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Does learning a skill with the expectation of teaching it impair the skill's execution under psychological pressure if the skill is learned with analogy instructions?

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ABSTRACT

Objective: Having learners practice a motor skill with the expectation of teaching it (versus an expectation of being tested on it) has been revealed to enhance skill learning. However, this improvement in skill performance is lost when the skill must be performed under psychological pressure due to 'choking under pressure.' The present study investigated whether this choking effect is caused by an accrual of declarative knowledge during skill practice and could be prevented if a technique (analogy instructions) to minimize the accrual of declarative knowledge during practice is employed.
Design: We used a 2 (Expectation: teach/test) x 2 (Instructions: analogy/explicit) x 2 (Posttest: low-pressure/high-pressure) mixed-factor design, with repeated measures on the last factor.
Methods: One-hundred fifty-six participants were quasi-randomly assigned (based on sex) to one of four groups. Participants in the teach/analogy and teach/explicit groups practiced golf putting with the expectation of teaching putting to another participant, and analogy instructions or explicit instructions, respectively. Participants in the test/analogy and test/explicit groups practiced golf putting with the expectation of being tested on their putting, and analogy instructions or explicit instructions, respectively. The next day all participants completed low- and high-pressure putting posttests, with their putting accuracy serving as the dependent variable.
Results: We observed an Expectation x Instructions × Posttest interaction, such that a main effect of expectation was found in the low-pressure posttest, with the teach group exhibiting superior accuracy, and an Expectation × Instructions interaction was revealed for the high-pressure posttest. This interaction resulted from the teach group showing greater accuracy than the test group exclusively when receiving analogy instructions.
Conclusion: Results show that participants who practiced with the expectation of teaching exhibited superior learning and indicate that they choked under pressure likely due to their accrual of declarative knowledge during practice, since the choking effect was prevented by having them practice with analogy instructions. Accordingly, having learners practice with the expectation of teaching and techniques that minimize the accrual of declarative knowledge is recommended.

1. Introduction

Determining practice conditions that enhance motor learning is important to facilitate motor behavior. The value of practice conditions that enhance motor learning depends on whether the learning benefits are transferred to novel contexts (Schmidt & Lee, 2019), particularly

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those likely to be encountered while performing the skill and those with high importance. For example, a practice condition may improve a learner's encoding and consolidation of a skill, however the practice condition's efficacy is limited if the skill cannot be successfully retrieved and performed in high-stakes environments, under psychological pressure. As many skills must be performed in high-stakes environments, such as sports competition, it is crucial to determine practice conditions that enhance learning and preserve learning benefits under psychological pressure. Recently, Daou, Hutchison, et al. (2019) revealed that practicing a motor skill with the expectation of teaching it to another person loses its benefit when the learned skill is performed under psychological pressure. Therefore, the purpose of the present study was to determine whether the expecting to teach approach can be modified to preserve its learning advantage, and, in so doing, shed light on the mechanisms underlying the loss of the benefit under psychological pressure.

Some initial research of the expecting to teach approach showed 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 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). Daou, Buchanan, Lindsey, Lohse, and Miller (2016) conducted the first investigation into whether expecting to teach enhances learning of motor skills, which rely more heavily on procedural knowledge than academic information does (Rosenbaum, Carlson, & Gilmore, 2001). Daou, Buchanan et al. observed having learners practice and study a motor skill with the expectation of teaching it to another person enhanced skill learning in comparison to having learners practice and study a skill with the expectation of being tested, and this effect has been replicated several times (Daou, Hutchison, et al., 2019; Daou, Lohse, & Miller, 2016; Daou, Lohse, & Miller, 2018; Daou, Rhoads, Jacobs, Lohse, & Miller, 2019). Although research has failed to reveal the mechanisms underlying the learning benefit of expecting to teach, studies have consistently shown that the learning advantage occurs concomitant to large gains in declarative knowledge about the learned skill (Daou et al., 2018; Daou, Buchanan, et al., 2016; Daou, Hutchison, et al., 2019; Daou, Lohse, & Miller, 2016; Daou, Rhoads, et al., 2019). As motor skills acquired with large gains in declarative knowledge are highly susceptible to decrement under psychological pressure (Lam, Maxwell, & Masters, 2009a, 2009b; Hardy, Mullen, & Jones, 1996; Koedijker, Oudejans, & Beek, 2007; Liao & Masters, 2001; Masters, 1992), it was unsurprising that Daou, Hutchison, et al. (2019) revealed that the expecting to teach benefit vanished under psychological pressure, due to participants who practiced with the expectation of teaching 'choking' in a high-pressure posttest. Daou, Hutchison et al. concluded that participants who practiced with the expectation of teaching choked likely due to their accrual of declarative knowledge while practicing, however the authors were unable to provide evidence to support this conclusion. Nonetheless, their conclusion is consistent with reinvestment theory (Masters & Maxwell, 2008), which 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, ironically, impairs performance (Wulf, 2013). Critically, 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.

A corollary of reinvestment theory is that motor skills learned relatively implicitly, with minimal gains in declarative knowledge, should be resilient to psychological pressure (Masters & Maxwell, 2008), and research generally supports this proposition (Hardy et al., 1996; Koedijker et al., 2007; Lam et al., 2009a, 2009b; Liao & Masters, 2001; Masters, 1992). An effective strategy to encourage implicit motor learning is to provide learners with an analogy about how to perform the skill rather than explicit rules, strategies, and techniques regarding skill performance (Lam et al., 2009a, 2009b; Liao & Masters, 2001). With an analogy, declarative knowledge about multiple rules is reduced into a single, comprehensive rule. For example, Lam et al. (2009a) instructed participants in an analogy practice condition to "shoot as if you are trying to put cookies into a cookie jar on a high shelf" (p.344) while practicing a basketball free throw, whereas participants in an explicit practice condition group were instructed to follow a list of eight specific rules while practicing. Participants in both conditions performed low- and high-pressure posttests and were asked to recall free-throw shooting rules. Participants who practiced in the analogy condition reported fewer rules, indicative of more implicit learning, and performed equally well under low- and high-pressure posttests, whereas the explicit condition group performed worse under the high-pressure than the low-pressure posttest (i.e., they choked under pressure).

Since the expecting to teach approach is a practical way to enhance motor learning, it would be beneficial to determine a way to maintain the learning advantage under psychological pressure. As Daou, Hutchison, et al. (2019) attributed the choking effect exhibited by participants who practiced with the expectation of teaching to the accrual of declarative knowledge, a promising means to prevent the choking effect is to promote implicit learning by instructing learners to use an analogy to practice a motor skill rather than a list of rules. Indeed, Daou, Hutchison et al. asked participants to study an instruction booklet containing a list of rules to follow while practicing the skill, likely prompting learners who expected to teach to attend to the rules so that they could disseminate them to another person; an analogy instruction would reduce this attention to rules. Importantly, it is unlikely that minimizing the accrual of declarative knowledge by learners who expect to teach will reduce their learning advantage, as declarative knowledge has been found to not significantly relate to motor learning in an expecting to teach paradigm (Daou, Buchanan, et al., 2016). Even with analogy instructions, it is possible that learners who expect to teach could accrue greater declarative knowledge by engaging in more learning activities, such as discovery learning and hypothesis testing, than those who expect to test. Nonetheless, the practical question of whether the choking effect associated with the expecting to teach approach is prevented by using analogy instructions can still be answered.

The present study investigated whether having learners practice a motor skill with the expectation of teaching it and using an analogy to practice it preserves the learning advantage of expecting to teach under psychological pressure. Specifically, participants were assigned to four groups. One group practiced with the expectation of teaching a motor skill and received an analogy instruction (teach/analogy); one group practiced with the expectation of teaching the skill and received specific explicit rules related to the skill (teach/explicit); one group practiced with the expectation of being tested on the skill and received an analogy instruction (test/analogy); and one group practiced with the expectation of being tested on the skill and received explicit rules about the skill (test/explicit). One day after skill practice (6 blocks of 10 putts on a single day), all groups performed low- and high-pressure posttests. With this 2 (Expectation: teach/test) x 2 (Instruction: analogy/explicit) x 2 (Posttest: low-pressure/high-pressure) design, we predicted a 3-way interaction. In particular, we predicted participants in the teach groups would exhibit superior posttest performance on the low-pressure posttest relative to their test group counterparts, but the effect of expecting to teach on performance in the high-pressure posttest would be moderated by instruction. Specifically, expecting to teach would be advantageous for participants who trained with analogy instructions, but not for participants who trained with explicit instructions. This result would indicate that practicing a motor skill with the expectation of teaching and an analogy imparts a learning advantage that can be manifested in a high-stakes environment. Crucially, this result would also strongly suggest that the reason learners who practice with the expectation of teaching choke under pressure is due to their accrual of declarative knowledge while practicing, thus addressing a shortcoming of Daou, Hutchison, et al. (2019). However, if the choking effect associated with the expecting to teach approach is not prevented by employing analogy instructions, this would not eliminate the possibility that the choking effect is caused by the accrual of declarative knowledge. Specifically, learners who practice with the expectation of teaching may accrue a relatively large amount of declarative knowledge despite receiving analogy instructions and use this knowledge during the high-pressure posttest. Importantly, we can use free recall tests of declarative knowledge use during the high-pressure posttest to shed light on this possibility.

2. Methods

2.1. Sample

Men and women between the ages of 18 and 30 years participated in the study and could receive course credit for participation. This demographic was convenient to the investigators and has been used in similar past studies (e.g., Daou, Hutchison, et al., 2019). Participants must had putted (anything from playing miniature golf to playing 18 holes on a standard golf course) between one and thirty times in their lifetime and not more than twenty times in the past year. Participants with this amount of experience were most sensitive to the expecting to teach and pressure manipulations in past experiments (Daou, Hutchison, et al., 2019). Since these participants had at least minimal putting experience, the instructions and practice likely afforded them an opportunity to improve their skill by internalizing the analogy/explicit rules rather than guiding them through a completely novel movement, and participants who expected to teach may especially have taken advantage of this opportunity in preparation for their teaching episode. Participants must have been free from physical illness, injury, or disability that could make putting difficult. Participants were asked to refrain from alcohol/drug consumption within 24 h of both days of the study, caffeine consumption within 3 h of both days of the study, and to get a good amount of sleep the night before each day of the study while also trying to get the same amount of sleep each night.

2.2. Sample size calculation

Since our study was novel, we were unable to estimate the effect size for an Expectation x Instructions × Posttest interaction. Thus, we powered our study to detect an Expectation × Posttest interaction, which we estimated to be medium ($\eta_p^2 = .093$) in our sample, based on the effect size observed in our past research among participants who met the inclusion and outlier criteria for the present study (Daou, Hutchison, et al., 2019). We powered the study to detect this interaction because the given instructions to the participants should moderate it to a relatively large degree, based on past research investigating the effects of instructions on posttest performance (Lam et al., 2009a; Liao & Masters, 2001). To do the power analyses, we used G*Power 3.1.9.2 (Faul, Erdfelder, Lang, & Buchner, 2007) and entered the aforementioned effect size (as in SPSS) along with the following parameters: $\alpha = 0.05$, power = .9, number of groups = 4, number of measurements = 2, and nonsphericity correction $\epsilon = 1$ into an ANOVA with repeated-measures testing for a within-between interaction. The required sample size was determined to be 148, but we decided to collect data from 164 participants to account for data loss (e.g., participant dropout, problems with data entry). We determined the final sample submitted to statistical analysis would include at least 148 participants, and we would ensure equal n in each group (by recruiting additional participants, if necessary). In terms of data exclusion, we decided to only exclude participants if there was a technical error in recording their putts at the posttest or if one of their average low- or high-pressure posttest radial error values had a z-score >3.00. In the latter case, we decided to report the primary statistical results with and without the inclusion of the participant.

2.3. Task

All participants used a standard (88.9 cm) 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 (Daou, Hutchison, et al., 2019). Participants' objective was to make the ball stop as close to the center of the target as possible.

2.4. Procedure

All participants completed the experiment individually. After consenting to the experiment, participants completed a demographics questionnaire asking their sex, age, putting experience, any illness, injury, or disability that could make putting difficult, whether they consumed alcohol/drugs within the last 24 h, whether they consumed caffeine within the last 3 h, and how long they slept the previous night. (All questionnaires can be found on the Open Science Framework (https://osf.io/vpr92/), see Questionnaires and Instructions component.) Once the experimenter confirmed that the participants met the inclusion criteria (see Sample section), participants completed the Movement Specific Reinvestment Scale (Masters, Eves, & Maxwell, 2005). The Movement Specific Reinvestment Scale is frequently used to examine individual tendencies to reinvest in motor control (Huffman, Horslen, Carpenter, & Adkin, 2009; Kal et al., 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 Movement Specific Reinvestment Scale 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 responded on a 6-point scale anchored by "strongly disagree" and "strongly agree". The Movement Specific Reinvestment Scale data were collected because it was possible that they would be used to explore whether individual tendencies toward reinvestment explained residual variance in the model, thus increasing the amount of variance explained by the other factors in the model, as was the case in Daou, Hutchison, et al. (2019). Next, participants put a physiological monitoring device around their chest (BioHarness 3.0, Zephyr Technology, Annapolis, MD) to get used to wearing it, which they were asked to do the following day as well. Physiological data such as heart rate and heart rate variability were collected because it was possible that they would be extracted from the device for supplemental/exploratory analyses.

2.4.1. Pretest

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

2.4.2. Practice

After pretest, participants were quasi-randomly assigned (based on sex) to the teach/analogy, teach/explicit, test/analogy, or test/explicit groups, and the corresponding expectation manipulation occurred. Participants in the teach groups 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, the instruction manipulation occurred. Participants in the analogy groups read the following: "Keep your body still like a grandfather clock and use your arms the same way that the pendulum of the clock operates. (A pendulum is a weight hung from a fixed point so that it can swing freely backward and forward. [See diagram on the right].)" (Vine et al., 2013). Participants in the explicit groups read the following:

1. “Take your stance with your legs shoulder-width apart.
2. Set your position so that your head is directly above the ball looking down.
3. Keep your clubhead square to the ball.
4. Allow your arms and shoulders to remain loose.
5. In the putting action, your arms should swing freely backward and forward from your body, which should be still. Make sure that you accelerate through the ball.
6. After contact, follow through but keep your head still and facing down” (adapted from Vine et al., 2013). (For instruction sheets participants read, see https://osf.io/vpr92/, Questionnaires and Instructions.)

Participants in all groups had 2 min to read and study the analogy or explicit instructions (Daou, Hutchison, et al., 2019). Next, participants completed the practice phase by performing 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 (Ryan, 1982). Intrinsic Motivation Inventory data were collected because it was possible that they would be used for exploratory analyses.

2.4.3. Posttests

Twenty-two to 26 h after completing pretest and practice, participants returned to complete the experiment. Participants responded to the demographics questionnaire questions about drug/alcohol use, caffeine use, and previous night sleep. 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.” Then, participants put on the physiological recording device. 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 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 affixed an iPad to the edge of a 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. Our pressure manipulation involved two types of pressure revealed to elicit choking in previous studies: performance-contingent outcomes and monitoring by others (e.g., DeCaro, Thomas, Albert, & Beilock, 2011).

After each posttest, participants completed the Revised Competitive State Anxiety Inventory-2 (Cox, Martens, & Russell, 2003) to determine manipulation efficacy. The Revised Competitive State Anxiety Inventory-2 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 completed 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 corresponds 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 recall using to putt during the *high*-pressure posttest. This type of free recall test is frequently used to assess declarative knowledge in motor skill studies (Daou, Buchanan, et al., 2016; Daou et al., 2018; Daou, Lohse, & Miller, 2016; Maxwell, Masters, & Eves, 2000; Maxwell, Masters, Kerr, & Weedon, 2001; Zhu, Poolton, Wilson, Maxwell, & Masters, 2011).

2.5. Data processing

2.5.1. Putting

Putts were recorded with an iPad mounted to the ceiling above the target cross. We measured the ball’s location relative to the target using a custom-developed program written in the National Instruments LabVIEW graphical programming language by Neumann and Thomas (2008). Putting accuracy was indexed by recording radial error as recommended by Hancock, Butler, and Fischman (1995): *Radial Error* $= (x^2 + y^2)^{1/2}$, 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* $= \left\{ \left(\frac{1}{k}\right) \sum_{i=1}^{k} \left[(x_i - x_c)^2 + (y_i - y_c)^2 \right] \right\}^{1/2}$, where k = trials in a block and $_c$ = centroid along the given axis (x or y) for that block. Radial error and bivariate variable error were calculated over pretest (10 putts) with the possibility that they could be used as covariates in exploratory analyses. Crucially, we did *not* plan to use either as an a priori covariate. Radial error and bivariate variable error were calculated for the first, third, and sixth blocks of the practice phase to get a glimpse into improvement during performance without overly slowing data processing. To assess motor learning and choking under pressure, radial error and bivariate variable error were calculated for the low- and high-pressure posttests. (For Neumann and Thomas’s LabVIEW program to measure the distance of the ball to the target, and our Python code to import these distances, calculate radial error and bivariate variable error, and create a data frame with the error scores, see https://osf.io/vpr92/, Code to Measure Putt Location and Calculate Error Scores.)

2.5.2. Self-reported anxiety

Cronbach’s α was calculated to determine the reliability of the Competitive State Anxiety Inventory-2 cognitive and somatic anxiety subscales for the low- and high-pressure posttests. If reliability was good (αs $\geq$ 0.700), then items would be averaged within the subscales. Next, a Pearson’s correlation coefficient was calculated between the cognitive and somatic anxiety subscales for each posttest, and if $r \geq 0.500$, the subscales would be averaged together for each posttest. Otherwise, the subscales would not be combined for statistical analysis.

If the subscales did not exhibit good reliability, then physiological data would serve as the primary measure of anxiety. Specifically, Bioharness data would be extracted and analyzed using Omnisense software (Zephyr Technology, Annapolis, MD). Heart rate would be averaged from the time participants were read test instructions until they completed the test for the low- and high-pressure posttest. Heart rate variability (root mean square of successive differences and high frequency [0.150–0.400 Hz]) would also be assessed for these same periods.

2.5.3. Free recall

Two indices of declarative knowledge use were extracted from participants’ responses on the free recall test. 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, 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"). That is, hypothesis testing statements were those that indicated the participant made a prediction about the relationship between their putting movement and putt outcome (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.

2.5.4. Movement Specific Reinvestment Scale

Cronbach's α was calculated to determine the reliability of the Movement Specific Reinvestment Scale. Daou, Hutchison, et al. (2019) found the Movement Specific Reinvestment Scale had good reliability when all items were considered as one scale rather than dividing the Movement Specific Reinvestment Scale into its movement self-consciousness and conscious motor processing subscales. Further, we did not expect that either subscale should account for more residual variance in our data than the other subscale. Thus, we assessed the reliability across all items and summed them into a single scale if $\alpha \geq .700$. If the Movement Specific Reinvestment Scale had an $\alpha < 0.700$, then we would not consider conducting exploratory analyses with it.

2.6. Statistical analysis

We conducted a 2 (Expectation) x 2 (Instructions) ANOVA with pretest radial error serving as the dependent variable. If the η_p^2s of the main effects or interaction ≥ 0.0099 (Richardson, 2011), then pretest putting performance would be included as a covariate in all subsequent analyses involving putting performance.

Our primary analysis of interest was a 2 (Expectation) x 2 (Instructions) x 2 (Posttest) mixed-factor ANOVA with repeated-measures on the last factor, and radial error serving as the dependent variable. This follows because radial error (accuracy) was more sensitive to the Expectation × Posttest interaction observed by Daou, Hutchison, et al. (2019) and reflects the objective of the putting task (accuracy with respect to target). Nonetheless, we conducted a secondary analysis using the same model with bivariate variable error serving as the dependent variable. We predicted an Expectation x Instructions × Posttest interaction, which would be followed up with separate 2 (Expectation) x 2 (Instructions) ANOVAs for the low- and high-pressure posttests. For the low-pressure posttest, we predicted a main effect of expectation, such that the teach groups would exhibit lower radial error (greater accuracy) than the test groups. For the high-pressure posttest, we predicted an Expectation × Instructions interaction. We would follow up this interaction with separate one-tailed *t*-tests (expectation) for the analogy and explicit groups. For the analogy groups, we predicted a significant effect of expectation, such that the teach/analogy group would exhibit lower radial error than the test/analogy group. For the explicit groups, we did not predict a significant effect of expectation. Movement Specific Reinvestment Scale score and/or pretest error scores would be considered for use as covariates/between-subjects factors in exploratory analyses of putting data.

To assess practice performance, we conducted a 2 (Expectation) x 2 (Instructions) x 3 (Block: 1/3/6) mixed-factor ANOVA with repeated-measures on the last factor separately for radial error and bivariate variable error. We predicted a main effect of block, such that participants would exhibit a linear decrease in radial error and bivariate variable error as a function of block (Daou, Hutchison, et al., 2019).

To assess anxiety, we conducted a 2 (Expectation) x 2 (Instructions) x 2 (Posttest) mixed-factor ANOVA, with repeated-measures on the last factor and the total Revised Competitive State Anxiety Inventory-2 score serving as the dependent variable. If cognitive and somatic anxiety were not strongly correlated (see Self-Reported Anxiety section), then we would conduct a MANOVA instead, with the Revised Competitive State Anxiety Inventory-2 cognitive and somatic anxiety subscales serving as dependent variables. We predicted a main effect of posttest, with higher anxiety occurring on the high-pressure posttest. If heart rate and heart rate variability needed to be used to assess anxiety, they would be submitted to a 2 (Expectation) x 2 (Instructions) x 2 (Posttest) MANOVA.

To assess declarative knowledge use during high-pressure posttest, a 2 (Expectation) x 2 (Instructions) MANOVA was conducted with all concepts and hypothesis testing free recall scores serving as the dependent variables. We predicted an Expectation × Instructions interaction for all concepts (Daou, Hutchison, et al., 2019). We would then conduct separate one-tailed *t*-tests (expectation) for the analogy and explicit groups. For the explicit groups, we predicted that teach participants would recall using more concepts than test participants. We do not predict an effect for the analogy groups.

The mixed-factor ANOVAs should be robust to violations of homogeneity of variance since we ensured equal *n*s. For the practice performance ANOVA, which has three levels of the repeated-measure, we would apply the Greenhouse-Geisser correction if sphericity was violated. Although ANOVAs should be robust to violations of normality, the one-tailed *t*-tests that follow them may not be (Field, Miles, & Field, 2012). Thus, we tested for violations of normality using the Shapiro-Wilk test and Q-Q plots, both of which we would consider when determining whether the data were non-normal. Since data from similar past research (Daou, Hutchison, et al., 2019) suggests a positive skew is possible, a natural log transformation would be applied to data exhibiting a non-normal distribution. To test MANOVA assumptions, a Box test was used to assess homogeneity of covariance matrices, and a Shapiro test was used to determine multivariate normality. If these tests failed, then multiple ANOVAs would be conducted instead of MANOVAs.

We would conduct sensitivity analyses excluding participants who consumed alcohol/drugs within 24 h of the first or second day of the experiment, caffeine within 3 h of the first or second day of the experiment, or reported differences in sleep duration >2 h between the night before the first and second day of the experiment. If the statistical significance of results of the primary analysis did not change when excluding these participants, then they would remain in the dataset. If the statistical significance of results of the primary analysis did change when excluding these participants, then they will be removed, and we would recruit additional participants to ensure $N = 148$ with equal *n*s per group.

The Auburn University Institutional Review Board approved the study (Protocol #20–519 EP 2011) on December 12, 2020. Data collection began on March 8, 2021 and finished on April 14, 2022.

3. Results

3.1. Descriptive data

We collected data from 164 participants. However, 11 were excluded due to dropping out ($n = 3$), playing golf more than 30 times in their lifetime ($n = 3$), having missing posttest performance data ($n = 1$), showing up for posttests more than 26 h after pretest and practice ($n =$

2), or not being able to complete data collection (n = 2). We then collected data from 3 extra participants to ensure equal sample sizes per group, resulting in 39 participants per group and a total sample size of 156 participants. Data for statistical analysis and trial-by-trial putting data[2] can be found at https://osf.io/vpr92/, Data. R Markdown files showing all statistical analyses and results can be found at https://osf.io/vpr92/, R Markdown files for Statistical Analyses and Results. After computing z-scores for low- and high-pressure posttests, four participants were identified as outliers and excluded. Thus, our results are based on analyses of 152 participants. Results of the primary statistical analyses with the four outliers included can be found at https://osf.io/vpr92/, Supplemental Results. Table 1 shows age, sex, and putting experience data by group.

3.2. Putting accuracy

3.2.1. Posttests

Although the Shapiro-Wilk tests for low- and high-pressure posttest radial error data were significant (low-pressure posttest: $W(152) = 0.96$, $p < .001$; high-pressure posttest: $W(152) = 0.97$, $p = .006$), the Q-Q plots were fairly linear (Figure S1). To further investigate normality, we considered skewness and kurtosis values, which were modest (low-pressure posttest: skewness = 0.83, kurtosis = 0.88; high-pressure posttest: skewness = 0.63, kurtosis = 0.81). Thus, we concluded that data did not require a transformation to approximate normality. The 2 (Expectation) x 2 (Instructions) ANOVA with pretest radial error serving as the dependent variable revealed the main effect of expectation to be $\eta_p^2 = .02$ ($p = .141$), the main effect of instructions to be $\eta_p^2 < .01$ ($p = .327$), and the Expectation × Instructions interaction to be $\eta_p^2 < .01$ ($p = .961$). Since the main effect of expectation was $\eta_p^2 \geq .0099$, we included pretest radial error as a covariate in the analyses.[3]

The primary analysis, a 2 (Expectation) x 2 (Instructions) x 2 (Posttest) mixed-factor ANCOVA with repeated-measures on the last factor, posttest radial error serving as the dependent variable, and pretest radial error serving as the covariate showed main effects of expectation ($F(1, 147) = 4.05$, $p = .046$, $\eta_p^2 = .03$) and instructions ($F(1, 147) = 6.87$, $p = .01$, $\eta_p^2 = .05$) as well as an Instructions × Posttest interaction ($F(1, 147) = 18.25$, $p < .001$, $\eta_p^2 = .11$). As predicted, these effects were superseded by an Expectation x Instructions × Posttest interaction ($F(1, 147) = 8.62$, $p = .004$, $\eta_p^2 = .06$). There were no other significant effects for this ANCOVA (posttest: $p = .493$, $\eta_p^2 < .01$; Expectation x Posttest: $p = .170$, $\eta_p^2 = .01$; Expectation x Instructions: $p = .377$, $\eta_p^2 < .01$). The subsequent Expectation x Instructions ANCOVA for the low-pressure posttest revealed the predicted main effect of expectation ($F(1, 147) = 5.72$, $p = .018$, $\eta_p^2 = .04$), such that participants who practiced with the expectation of teaching exhibited lower radial error than those who practiced with the expectation of being tested (Figure 1). There were no other significant effects for this ANCOVA (instructions: $p = .499$, $\eta_p^2 < .01$; Expectation x Instructions: $p = .247$, $\eta_p^2 < .01$). The Expectation x Instructions ANCOVA for the high-pressure posttest showed a main effect of instructions ($F(1, 147) = 23.19$, $p < .001$, $\eta_p^2 = .14$), which was superseded by the predicted Expectation × Instructions interaction ($F(1, 147) = 6.71$, $p = .011$, $\eta_p^2 = .04$). The main effect of expectation was not significant ($p = .493$, $\eta_p^2 < .01$). To continue using pretest radial error as a covariate, we employed univariate ANCOVAs (expectation), rather than the planned one-sided t-tests (expectation), for the analogy and explicit instructions groups. As predicted, results revealed that participants who practiced with the expectation of teaching and analogy instructions exhibited lower radial error than those who practiced with the expectation of being tested and analogy instructions ($F(1, 73) = 6.98$, $p = .01$, $\eta_p^2 = .09$), whereas participants who practiced with the expectation of teaching and explicit instructions did not have significantly lower radial error than those who practiced with the expectation of being tested and explicit instructions ($p = .249$, $\eta_p^2 = .02$) (Figure 1).

3.2.2. Practice

The 2 (Expectation) x 2 (Instructions) x 3 (Block) mixed-factor ANCOVA with repeated-measures on the last factor, practice radial variable error serving as the dependent variable, and pretest radial variable error serving as the covariate did not reveal significant effects (block: $p = .102$, $\eta_p^2 = .02$; expectation: $p = .195$, $\eta_p^2 = .01$; instructions: $p = .764$, $\eta_p^2 < .01$; Expectation x Block: $p = .191$, $\eta_p^2 = .01$; Instructions x Block: $p = .714$, $\eta_p^2 < .01$; Expectation x Instructions: $p = .666$, $\eta_p^2 < .01$; Expectation x Instructions x Block: $p = .107$, $\eta_p^2 = .02$).[4]

3.3. Self-reported anxiety

Cronbach's α for Competitive State Anxiety Inventory-2 cognitive and somatic anxiety subscales was 0.85 and 0.89 for low-pressure posttest, and 0.85 and 0.89 for the high-pressure posttest, respectively. Thus, we averaged items within the subscales. The correlation coefficient between the cognitive and somatic anxiety subscales for low-pressure posttest was $r = 0.63$ and for high-pressure posttest it was $r = 0.64$, so we averaged the subscales together for each posttest. As predicted, the 2 (Expectation) x 2 (Instructions) x 2 (Posttest) mixed-factor ANOVA with repeated measures on the last factor and averaged self-reported anxiety serving as the dependent variable showed a main effect of posttest ($F(1, 148) = 136.03$, $p < .001$, $\eta_p^2 = .48$), such that participants reported more anxiety during the high-pressure posttest relative to the low-pressure posttest (Table 2). No other effects were significant (expectation: $p = .589$, $\eta_p^2 < .01$; instructions: $p = .667$, $\eta_p^2 < .01$; Expectation x Instructions: $p = .983$, $\eta_p^2 < .01$; Expectation x Posttest: $p = .790$, $\eta_p^2 < .01$; Instructions x Posttest: $p = .650$, $\eta_p^2 < .01$; Expectation x Instructions x Posttest: $p = .372$, $\eta_p^2 < .01$).

[2] Please note the following about missing data. For pretest, four participants (IDs 45, 74, 114, and 142) are missing a datapoint due to a missing photograph of a putt. One participant (ID 82) shot all putts in practice block 1 out of the frame of the iPad and we did not have time to measure the putts by hand, so all datapoints for this participant for this block are missing. For practice block 1, five participants (IDs 10, 76, 98, 135, and 157) are missing a datapoint due to a missing photograph of a putt. For practice block 6, seven participants (IDs 75, 76, 98, 111, 116, 134, and 144) are missing a datapoint due to a missing photograph of a putt. For high-pressure posttest, one participant (ID 11) is missing a datapoint due to a missing photograph of a putt.

[3] In hindsight, our pre-registered plan to include pretest radial error as a covariate if the size of the group main effects or interaction exceeded a particular value was not ideal because ANCOVA assumes the covariate is independent from the treatment effect (Field et al., 2012). However, we decided to follow our pre-registered plan since pretest radial error did not significantly differ as a function of treatment. Additionally, since our main statistical analyses were ANCOVAs, we tested the homogeneity of regression slopes assumption for each model to determine whether the covariate (pretest radial error) significantly interacted with any of the independent variables (expectation, instructions, and posttest) or their interaction terms. Specifically, for each ANCOVA with posttest radial error serving as the dependent variable, we added an interaction term for pretest radial error and each other independent variable, and, when applicable, an interaction term for pretest radial error and each interaction between the other independent variables, then reran the model. Importantly, results showed that pretest radial error did not significantly interact with any of the other independent variables or their interaction terms in any of the models (ps ≥ .066, η_p^2s < .03). The analyses described above can be found in the R Markdown file (https://osf.io/vpr92/, R Markdown Files for Statistical Analyses and Results).

[4] We predicted a main effect of block on radial error during practice, but our ability to detect this effect was likely constrained by including a covariate of pretest radial error, which explained a substantial amount of variance in radial error during practice ($p < .001$, $\eta_p^2 = .21$), reducing the amount of variance that could be explained by block.

Table 1
Age, sex, and putting experience by group.

	Teach/Analogy (n = 38; 21 females)		Teach/Explicit (n = 39; 24 females)		Test/Analogy (n = 38; 21 females)		Test/Explicit (n = 37; 24 females)	
	M	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
Age (Years)	20.82	1.78	20.69	1.84	21.47	2.69	20.59	2.13
Lifetime Putting Experience[a]	1.55	0.76	1.62	0.75	1.63	0.75	1.43	0.65
Past-Year Putting Experience[a]	0.74	0.45	0.82	0.51	0.74	0.60	0.62	0.49

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

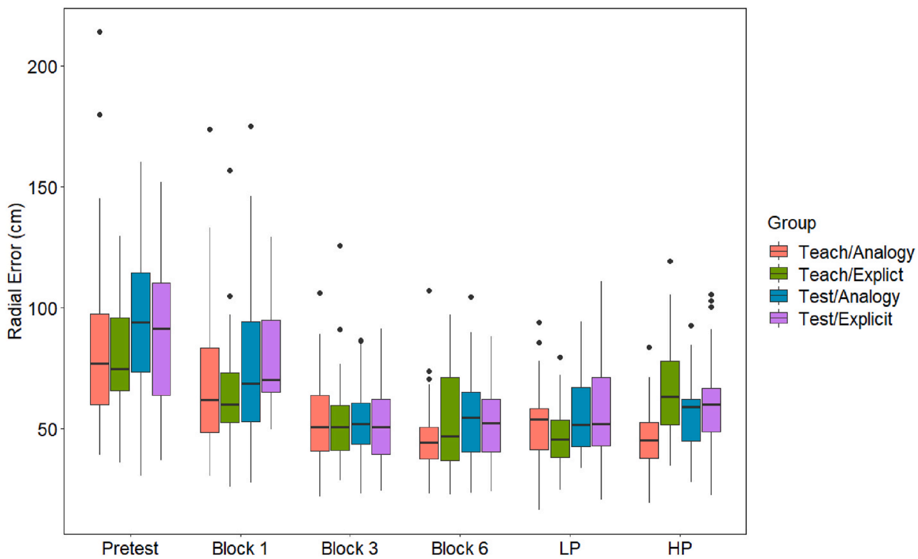

Fig. 1. Putting accuracy as a function of group and phase.
Note. Radial error (y-axis) represents putting accuracy, with lower values indicating greater accuracy. Study phase (x-axis) shows pretest, practice blocks 1, 3, and 6, as well as low-pressure (LP) and high-pressure (HP) posttests. Each rectangular box represents the interquartile range, and the horizontal line in the middle of each box identifies the median. The lines extending from each rectangular box are 1.5 x the interquartile range. Each dot represents an individual data point.

Table 2
Self-reported anxiety by posttest and group.

	Teach/Analogy		Teach/Explicit		Test/Analogy		Test/Explicit	
	M	95% *CI*	*M*	95% *CI*	*M*	95% *CI*	*M*	95% *CI*
Low-Pressure Anxiety	20.07	15.16–24.99	19.31	14.51–24.11	19.64	14.26–25.02	17.47	13.28–21.65
High-Pressure Anxiety	30.52	23.85–37.19	29.01	24.67–33.34	28.12	21.89–34.35	28.23	22.19–34.27

3.4. Free recall

Data from one participant was illegible, so we removed it from the dataset. The multivariate Shapiro-Wilk test revealed that the multivariate normality assumption was violated ($W(151) = 0.75, p < .001$), and the Box test showed that covariance matrices were not homogenous ($M(9) = 29.1$, $p < .001$). Therefore, we proceeded with two separate 2 (Expectation) x 2 (Instructions) ANOVAs with all concepts and hypothesis testing serving as dependent variables.

For all concepts, there was a main effect of instructions ($F(1, 147) = 49.89$, $p < .001$, $\eta_p^2 = .25$), such that participants who practiced with explicit instructions reported using more concepts during the high-pressure posttest than those who practiced with analogy instructions (Table 3). Contrary to predictions, no other effects were significant (expectation: $p = .404$, $\eta_p^2 < .01$; Expectation x Instructions: $p = .794$, $\eta_p^2 < .01$). For hypothesis testing, contrary to hypotheses, no effects were significant (expectation: $p = .330$, $\eta_p^2 < .01$; instructions: $p = .200$, $\eta_p^2 = .01$; Expectation x Instructions: $p = .080$, $\eta_p^2 = .02$).

3.5. Other variables collected

Secondary analyses for posttest and practice putting with bivariate variable error serving as the dependent variable can be found at https://osf.io/vpr92/, Supplemental Results. Movement Specific Reinvestment Scale data, Competitive State Anxiety Inventory-2 self-confidence subscale data for the low- and high-pressure posttests, and Intrinsic Motivation Inventory data for practice were not analyzed (other than computing Cronbach's α for the Movement Specific Reinvestment Scale, which was 0.78), but they can be found at https://osf.io/vpr92/, Data (Item-Level Questionnaire Data). Finally, we did not

Table 3
Declarative knowledge use in high-pressure posttest by group.

	Teach/Analogy		Teach/Explicit		Test/Analogy		Test/Explicit	
	M	95%*CI*	*M*	95%*CI*	*M*	95%*CI*	*M*	95%*CI*
Free Recall All Concepts	2.05	1.58–2.53	3.85	3.24–4.45	1.76	1.32–2.21	3.69	3.10–4.29
Free Recall Hypothesis Testing	0.53	0.28–0.78	0.59	0.30–0.88	0.63	0.29–0.98	0.22	0.06–0.39

extract physiological data (heart rate and heart rate variability) because Competitive State Anxiety Inventory-2 cognitive and somatic subscales exhibited good reliability. Since we did not extract physiological data, we deviated from our plan to consider replacing participants who did not follow instructions to abstain from alcohol within 24 h of the experiment, caffeine within 3 h of the experiment, and/or did not get similar amounts of sleep the night before each day of the experiment. This follows because these instructions were intended only to facilitate physiological data collection. After consultation with an editor, we decided to conduct a sensitivity analysis of the primary statistical model excluding the participants who failed to follow instructions ($n = 19$), but we decided not to recruit additional participants, even if the statistical significance of the results of the primary analysis changed when excluding these participants. (Results of the primary analysis were similar when excluding these participants, and the predicted Expectation x Instructions × Posttest interaction remained significant [$p = .026$, $\eta_p^2 = .04$]). For complete results of this sensitivity analysis, see https://osf.io/vpr92/, Supplemental Results.)

4. Discussion

Having learners practice a motor skill with the expectation of teaching it has been revealed to enhance skill learning and promote large gains in declarative knowledge (Daou, Buchanan, et al., 2016; Daou, Hutchison, et al., 2019; Daou, Lohse, & Miller, 2016; Daou et al., 2018; Daou, Rhoads, et al., 2019). However, the learning benefit vanishes when the skill is performed under pressure, likely due to the use of declarative knowledge during skill execution, although this explanation lacked strong evidence (Daou, Hutchison, et al., 2019). Therefore, the present study aimed to investigate whether this choking effect is, in fact, caused by an accrual of declarative knowledge during skill practice and whether choking is prevented if a technique to minimize the accrual of declarative knowledge during practice (analogy instruction) is applied. Our results showed that, on a low-pressure posttest, participants who practiced with the expectation of teaching exhibited greater putting accuracy than those who practiced with the expectation of being tested, but, on a high-pressure posttest, this advantage was only present for participants who also practiced with analogy instructions. Therefore, we provide evidence that the choking observed for participants who practiced with the expectation of teaching may indeed be caused by declarative knowledge use and that analogy instruction is an effective learning strategy to maintain the benefit of expecting to teach when participants are performing under pressure.

Our results are consistent with the bulk of the evidence showing that expecting to teach a motor skill while practicing it enhances skill learning (Batista et al., 2022; Daou, Buchanan, et al., 2016; Daou, Hutchison, et al., 2019; Daou, Lohse, & Miller, 2016; Daou et al., 2018; Daou, Rhoads, et al., 2019; see McKay, Hussien, Yantha, Carter, & Ste-Marie (2021) for a high-powered study that did not observe this effect on learning). Specifically, we found a main effect of expectation in both the Expectation x Instructions x Posttest ANCOVA and the Expectation x Instructions ANCOVA for the low-pressure posttest, which is the pressure condition under which previous studies have observed an expecting to teach advantage (Batista et al., 2022; Daou, Buchanan, et al., 2016; Daou, Hutchison, et al., 2019; Daou, Lohse, & Miller, 2016; Daou et al., 2018; Daou, Rhoads, et al., 2019). Although expecting to teach has frequently been shown to benefit motor learning, the mechanisms behind this effect remain unclear, despite several candidates being examined, such as motivation and pressure (Daou, Buchanan, et al., 2016; Daou, Lohse, & Miller, 2016; Daou et al., 2018; Daou, Rhoads, et al., 2019), length of motor preparation (Daou, Lohse, & Miller, 2016; Daou, Rhoads, et al., 2019), and cortical dynamics during motor preparation (Daou et al., 2018). Notably, present results suggest the mechanism underlying the expecting to teach benefit is *not* declarative knowledge, since the benefit was exhibited by participants who practiced with analogy instructions and reported minimal declarative knowledge use. Thus, the accrual of declarative knowledge is likely a byproduct of practicing with the expectation of teaching, consistent with past research reporting that it was unrelated to motor learning in an expecting to teach paradigm (Daou, Buchanan, et al., 2016). More research is warranted to elucidate the underpinnings of the expecting to teach benefit to motor learning.

An important finding from Daou, Hutchison, et al. (2019) was that the expecting to teach advantage vanished when participants were performing under pressure. Here, we replicated this result by finding an Expectation x Instructions × Posttest interaction due to a main effect of expectation during the low-pressure posttest, but not high-pressure posttest, where we found an Expectation × Instructions interaction. Daou, Hutchison et al. argued that participants who practiced with the expectation of teaching choked likely due to their accrual of declarative knowledge while practicing, although the authors failed to provide evidence to support this conclusion. Specifically, Daou, Hutchison et al. reported that declarative knowledge use during posttests did not mediate the relationship between expecting to teach and choking, possibly because declarative knowledge use was measured via free recall, which may be an imprecise measure of thoughts while putting. For example, Beilock, Wierenga, and Carr (2002) reported that trained novice golfers (like those in the present study) had only modest episodic memories of putts they executed. Here, we circumvented the problem of measuring declarative knowledge use during putting by successfully minimizing its availability in half the participants who practiced with the expectation of teaching by giving them analogy instructions (see main effect of instructions for free recall of all concepts). We reasoned that if they ceased to choke under pressure, then the choking effect could be attributed to declarative knowledge use. Results did indeed show that participants who practiced with the expectation of teaching and analogy instructions maintained their advantage under high-pressure, whereas those who practiced with explicit instructions did not. This was evidenced by an Expectation × Instructions interaction for the high-pressure posttest, caused by a main effect of expectation for participants who practiced with analogy instructions, but not for participants who practiced with explicit instructions. In sum, results strongly suggest that the reason learners who practice with the expectation of teaching choke under pressure is due to declarative knowledge use, and that limiting their accrual of declarative knowledge during practice with analogy instructions can maintain the expecting to teach benefit under pressure.

In the Expectation x Instructions x Posttest ANCOVA, we found a main effect of instructions that was superseded by an Instructions × Posttest interaction. This interaction was due to a main effect of instructions for the high-pressure posttest, such that the analogy groups presented greater accuracy, relative to the explicit groups, whereas no effect of instructions was observed for the low-pressure posttest. As analogy instructions promote implicit learning, these results are consistent with reinvestment theory (Masters & Maxwell, 2008) and a recent meta-analysis showing that implicit learning is beneficial to performance under pressure (Cabral, Wilson, & Miller, 2022). This feature of implicit learning can be attributed to the lack of declarative knowledge to reinvest in conscious control of movement (Masters & Maxwell, 2008). Although research seems to point to a benefit of implicit learning on performance under pressure, there is evidence that the benefit is overestimated due to reporting bias and underpowered studies (Cabral et al., 2022). Besides these problems in implicit learning studies, there have been critiques about how explicit learning manipulations are implemented in the studies. For example, explicit learning groups are often given a greater number of instructions and ones that are of poor quality and have different meanings than those given to implicit learning groups (Bobrownicki, Collins, Sproule, & MacPherson, 2018). Therefore, pre-registered studies with greater statistical power and careful consideration of instructions given to explicit learning groups are recommended (Bobrownicki, Carson, MacPherson, & Collins, 2021; Cabral et al., 2022).

The present study's results have a few limitations, including their generalizability. Specifically, the sample consisted of healthy, young adults, like other studies examining the effect of expecting to teach on motor learning (Batista et al., 2022; Daou, Buchanan, et al., 2016; Daou et al., 2018; Daou, Hutchison, et al., 2019; Daou, Lohse, & Miller, 2016; Daou, Rhoads, et al., 2019; McKay, Hussien, Carter, Yantha, & Ste-Marie, 2021), so it is unclear whether expecting to teach effects apply to other populations. Notably, analogy instructions have been shown to promote stable motor performance in other populations, such as older adults (Tse, Wong, & Masters, 2017). The present study's sample was also relatively novice at the motor skill, like most motor learning studies (Williams, Fawver, & Hodges, 2017), and thus whether the effects of expecting to teach and analogy instruction extend to experts is unclear. Finally, the generalizability of the expecting to teach and analogy instructions effects to motor skills other than closed ones is uncertain because most studies examining these effects have done so with skills such as golf putting, dart throwing, and basketball shooting (e.g., Batista et al., 2022; Daou, Hutchison, et al., 2019; Lam et al., 2009a). Another limitation of the present study's results is the ecological validity of the analogy instructions benefit, since the control (explicit instructions) group practiced with instructions that may be unrepresentative of those given in the real world (Bobrownicki, Carson, & Collins, 2022). Finally, participants who practiced with the expectation of teaching and analogy instructions did not choke under pressure, strongly suggesting that the choking effect for those who practiced with the expectation of teaching and explicit instructions was caused by declarative knowledge use; yet, participants who practiced with the expectation of teaching did not recall using more declarative knowledge during the high-pressure posttest. However, it is important to reiterate that free recall is likely an imprecise measure of declarative knowledge use, such that it is only sensitive enough to detect very large differences in declarative knowledge use (e.g., differences associated with analogy vs. explicit instructions). Nonetheless, the present study's design limits its ability to assess the effect of expecting to teach on declarative knowledge use, because analogy or explicit instructions were combined with this intervention.

The present study is methodologically strong, and its results can inform future research and practical applications. The methodological strength is rooted in the study being a registered report, which allowed the study design to undergo a peer review that led to high statistical power and well-planned experimental manipulations, analyses, and outcome measures. These attributes along with the open science practices (e.g., access to experimental materials, statistical processing code, and data) required for the registered report increase the positive predictive value of the study results (Ioannidis, 2005), add precision to the effect size estimates, and facilitate future research, including replications and meta-analyses. Other avenues for future research include addressing the limitations to the generalizability of the expecting to teach and analogy instructions benefits by conducting studies with different populations (including experts) and tasks. Further, researchers are encouraged to investigate other methods aiming to maintain the benefits of expecting to teach when performing under pressure. These methods could involve other implicit learning strategies (e.g., errorless learning; Zhu et al., 2011), but also techniques commonly associated with theories of choking under pressure other than reinvestment theory. For example, researchers could examine whether having participants practice a motor skill with the expectation of teaching and psychological pressure (anxiety training) prevents choking due to distraction (distraction theories; DeCaro et al., 2011) in subsequent high-pressure posttests (Low et al., 2021; Oudejans & Pijpers, 2010). Researchers could also investigate whether having participants practice with the expectation of teaching and providing an intervention at the time of a high-pressure posttest prevents choking under pressure. Such interventions could include pre-performance routines, mental imagery, self-talk, and meditation (Chen et al., 2012; Fekih et al., 2021; Mesagno, Geukes, & Larkin, 2015; Walter, Nikoleizig, & Alfermann, 2019). In practice, the present results indicate that learners should train under the expectation of teaching and analogy instructions to enhance motor learning and preserve skill performance under pressure. Instructors may also consider having learners practice with the expectation of teaching and, pending future study results, implementing strategies other than analogy instructions to prevent choking under pressure.

In conclusion, the present study indicates that learners who practice with the expectation of teaching exhibit superior learning, and when they choke under pressure it is likely due to their accrual of declarative knowledge during practice, since the choking effect is prevented by having them practice with analogy instructions. Accordingly, instructors are recommended to have learners practice with the expectation of teaching and techniques that minimize the accrual of declarative knowledge, such as analogy instructions.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

R Markdown files showing all statistical analyses and results can be found at https://osf.io/vpr92/, R Markdown files for Statistical Analyses and Results.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.psychsport.2022.102323.

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