168$), suggesting the psychological pressure manipulation was effective.

Practice Performance and Motor Learning

The left panel of Figure 1 shows accuracy (RE) for the groups across study phases. For practice RE, no main effect of group ($F(1, 79) = 2.06$, $p = .155$, $\eta^2_p = .025$) or block ($F(4.27, 338) = 1.03$, $p = .397$, $\eta^2_p = .013$, $\varepsilon = .855$) was observed nor was a Group x Block interaction revealed (*F*(4.27, 338) = 1.229, p = .298, η^2_p = .015, ε = .855), controlling for pretest RE. The right panel of Figure 1 shows precision (BVE) for the groups across all study phases. For practice BVE, no main effect of group (*F*(1, 79) = 2.45, p = .121, η^2_p = .030) or block (*F*(4.32, 341) = 1.32, p = .259, η^2_p = .016, ε = .864) was observed nor was a Group x Block interaction revealed (*F*(4.27, 338) = 1.229, p = .298, η^2_p = .015, ε = .864), controlling for pretest BVE. These results are consistent with past experiments showing expecting to teach does not improve performance while practicing the skill (Daou, Buchanan et al., 2016; Daou, Lohse et al., 2016; Daou, Lohse et al., 2018).

For posttest RE, a main effect of group was observed (*F*(1, 79) = 4.34, p = .041, η^2_p = .052), but a main effect of posttest was not (*F*(1, 79) = 1.31, p = .256, η^2_p = .016), controlling for pretest RE. Importantly, the predicted Group x Posttest interaction approached our alpha level (*F*(1, 79) = 3.82, p = .054, η^2_p = .046), controlling for pretest RE[9]. Thus, we conducted univariate (group) ANCOVAs for each posttest, controlling for pretest RE. The group effect was significant for the low pressure posttest, with the teach group exhibiting superior accuracy (*F*(1, 79) = 10.6, p = .002, η^2_p = .119). This result is consistent with prior experiments and indicates expecting to teach enhances motor learning as measured by accuracy (Daou, Buchanan et al., 2016; Daou, Lohse et al., 2016; Daou, Lohse et al., 2018). Importantly, the group effect was not significant for the high pressure posttest, with the teach and test groups exhibiting similar accuracy (*F*(1, 79) = 0.235, p = .630, η^2_p = .003). Crucially, the reason for this is revealed by paired sample *t*-tests

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[9] Notably, the Group x Posttest interaction is significant when not controlling for pretest RE (*F*(1, 79) = 4.35, p = .040, η^2_p = .052), which would be justifiable given that participants were quasi-randomly assigned to groups based on pretest RE.

demonstrating that teach group was significantly less accurate under high pressure relative to low pressure ($t(39) = 2.31$, $p = .026$, $d = 0.449$), whereas test group's accuracy exhibited a non-significant improvement ($t(41) = 0.627$, $p = .534$, $d = 0.102$). That is, expecting to teach enhanced motor learning as measured by accuracy, but this advantage was eliminated under high pressure, due to a choking effect caused by expecting to teach.

For posttest BVE, there was no main effect of group ($F(1, 79) = 3.04$, $p = .085$, $\eta^2_p = .003$) or posttest ($F(1, 79) = 0.581$, $p = .448$, $\eta^2_p = .007$), nor was there a Group x Posttest interaction ($F(1, 79) = 2.26$, $p = .137$, $\eta^2_p = .028$), controlling for pretest BVE. Nonetheless, we conducted exploratory univariate (group) ANCOVAs for each posttest, controlling for pretest BVE. The group effect was significant for the low pressure posttest, with the teach group exhibiting superior precision ($F(1, 79) = 6.87$, $p = .011$, $\eta^2_p = .080$). This result is consistent with prior experiments and indicates expecting to teach enhances motor learning as measured by precision (Daou, Buchanan et al., 2016; Daou, Lohse et al., 2016; Daou, Lohse et al., 2018). Importantly, the group effect was not significant for the high pressure posttest, with the teach and test groups exhibiting similar precision ($F(1, 79) = 0.188$, $p = .665$, $\eta^2_p = .002$). Crucially, the reason for this is revealed by paired sample *t*-tests demonstrating that teach group was significantly less precise under high pressure relative to low pressure ($t(39) = 2.39$, $p = .022$, $d = 0.489$), whereas the test group's precision exhibited a non-significant decrease ($t(41) = 0.189$, $p = .851$, $d = 0.032$). That is, exploratory analyses revealed expecting to teach enhanced motor learning as measured by precision, but this advantage was eliminated under high pressure, due to a choking effect caused by expecting to teach.

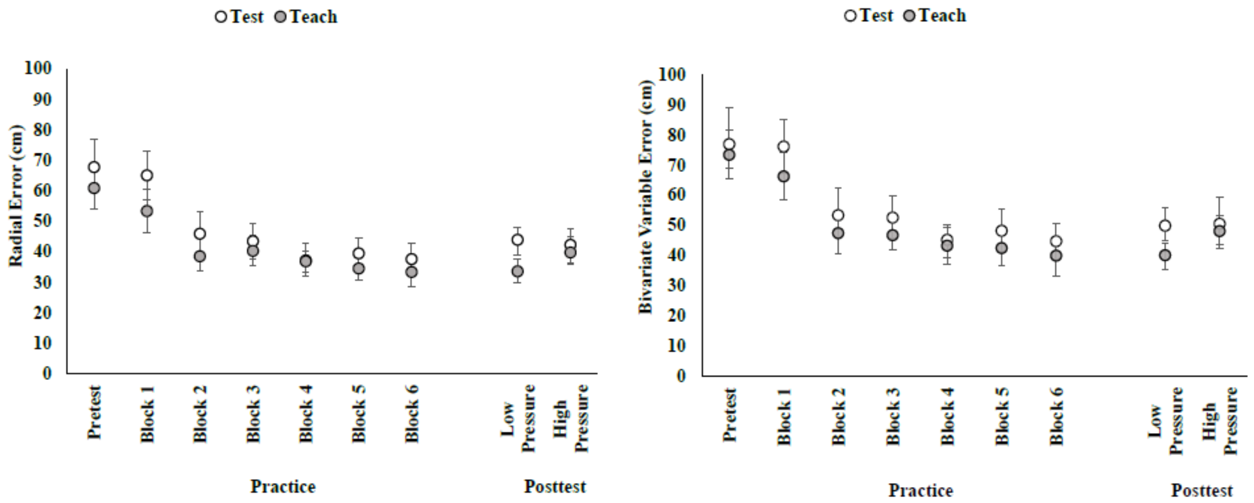

Fig. 1. Putting accuracy (left panel) and precision (right panel) as a function of study phase and group. Lower RE and BVE indicate superior accuracy and precision, respectively. All error bars represent 95% *CI*s. Generally, teach and test group exhibited similar accuracy and precision during pretest and practice. Conversely, teach group tended to show superior accuracy and precision under low pressure posttest, but not under high pressure posttest, wherein teach group's accuracy and precision declined (teach group choked under pressure).

MSRS

Next, we conducted an exploratory 2 (MSRS: low-reinvestor/high-reinvestor) x 2 (Group) x 2 (Posttest) ANCOVA for RE, with pretest RE serving as the covariate[10]. Results

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[10] We conducted similar exploratory analyses for BVE (that yielded results similar to those for RE). These analyses and results can be found in Supplementary Online Material. We focused on RE for the exploratory analysis and the subsequent mediation analysis, since RE was shown to be more sensitive to the effects of group and posttest in the primary analyses, and because choking under pressure is more commonly studied with respect to accuracy (represented by RE) than precision (represented by BVE).

failed to reveal a significant MSRS x Group (*F*(1, 70) = 1.01, $p = .319$, $\eta^2_p = .014$), MSRS x Posttest (*F*(1, 70) = 0.030, $p = .862$, $\eta^2_p < .001$), or MSRS x Group x Posttest interaction (*F*(1, 70) = 0.236, $p = .628$, $\eta^2_p = .003$). Notably, there was a main effect of MSRS (*F*(1, 70) = 5.43, $p = .023$, $\eta^2_p = .072$), with low-reinvestors exhibiting lower RE (M_{adjusted} = 37.0 cm, 95% *CI* = 33.2 cm – 40.9 cm) than high-reinvestors (M_{adjusted} = 43.2 cm, 95% *CI* = 39.5 cm – 46.9 cm). Further, the main effect of group (*F*(1, 70) = 6.88, $p = .011$, $\eta^2_p = .089$) and the Group x Posttest interaction (*F*(1, 70) = 4.61, $p = .035$, $\eta^2_p = .062$) became stronger when accounting for MSRS score. Thus, an individual's tendency to reinvest does not moderate the effect of expecting to teach on motor learning or choking under pressure, as measured by accuracy. However, accounting for an individual's tendency to reinvest does strengthen these effects, as one's tendency to reinvest explains individual differences in accuracy.

Free Recall

A significant effect of group was demonstrated for all concepts, with teach group exhibiting superior recall (*t*(79) = 2.58, $p = .012$, $d = 0.573$). However, no significant effects were observed for key concepts (*t*(79) = 1.49, $p = .141$, $d = 0.332$) or hypothesis testing (*t*(79) = 0.150, $p = .881$, $d = 0.070$). Thus, results suggest expecting to teach increased declarative knowledge use during posttests, but not hypothesis testing.

Mediation of Low Pressure – High Pressure Posttest Performance Change by Free Recall

Thus far, results have generally shown that participants who expected to teach exhibited superior motor learning, but that this benefit is eliminated under high pressure, because these participants choked under pressure. Additionally, results have demonstrated that these participants self-reported more declarative knowledge about the task. However, it is unknown whether the amount of declarative knowledge explains the choking effect exhibited by participants who expected to teach. To address this question, we conducted a mediation analysis using linear regressions (Barron & Kenny, 1986). Specifically, we considered group as the independent variable, all concepts (the free recall variable that differed between groups) as the mediator variable, and the low pressure – high pressure posttest difference for RE (ΔRE) as the dependent variable.[11] (Lower ΔRE indicates great choking under pressure.) Figure 2 depicts the mediation. First, group (coded as test = 0 and teach = 1) was shown to predict ΔRE (Path C: $\beta_{\text{unstandardized}}$ = -7.79 cm, *p* = .040). Next, group was shown to predict all concepts (Path A: $\beta_{\text{unstandardized}}$ = 1.26, *p* = .012). However, all concepts failed to predict ΔRE (Path B: $\beta_{\text{unstandardized}}$ = 0.640 cm, *p* = .453), and group still predicted ΔRE when adding all concepts to the regression (Path C': $\beta_{\text{unstandardized}}$ = -8.85 cm, *p* = .026). This result suggests declarative knowledge use, as measured by free recall of all concepts, does not explain the choking effect exhibited by participants who expected to teach.

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[11] A similar mediation was conducted with ΔBVE serving as the dependent variable. This analysis and result can be found in the appendix of this dissertation.

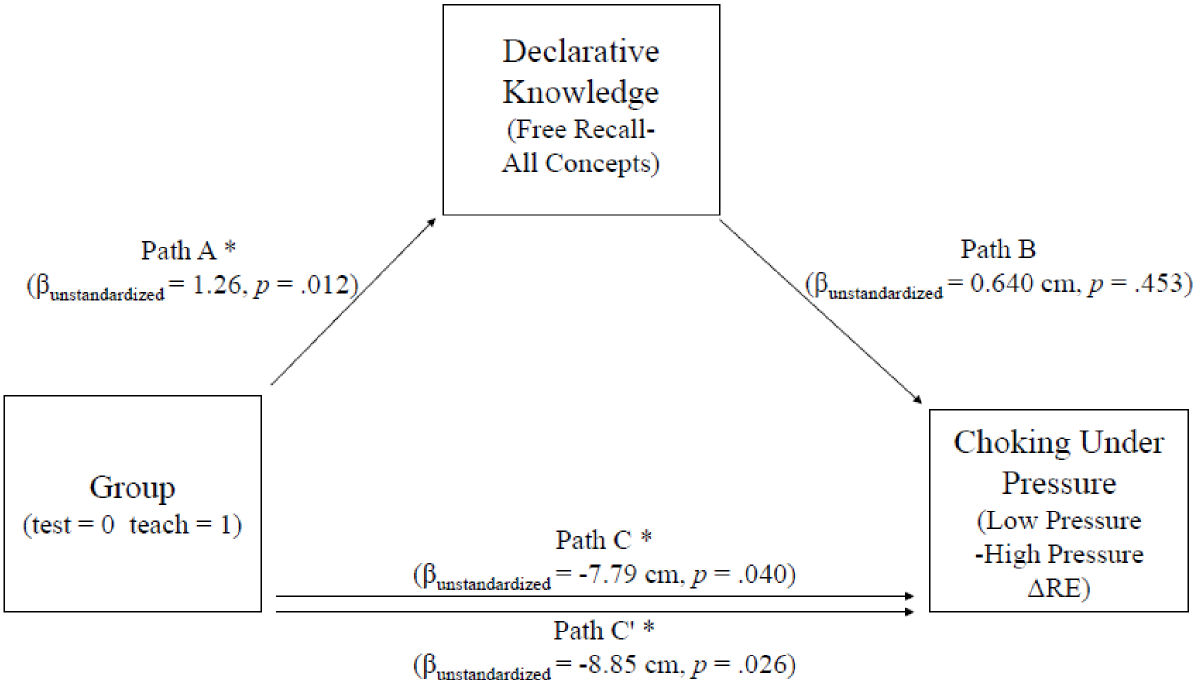

Figure 2. Mediation model testing whether the teach group's greater declarative knowledge use (Path A), as measured by all concepts recalled, explains their increased choking under pressure (Path C), reflected by greater change from low pressure to high pressure posttest accuracy. Declarative knowledge use did not explain the relationship between group assignment (teach/test) and choking under pressure. Specifically, declarative knowledge use did not predict choking under pressure (Path B), and group assignment still predicted choking under pressure, even when accounting for declarative knowledge use (Path C').

Discussion

The aim of the present study was to investigate whether the learning benefit of expecting to teach is eliminated when the acquired skill is performed under high pressure. As predicted, teach group participants generally exhibited superior accuracy and precision on a low pressure posttest relative to test group participants, but this group difference was not present on a high pressure posttest. Importantly and as predicted, the failure of the teach group to outperform the test group under high pressure was due to the teach group’s accuracy and precision significantly decreasing from low pressure to high pressure posttest, which did not occur for the test group. That is, the teach group choked under pressure, but the test group did not. It was predicted that the cause of the choking effect would be the teach group’s greater use of declarative knowledge than the test group, but results do not support this hypothesis. In particular, although the teach group did recall using more skill concepts while performing posttests, the number of skill concepts did not predict choking under pressure (decreases in accuracy and precision from low pressure to high pressure posttest).

Present results are consistent with prior experiments revealing that expecting to teach enhances motor learning in comparison to expecting to test (Daou, Buchanan et al., 2016; Daou, Lohse et al., 2016; Daou et al., 2018). Specifically, expecting to teach improved skill accuracy and precision, suggesting enhanced procedural knowledge. Thus, the present experiments adds to the growing body of evidence that practicing and studying a motor skill with the expectation of teaching it enhances learning. However, it is important to note that past experiments assessed golf putting like the present study. Therefore, future investigations may attempt to replicate the expecting to teach effect with other discrete aiming skills or more dissimilar skills (e.g., continuous skills, such as swimming).

Previous experiments have revealed participants who expect to teach can recall more skill concepts than participants who expect to test, indicating expecting to teach enhances declarative knowledge (Daou, Buchanan et al., 2016; Daou, Lohse et al., 2016; Daou, Lohse et al., 2018). Notably, Reinvestment Theory posits that learners who have more declarative knowledge are at increased risk of choking under pressure, because they may reinvest their knowledge in motor control (Masters & Maxwell, 2008). Thus, present results showing participants who expected to teach reported using more rules, methods, and/or techniques during posttests and choked under pressure are consistent with prior expecting to teach experiments and Reinvestment Theory. However, the result that expecting to teach increased the use of declarative knowledge coincident to choking under pressure does not demonstrate the increase in knowledge use caused choking. Indeed, declarative knowledge use, as measured by free recall of concepts used for putting, did not predict choking under pressure. Thus, it is possible there is an alternative explanation for why expecting to teach caused choking, but such an explanation is unapparent to us. Rather, it is possible free recall is an imprecise measure of declarative knowledge use and, therefore, may poorly predict choking. Specifically, measuring declarative knowledge use with free recall assumes participants are aware of their thoughts during performance, which may be a poor assumption given that individuals may mind-wander during motor performance (Kam et al., 2012). Indeed, it is notable that Reinvestment Theory experiments report increases in declarative knowledge use coincident to choking, but have not shown knowledge use predicts choking (Hardy et al., 1996; Koedijker et al., 2007; Lam et al., 2009a, 2009b; Liao & Masters, 2001; Masters, 1992). To improve the precision of measuring declarative knowledge use during performance, researchers may employ ‘online’ measurement techniques, such as EEG measures of neural activation in verbal-analytic brain regions and networking between these regions and motor planning regions (e.g., Buszard, Farrow, Zhu, & Masters, 2016; Deeny, Hillman, Janelle, & Hatfield, 2003; Dyke et al., 2014; Zhu et al., 2010; Zhu, Poolton, Wilson, Hu et al., 2011; Zhu et al., 2011).

Besides using an imprecise measure of declarative knowledge use, other limitations of the present experiment should be noted. First, although results concerning the effect of expecting to teach on motor learning (e.g., accuracy and precision for the low pressure posttest) were moderately strong in terms of effect size and *p*-value, results concerning the effects of expecting to teach on choking (e.g., Group x Posttest interaction) tended to be weak. This weak effect could be a result of participants using declarative knowledge in the low pressure posttest, which could be expected given that they were generally in the early stages of learning (see Table 1 for descriptive statistics of putting experience), wherein individuals rely upon declarative knowledge (Fitts & Posner, 1967). Since participants may have already been using declarative knowledge in the low pressure posttest, they would exhibit somewhat of a ‘ceiling effect’ in their declarative knowledge use in the high pressure posttest, thus limiting their reinvestment of declarative knowledge and, consequently, minimizing their choking. Future research may investigate whether the effect of expecting to teach on choking under pressure is stronger for more advanced learners, who presumably tend to rely less on declarative knowledge for performance and, thus, have a higher ceiling for reinvestment of declarative knowledge and, consequently, choking under pressure. Notably, accounting for participants' tendencies to reinvest in their movement (MSRS scores) strengthened the effects of expecting to teach on choking (and motor learning). Further, participants with high MSRS scores exhibited worse accuracy and precision relative to their low MSRS counterparts. Thus, the present results highlight the value of employing the MSRS to reduce between-subjects variability in motor learning and performance research. Unfortunately, due to problems with OSPAN data collection, we were unable to account for working memory capacity, which could have further accounted for between-subjects variability in the effects of expecting to teach on choking (Beilock & Carr, 2005; Wood et al., 2016).

Determining practical ways to enhance people's learning while they study and practice motor skills is crucial to improving behavior. Present results add to a growing body of evidence that having learners study or practice with the expectation of teaching is one means to improvement. Although present results suggest the enhancement imparted by expecting to teach may be eliminated when a learner is required to perform under high psychological pressure, it is crucial to note that present results suggest expecting to teach does *not* cause learners to perform worse under high pressure than learners who studied and practiced with the expectation of testing. Rather, high pressure brought learners who expected to teach back to the level of those who expected to test. Thus, having learners study and practice with the expectation of teaching is still preferable over having them study and practice with the expectation of testing, especially considering the strong and reliable effect of the expecting to teach effect on motor learning in comparison to the weak effect of expecting to teach on choking. However, future research should explore means by which the expecting to teach benefit can be preserved when learners must perform under high pressure. Present results are inconclusive as to whether the elimination of the expecting to teach advantage was caused by relatively large accruals of declarative knowledge. Even so, it is advisable to investigate whether having learners study and practice in ways likely to promote implicit learning (minimize gains in declarative knowledge) while expecting to teach can prevent choking under pressure by learners who study and practice with the expectation of teaching. There are multiple techniques to encourage implicit learning, including providing instructions in the form of analogies (Lam et al., 2009a, 2009b; Liao & Masters, 2001; Masters, Poolton, Maxwell, & Raab, 2008) and minimizing errors during practice (Orrell, Eves, & Masters, 2006; Poolton, Masters, & Maxwell, 2005; Zhu et al.., 2011), and these techniques should be considered in conjunction with having learners expect to teach.

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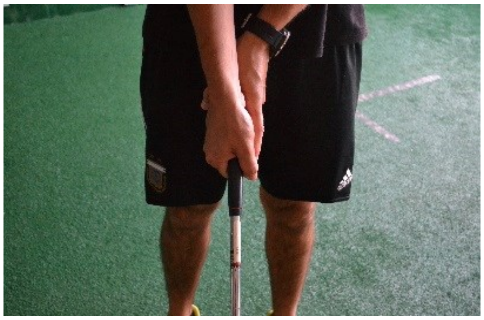
Appendix 1 – Golf putt Instruction booklet

Putting Directions

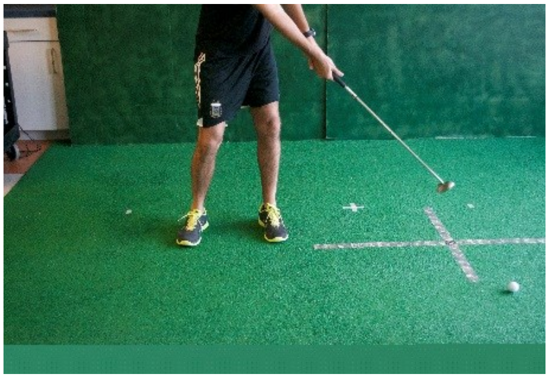
Start by taking your grip. Place both hands on the grip, right hand below left. Allow the grip to rest where the fingers meet palm. Extend the index finger of the left hand down and rest it on top of the right fingers.



Place the putter head behind the ball. Now take your stance. Start with your feet together. Now, position the feet hip width apart.

Now, hinge at the hips and bend the knees until you feel like your eyes are directly over the ball. Your elbows should be hugging your sides.

Keeping the lower body still and without breaking the wrists, stroke the ball in a straight-back, straight through motion.



Appendix 2 – MSRS and BVE exploratory analysis (chapter five)

MSRS and BVE

We conducted an exploratory 2 (MSRS: low-reinvestor/high-reinvestor) x 2 (Group) x 2 (Posttest) ANCOVA for BVE, with pretest BVE serving as the covariate. Results failed to reveal a significant MSRS x Group ($F(1, 70) = 0.711$, $p = .402$, $\eta^2_p = .010$), MSRS x Posttest ($F(1, 70) = 0.030$, $p = .862$, $\eta^2_p < .001$), or MSRS x Group x Posttest interaction ($F(1, 70) = 0.475$, $p = .493$, $\eta^2_p = .007$). Notably, there was a main effect of MSRS ($F(1, 70) = 4.34$, $p = .041$, $\eta^2_p = .058$), with low-reinvestors exhibiting lower BVE ($M_{\text{adjusted}} = 44.0$ cm, 95% $CI = 39.2$ cm – 48.8 cm) than high-reinvestors ($M_{\text{adjusted}} = 51.0$ cm, 95% $CI = 46.4$ cm – 55.6 cm). Further, the main effect of group ($F(1, 70) = 5.12$, $p = .027$, $\eta^2_p = .068$) and the Group x Posttest interaction ($F(1, 70) = 2.83$, $p = .097$, $\eta^2_p = .039$) became stronger when accounting for MSRS score. Thus, an individual’s tendency to reinvest does not moderate the effect of expecting to teach on motor learning or choking under pressure, as measured by precision. However, accounting for an individual’s tendency to reinvest does strengthen these effects, as one’s tendency to reinvest explains individual differences in precision.

Mediation for BVE

We considered group as the independent variable, all concepts (the free recall variable that differed between groups) as the mediator variable, and the low pressure – high pressure posttest difference for BVE (ΔBVE) as the dependent variable. (Lower ΔBVE indicates great choking under pressure.) Figure S1 depicts the mediation. First, group (coded as test = 0 and teach = 1) did not predict ΔBVE (Path C: $\beta_{\text{unstandardized}}$ = -7.35 cm, *p* = .130). Next, group was shown to predict all concepts (Path A: $\beta_{\text{unstandardized}}$ = 1.26, *p* = .012). However, all concepts failed to predict ΔBVE (Path B: $\beta_{\text{unstandardized}}$ = 0.771 cm, *p* = .478). Finally, group became a stronger, albeit still nonsignificant, predictor of ΔBVE when adding all concepts to the regression (Path C': $\beta_{\text{unstandardized}}$ = -8.63 cm, *p* = .091).

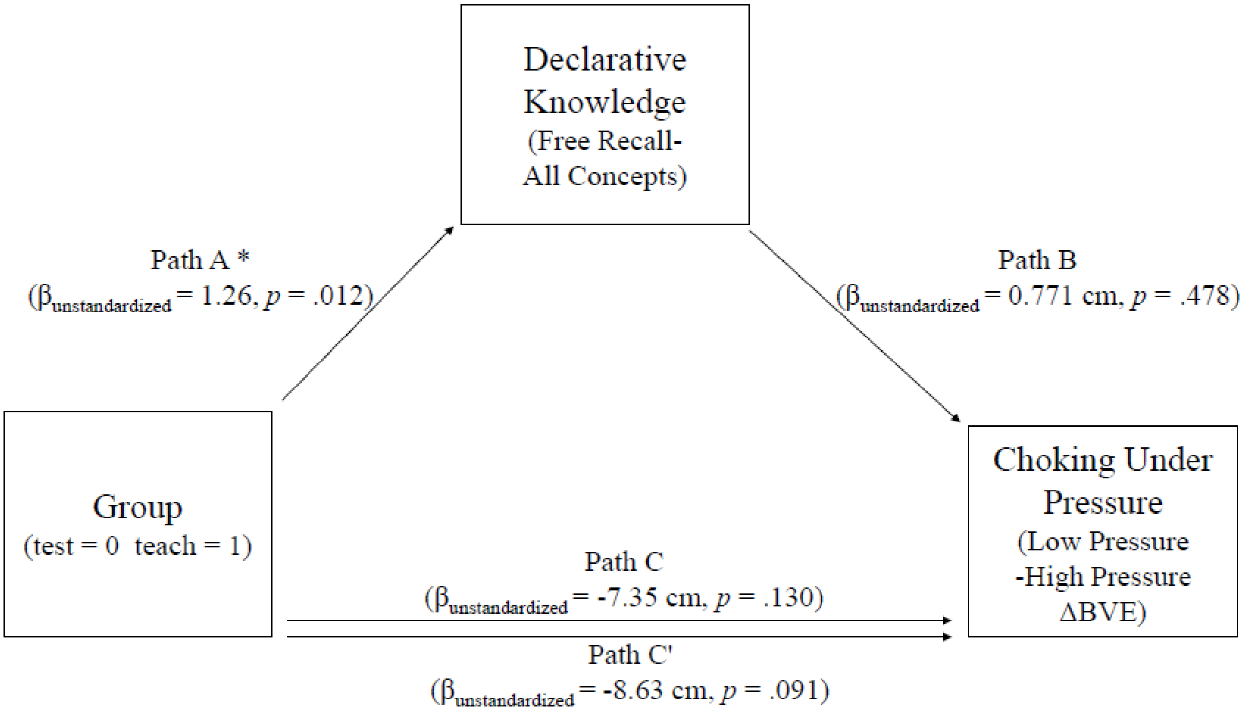


Figure S1. Mediation model testing whether the teach group's greater declarative knowledge use (Path A), as measured by all concepts recalled, explains the relationship between group assignment (teach/test) and choking under pressure (Path C), reflected by change from low pressure to high pressure posttest precision. (Note that group assignment did not predict choking under pressure.) Declarative knowledge use did not predict choking under pressure (Path B), and group assignment became a stronger, albeit still nonsignificant, predictor of choking under pressure, even when accounting for declarative knowledge use (Path C').