What moves you? The Role of Enhanced Expectancies and Reward Processing in Motor Performance and Learning

by

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Abstract

This dissertation describes a research program focused on dissecting the contributions of motivation and rewards to motor skill acquisition and learning. Two prominent theories served as a framework for this work: the OPTIMAL theory of motor learning (Wulf & Lewthwaite, 2016) and reinforcement learning (RL) theory (Rescorla & Wagner, 1972). The former claims that enhancing learners' expectancies for future positive outcomes and perception of autonomy leads to higher levels of motivation, which strengthens the coupling of goals to actions, culminating in better motor performance and learning. It is hypothesized that expectancies reflect reward anticipation, which might explain the learning benefits given the association between rewards and the release of dopamine, a neurotransmitter that plays a crucial role in movement, reward processing, and memory consolidation. Rewards, and more specifically, reward-prediction errors (the difference between actual and anticipated reward) are the major driver of RL theory. According to this theory, humans adjust their behavior based on reward-prediction errors to maximize the likelihood of receiving rewards. In short, behaviors that lead to rewards are more likely to re-occur in the future, whereas behaviors that are not rewarded are less likely to re-occur in the future. Together, OPTIMAL and RL theory make predictions about how motivation and rewards affect short- and long-term behavior adaptation. Through a series of studies that combine behavioral, psychophysiological, and meta-analytical research, the present research program investigated how these predictions apply to motor skill acquisition and retention.

The first paper (chapter 1), published in the *Journal of Motor Learning and Development*, sought to tease apart the contributions of extrinsic rewards, a common means to enhance learners' expectancies, to promoting learning of two components of a motor skill, namely the action selection (i.e., what to do) and action execution component (i.e., how to execute the

action). Results showed that giving learners extrinsic rewards during practice did not improve their ability to choose the correct action and execute the movement accurately. Interestingly, learners' self-reported motivation, irrespective of whether they could receive extrinsic rewards, did predict action selection and action execution performance. The second paper (chapter 2), published in the journal *International Review of Sport and Exercise Psychology*, used a meta-analytic approach to examine the effect of enhanced expectancies on motor learning and whether the effect depended on the type of manipulation adopted. Results revealed a medium-sized, positive effect of enhanced expectancies on motor learning, which varied as a function of the type of manipulation, and is likely overestimated due to the presence of small-study effects and underpowered studies in the sample. The third paper (chapter 3), published in the journal *Psychology of Sport and Exercise*, investigated the mechanisms underlying the self-controlled feedback learning benefit. As postulated by OPTIMAL theory, increasing learners' perception of autonomy leads to higher levels of motivation and consequent better performance and learning. One common autonomy support manipulation consists of giving learners control over their feedback schedule, which has been shown to enhance motor learning, though the underlying mechanisms are still unclear. Since motivational and information processing factors have been suggested as potential underpinnings, the second paper aimed to dissociate their contribution to the self-controlled feedback learning benefit. Results showed no effect of self-controlled feedback on learning, although self-reported motivation predicted post-test performance at the individual level, irrespective of whether learners controlled their feedback schedule. Finally, the fourth paper (chapter 4) investigated RL predictions and their underlying mechanisms in a motor learning context. Specifically, mixed-effect regression models were used to analyze the relationship between learners' feedback-evoked electroencephalogram (EEG) activity (i.e., reward positivity; RewP) and their short- and long-term behavior adaptation. Results showed that RewP scaled with feedback about learners' accuracy and explained adjustments in their performance, suggesting that it reflects reward-prediction errors. Moreover, although RewP was implicated in short-term performance adjustments, it did not predict long-term behavior adaptation.

Taken together, the studies described in this dissertation provide evidence for some aspects of OPTIMAL theory, such as the relationship between learners' motivation and their motor learning, but cast doubt on others, such as the benefit of increasing learners' autonomy and their motor learning. Further, the final study provides evidence that the RewP can shed light on how RL mechanisms (reward-prediction errors) explain short-term performance adjustments when learning a complex motor skill, but indicates that other mechanisms may need to be considered to explain long-term behavior adaptation.

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Table of Contents

List of Tables

List of Figures

Chapter 1: The effect of rewards and punishments on learning action selection and execution components of a motor skill

Introduction

The processes underlying motor skill learning have long been of interest to many investigators across different disciplines. For instance, a growing body of evidence posits that motor learning may be associated with processing of sensory-prediction errors (Shadmehr et al., 2010), often referred to as model-based learning (Haith & Krakauer, 2013). According to this framework, motor learning results from movement adjustments that are made based on comparison between expected and actual sensory feedback (e.g., vision, proprioception), hence the name *sensory-prediction error.* Studies investigating sensory-prediction errors have extensively used visuomotor tasks wherein motor adaptation occurs in response to a change in the environment (i.e., perturbation) and is measured across training session with rare or no use of retention tests (Izawa & Shadmehr, 2011; Shmuelof et al., 2012; Synofzik et al., 2006, 2008).

Although current and relevant, the discussion around sensory-prediction errors is beyond the scope of the present study, which will focus on another mechanism used to explain learning: *reward-prediction errors.* This perspective, frequently coined as model-free learning (Haith & Krakauer, 2013), claims that motor learning can also be driven by rewards (Galea et al., 2015; Izawa & Shadmehr, 2011; K. Lohse et al., 2019; Nikooyan & Ahmed, 2015). This idea is grounded on reinforcement learning (Rescorla & Wagner, 1972;Sutton & Barto, 1998), a theory rooted in the seminal work by Skinner (Ferster & Skinner, 1957; Skinner, 1963) and others who followed, which posits that behaviors that are rewarded are more likely to reoccur in the future compared to behaviors that are punished. In principle, humans engage in a process of comparing expected to actual outcomes and, based on the resulting difference, make behavioral adjustments with the main goal of maximizing rewards. The discrepancy between expected and actual outcome is referred to as a prediction error, which is one of the most crucial components of reinforcement learning theory. When accurate, prediction errors or, more specifically, *reward-prediction errors* (difference between expected and actual reward), can indicate that a behavior needs to be changed in order to achieve a successful outcome. In theory, receiving rewards after a successful, unexpected outcome maximizes reward-prediction errors by making the difference between expected and actual reward larger. Since larger reward-prediction errors indicate that the outcome is either much worse or much better than predicted, making a successful outcome even more rewarding would cause that behavior to be reinforced and, therefore, more likely to reoccur in the future. The concept that learning is guided by reward-prediction errors has been mainly investigated in psychology (Rescorla & Wagner, 1972) and computer science (Sutton & Barto, 1998). However, even though reinforcement learning is also a critical component of motor skill acquisition (Lohse et al., 2019), very few studies have investigated the effects of rewards on long-term retention of a motor skill.

Findings derived from the growing body of studies investigating reinforcement learning in the context of motor skill acquisition have shown that rewards may enhance motor skill learning as indexed by post-test performance (Abe et al., 2011; Dayan et al., 2014; Hasson et al., 2015; Steel et al., 2016). For instance, Abe et al. (2011) designed a study to investigate the effects of rewards and punishments on learning an isometric pinch-force tracking task. In this study, participants were randomly assigned to one of three conditions: rewarded, punished or neutral. Participants in the rewarded condition earned money for time spent on target, whereas participants in the punished and neutral conditions either lost money for time spent off target or received a flat rate at the end of practice regardless of performance, respectively. Results showed that giving participants monetary compensation after a successful trial resulted in better performance at 6-hr, 24-hr and 30-day post-test. Moreover, even in the absence of practice, the rewarded condition showed performance improvement from immediate to 24-hr and 30-day post-test. Conversely, participants in the punished and neutral conditions showed performance decrement from immediate to 6-hr and 30-day post-test. These findings suggest that rewarding participants after a successful trial might contribute to long-term retention (consolidation) of a recently acquired motor skill through performance stabilization and offline gains (Trempe & Proteau, 2012).

From a scientific standpoint, the existing literature favors the use of rewards to enhance motor skill learning. However, when translating that scientific knowledge into a more applied setting, it is useful to know what component or components of a skill rewards and/or punishments may impact. In motor learning, performance improvement can occur through two main components: action selection and action execution. We acknowledge that there are different ways to delineate the components of a skill, and processes underlying action selection and action execution may not be entirely dissociable. However, for the purposes of this study, we will adopt action selection and action execution as being two separate processes as this distinction more closely aligns with the reality in an applied setting. Thus, consistent with Schmidt (1976), action selection refers to choosing the appropriate action based on one's perception of the environment (e.g., a pinch grip versus a power grip), whereas action execution refers to how the motor system carries out the chosen action (e.g., for force required lifting different objects with the same grip). In the majority of sports, for example, success is largely tied to the athlete's ability to choose the appropriate response and execute the chosen action accordingly. Now, picture yourself as a soccer coach trying to teach a new play to one of your athletes. If your athlete chooses the right player to whom to pass the ball but executes the movement poorly, what do you do? Do you reward her for making the correct action selection? Do you punish her for executing the movement poorly? Do you do both? Or do you do neither? Prominent sport psychology textbooks advise against giving rewards too frequently (e.g., 100% of the time; Weinberg & Gould, 2019), and reinforcement learning theory supports the advice, since the theory posits that frequent rewards become too predictable, lessening their impact on learning (Lohse et al., 2019). What little experimental evidence exists also supports this notion (Dayan et al., 2014). Since rewards should not be overused, it is useful for coaches to know how to best allocate them during practice. In fact, coaches constantly make decisions about what components of the skill to reward and/or punish during training sessions. However, these decisions are largely based on coaching manuals and lack empirical support (Chen et al., 2018).

Therefore, in an attempt to address this knowledge gap, the present study investigated whether rewards and/or punishments affect action selection and/or action execution components of a skill. Thus, we designed an experiment where these two components could be examined independently. On a trial by trial basis, participants performed an information-integration category-learning task (i.e., action selection; (DeCaro et al., 2011; Waldron & Ashby, 2001) followed by a golf putting task (i.e., action execution). For the action selection task, participants had to learn the association between eight complex stimuli to decide toward which one of two targets to execute an action. On each trial, participants were instructed to select the correct target based on a randomly selected stimulus displayed on a TV screen. After selecting the target, participants putted as accurately as possible to the chosen target (i.e., action execution task). First, we conducted a pilot study to ensure that both tasks would yield learning effects that could be posteriorly moderated by other factors. Next, we proceeded with the main experiment in which participants performed the same action selection and action execution tasks after being assigned to one of three conditions: neutral, reward and punishment. Participants in the neutral condition received a fixed number of raffle tickets while participants in the reward and punishment conditions gained or lost raffle tickets based on whether they chose the correct or incorrect target, respectively, and based on putting accuracy. Number of correct responses in the action selection task was indexed as a measure of action selection accuracy whereas radial error and bivariate variable error served as measures of putting accuracy and precision, respectively (Hancock et al., 1995).

Rewards affect motivation by increasing more extrinsic forms of motivation and decreasing or increasing intrinsic motivation, depending on various factors (e.g., interest in the task, type and timing of reward), (Cameron et al., 2001). Thus, we assessed motivation through the intrinsic motivation inventory (IMI; (McAuley et al., 1989), which assesses intrinsic motivation as well as other variables linked to motivation (e.g., effort). Specifically, we conducted secondary analyses to determine whether any form of motivation differed as a function of practice condition. Importantly, motivation, irrespective of its form (extrinsic vs. intrinsic), is theorized to explain variance in motor learning (Wulf & Lewthwaite, 2016). Thus, we also conducted an exploratory analysis to investigate whether motivation explained individual differences in motor learning, irrespective of practice condition. Based on studies investigating the effect of rewards on motor skill learning (Abe et al., 2011; Dayan et al., 2014; Steel et al., 2016), we predicted that giving participants rewards during practice would result in better learning of both action selection and action execution tasks as indexed by performance at 24 hr and 7-day post-test.

Methods

All study materials and data are available at the Open Science Framework website (https://osf.io/9ufgb/).

Pilot Study

The purpose of this experiment was to ensure that the information-integration category-learning task and the golf putting task exhibited learning effects, which could posteriorly be moderated by other factors.

Participants

Seventeen young adults (7 females, M_{age} = 21.0 years, 95% CI [20.1, 22.0]) without any previous experience with either task and who were naïve to the purpose of the study participated in this study. Four participants were excluded from the final analysis due to failure to complete the second day of data collection (n = 1) and data loss[1] (n = 3) resulting in a final N = 13 (5 females). Recruitment was done through SONA, the College of Education Research Participation System at Auburn University, and by word-of-mouth. A total of four course credits was offered in exchange for participation when applicable, in addition to 480 raffle tickets[2] to be entered into a virtual drawing for $200 USD. (A participant was indeed awarded $200 based on the raffle.) Participants reported no neuromuscular impairments that would affect the execution of a golf putting task or any difficulties in distinguishing between colors, which could affect the categorization task. All participants provided written consent to an institution-approved research protocol (18-178 EP 1806) in agreement with the 1964 Declaration of Helsinki.

Procedures

[1] iPads were used to record putting accuracy and precision during the motor task. For these participants, the iPads stopped working during data collection, which resulted in data loss.
[2] All 17 participants received the same number (i.e., 480) of raffle tickets regardless of their performance during the task.

Tasks. On each trial, participants performed an information-integration category-learning task (DeCaro et al., 2011; Waldron & Ashby, 2001) followed by a golf putting task. For the former task (i.e., action selection), participants had to learn the association between eight different complex stimuli to determine toward which one of two targets to execute an action. Each stimulus resembled a horizontally-oriented rectangular playing card presented on a white screen. The cards varied according to three binary-valued dimensions: shape of the embedded symbol (square or circle), symbol's color (green or red) and number of symbols (one or two). Originally, the card's background color (i.e., blue or yellow) was included as the fourth irrelevant dimension. After a series of pilot studies, we decided to keep the card's background color (i.e., blue) the same across all stimuli, which resulted in eight different stimuli. Each binary dimension was assigned the arbitrary value of -1 or 1 (i.e., square = 1 and circle = -1; green = 1 and red = -1; two symbols = 1 and one symbol = -1). If the sum of all three dimensions was greater than zero, participants should choose target A, whereas if the sum of all three dimensions was less than zero, participants should choose target B. Participants were not informed about the dimension values or formula that mapped the sum of the values to a target until after the study. At the beginning of each trial, a randomly selected card was displayed on a 152-cm Samsung® HDTV mounted to the wall, 193-cm above the ground and, approximately, 410-cm away from where the participant was standing. Participants were allowed to look at the card for as long as they wanted. This task was chosen because it is best acquired implicitly (DeCaro et al., 2011), which is also the contention for action execution tasks such as golf putting (Masters & Poolton, 2012). After deciding to which target the card was referring, participants were asked to putt a golf ball to the chosen target (i.e., action execution).

For the action execution task, participants used a standard right-handed golf putter to putt a standard golf ball on an artificial grass surface to one of two targets located 300-cm away from a 5-cm long, red starting line. Both targets, placed 14-cm apart, were comprised of a bull's eye with a radius of 8-cm surrounded by nine concentric circles, which had their radii progressively increased by 8-cm. The goal was to putt as accurately as possible by having the ball stop as close to the center of the target as possible.

Day 1 of data collection. The experimental set-up is illustrated in Figure 1. On day 1, after signing the consent form, participants filled out a demographic questionnaire. They were asked to report age, sex, and putting experience (e.g., miniature golf, golf simulator, and 18 holes on a standard gold course) over their lifetime and within the past year. Next, in order to measure baseline golf putting skill level, participants completed the pretest phase, which consisted of one block of eight trials. Participants were asked to putt four times in a row to one target and four times to the other target. Half of the participants putted to target A first and the other half putted to target B, in a counterbalanced order. Participants did not perform the action selection task during pretest. When ready, participants were instructed to pick up a golf ball from a basket that rested on a chair behind the participant, place the golf ball on the starting line for the respective target and putt as accurately as possible. After completing the pretest, the following instructions were read to the participants:

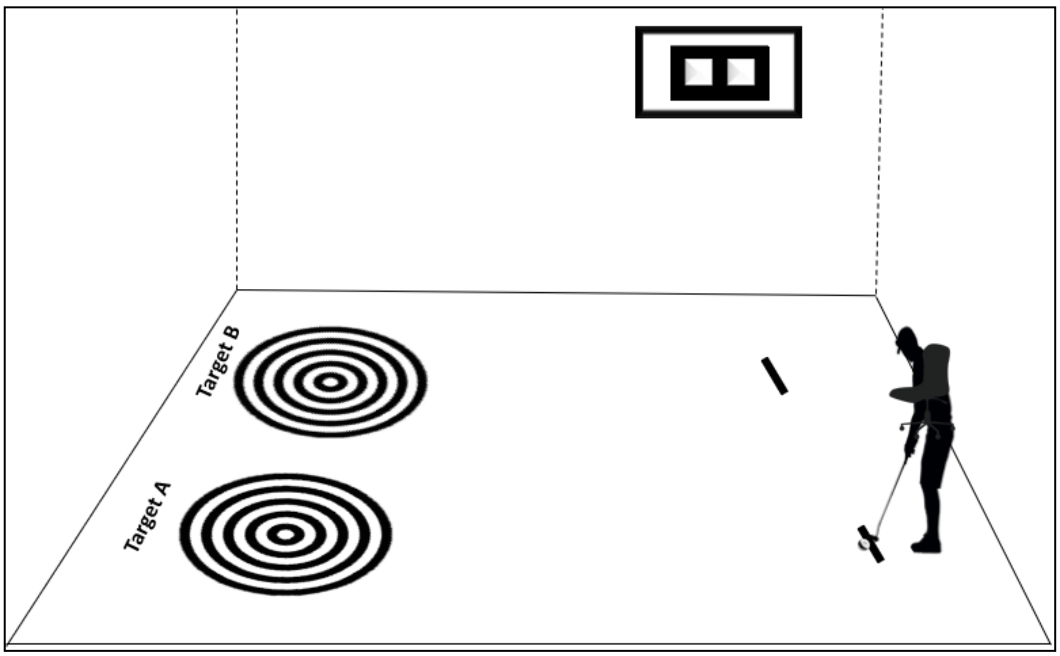
> *"Now, you will perform the whole task. Through practicing the task, you will get the standard 480 raffle tickets, like other participants, to be entered into a virtual drawing for $200. The number of raffle tickets you get is completely unrelated to your performance while practicing the task. Your first goal is to choose the correct target. Your second goal is to putt as accurately as possible to the chosen target. The closer the*

ring relative to the chosen target your putt stops, the more accurate your putt was. Please note that putt accuracy is not related to target selection. In other words, you can still have an accurate putt, even to the wrong target, and you can still have a correct target selection, even if you putt inaccurately. Please prioritize target selection and putt execution equally. For the target selection, a card will appear on the screen and you are going to have to figure out which target it refers to: Target A or Target B. Once you have decided which target the card refers to, pick up a ball from the basket and place it on the starting point for the target you chose. Then putt as accurately as possible. After you putt, you will receive feedback as to whether you chose the correct target and in which ring your putt stopped. After each trial, I will ask you if you are ready to start the next one. Please, proceed at your own pace, moving on to the next trial when you are ready. During the time after each trial, you should look at the card from the previous trial to compare it to the feedback you received about whether you chose the correct target. At the end of this practice session, you will get the standard 480 raffle tickets, like other participants in the experiment, to be entered into the drawing for $200. To reiterate, the number of raffle tickets you get is completely unrelated to your performance during this practice session. Please remember to prioritize target selection and putt execution equally. You will have to perform six blocks of eight trials. You will have a 1-minute break after each block. Do you have any questions?"

After receiving feedback, participants were asked to report when they were ready to begin the next trial. The stimulus remained on the TV screen until the participant was ready to move on. The next trial started when the researcher projected the next stimulus onto the TV screen. During the 1-min break between each block, participants completed single-item engagement and motivation questionnaires asking on a scale of 0 to 10 how engaging and motivating they would rate the action selection and the action execution tasks (Leiker et al., 2019; Pathania et al., 2019).

Figure 1

Experiment Set-up



Note. This figure illustrates the experiment set-up. The upper right rectangle depicts the action selection task whereas the targets (Target A and Target B) for the action execution task are shown in the lower left part of the figure.

At the end of the acquisition phase, participants were given a 5-min break before completing the first post-test. During this period, participants completed the Intrinsic Motivation Questionnaire (IMI; McAuley et al., 1989). The IMI was designed to assess participants' experience with experimental tasks. Specifically, it assesses participants' interest/enjoyment, effort/importance, value/usefulness, pressure/tension, perceived choice, and perceived competence during practice of a given task, resulting in a total of six subscales scores. The IMI was comprised of 37 questions answered based on a seven-point Likert scale ranging from "not true at all" to "very true". All six subscales scores were included in the statistical analyses.

Following the 5-min break, participants performed four warm-up golf putting trials to the wall behind them, not to the target. Next, they completed the immediate retention test, which consisted of one block of eight trials of both the action selection and execution tasks. Participants were told that they would perform the same task they had just practiced but that, this time, feedback would not be provided. After completion of the immediate retention test, participants were thanked and reminded to return the following day to complete the second post-test.

Day 2 and 3 of data collection. Participants were required to return 24 hours and 6 days after completing day 1 in order to perform the 24 hr and the 7-day post-tests. Both tests were the same as the immediate post-test with four warm-up golf putting trials before the beginning of the test. After the last day of data collection, participants were debriefed with respect to the purpose of the study and thanked for their participation.

Dependent variables and data processing

Since the purpose of this pilot study was to measure performance and learning effects associated with the action selection and action execution tasks, the dependent variables of interest were action selection accuracy and golf putting accuracy and precision. Radial error (RE) and bivariate variable error (BVE) were measured to index putting accuracy and precision, respectively, as recommended by Hancock et al. (1995). These two measures were recorded using Dartfish Live 9.0® motion analysis software and calculated for the pretest, all blocks in the acquisition phase and post-tests (immediate, 24 hr and 7-day). Action selection accuracy was indexed by the number of trials in which a correct response was selected (i.e., 4 correct responses within a block of 8 trials represents 0.5 or 50% accuracy). This measure was calculated for all blocks in the acquisition phase and post-tests.

Statistical Analysis

For the action selection task, post-test was compared to a test value of 0.5 (chance accuracy) in one-sample *t*-tests. To assess learning for the action execution task, separate repeated-measures ANOVAs were conducted for both RE and BVE with test (pretest/immediate post-test/24 hr post-test/7-day post-test) serving as the independent variable. Alpha was set to .05, and the Greenhouse-Geisser correction was applied when sphericity was violated.

Results

Action Selection Accuracy. The one-sample *t*-tests between the sample mean and the test value were significant for the immediate post-test ($t(12) = 3.55$, $p = .004$, $dz = 0.985$) and the 7-day post-test ($t(12) = 2.26$, $p = .043$, $dz = 0.627$), indicating that participants scored significantly above chance for these tests (see Table 1). For the 24 hr post-test, the one-sample *t*-test revealed a non-significant difference between the sample mean and the test value ($t(12) = 1.96$, $p = .073$, $dz = 0.544$), indicating that participants did not score above chance on this post-test.

Putting Accuracy and Precision. The repeated-measures ANOVA for RE revealed a significant main effect of phase ($F(3, 36) = 4.87$, $p = .006$, $\eta_p^2 = .289$). Fisher LSD post-hoc comparisons revealed a significant difference between pretest and immediate ($p = .004$, $dz = 0.989$) and 7-day post-tests ($p = .009$, $dz = 0.870$) indicating that participants were significantly more accurate during these post-tests compared to pretest. The comparison between pretest and 24 hr post-test revealed nonsignificant results ($p = .081$). The repeated-measures ANOVA for BVE revealed a significant main effect of phase ($F(3,36) = 4.11$, $p = .013$, $\eta_p^2 = .255$). Fisher

LSD post-hoc comparisons revealed a significant difference between pretest and both immediate (p = .008, dz = 0.887) and 7-day post-tests (p = .018, dz = 0.760) indicating that participants were significantly more precise during these post-tests compared to pretest. The comparison between pretest and 24 hr post-test revealed nonsignificant results (p = .162).

Table 1

Pilot Study – Action Selection Accuracy, Radial Error and Bivariate Variable Error

	Action Selection Accuracy	Radial Error	Bivariate Variable Error
Test	*M* (cm) [CI]	*M* (cm) [CI]	*M* (cm) [CI]
Pre-test	-	68.4 [55.1, 81.8]	74.8 [59.4, 90.2]
Immediate post-test	.692 [.586, .798]	43.6 [36.8, 50.3]	49.6 [41.8, 57.3]
24 hr post-test	.635 [.500, .769]	54.6 [41.9, 67.3]	62.2 [49.6, 74.7]
7-day post-test	.654 [.520, .787]	50.8 [42.5, 59.1]	55.5 [45.5, 65.5]

Note. Action selection accuracy, radial error and bivariate variable error as a function of test pre-test, immediate post-test, 24 hr post-test, 7-day post-test). *M* = mean, CI = 95% confidence interval.

Discussion

The goal of the pilot study was to assure that the information-integration category-learning task and the golf putting task exhibited learning effects. Based on the results, participants performed significantly above chance on the immediate and 7-day action selection task post-tests and showed superior putting performance on the immediate and 7-day post-tests compared to pretest, indicative of learning.

Main Experiment

The data collection and analyses were registered on the website AsPredicted.org (https://aspredicted.org/ce4sw.pdf) before the experiment was initiated. The purpose of this experiment was to investigate the effect, if any, of rewards and punishments on the capability to select the appropriate response for one's movement (action selection) and on the movement itself (action execution).

Participants

Seventy-seven young adults (55 females, M_{age} = 20.7 years, 95% CI [20.1, 21.3]) without any previous experience with either task and who were naïve to the purpose of the study participated in this experiment. Eight participants were excluded from the final analysis due to data loss[3] resulting in a final $N = 69$ (51 females), which still met the pre-established sample size of 66 participants. Sample size was determined with an a priori power calculation to reach 80% power ($\alpha \leq .05$) to detect a medium/large-sized interaction ($\eta^2_p = .09$; Cohen, 1988) between experimental group (reward/punishment/neutral) and test (pretest/immediate post-test/24 hr post-test/7-day post-test). Given the applied nature of our study, we powered it to detect a medium/large-sized effect, as we reasoned that the cost to an instructor of rewarding and punishing learners in practice should be offset by at least a medium/large-sized benefit. Recruitment was done through SONA, the College of Education Research Participation System at Auburn University, and by word-of-mouth. A total of four course credits was offered in exchange for participation when applicable, in addition to a certain number of raffle tickets[4] to be entered into a virtual drawing for $200 USD. Participants did not report any neuromuscular impairment that would affect the execution of a golf putting task or any difficulties in distinguishing between colors, which could affect the categorization task. All participants provided written consent to an institution-approved research protocol (18-178 EP 1806) in agreement with the 1964 Declaration of Helsinki.

Procedures

[3] The iPads stopped working during data collection, which resulted in data loss.
[4] The number of raffle tickets offered varied according to the participant's condition. However, on average, all participants received approximately the same number of raffle tickets.

Tasks. The tasks were the same as in the pilot study (i.e., information-integration category-learning task and golf putting task).

Day 1 of Data Collection. The procedures were the same as in pilot study with one exception. Based on pretest golf putting RE, participants were quasi-randomly assigned to one of three conditions: reward, punishment, or neutral. Participants in the neutral condition received 480 raffle tickets regardless of their performance during practice, whereas participants in the reward and punishment conditions gained or lost, respectively, raffle tickets on each trial based on whether they chose the correct/incorrect target and how accurate their putt was. More specifically, participants in the reward condition were given the following instructions:

> *"Through practicing the task, you have the opportunity to gain raffle tickets to be entered into a virtual drawing for $200. Like other participants, you will start with the standard zero raffle tickets. The more raffle tickets you gain, the more likely you are to win the $200. Your first goal is to choose the correct target. Every time you choose the correct target, you will gain seven raffle tickets. Your second goal is to putt as accurately as possible to the chosen target. The closer the ring relative to the chosen target your putt stops, the more accurate your putt was and the more raffle tickets you will gain. Specifically, you will gain 10 tickets if your putt stops in the innermost ring, 9 tickets if your putt stops in the second innermost ring, and so on. If your putt misses the outermost ring, then you will gain zero tickets".*

Participants in the punishment group were told that they were:

> *"starting this task with the standard 960 raffle tickets, like other participants, to be entered into a virtual drawing for $200. Through practicing this task, you will have the opportunity to lose raffle tickets. The fewer raffle tickets you lose, the more likely you are*

to win the $200. Your first goal is to choose the correct target. Every time you choose the incorrect target, you will lose seven raffle tickets. Your second goal is to putt as accurately as possible to the chosen target. The closer the ring relative to the chosen target your putt stops, the more accurate your putt was and the fewer raffle tickets you will lose. Specifically, you will lose zero tickets if your putt stops in the innermost ring, one ticket if your putt stops in the second innermost ring, and so on. If your putt misses the outermost ring, then you will lose 10 tickets".

Prior to the pilot study, we chose to assign seven raffle tickets for choosing the correct target in order to approximately equate the number of raffle tickets that would be earned through the action selection and action execution tasks. Specifically, we used data from previous golf putting experiments in our lab (Daou et al., 2018, 2019) to determine that participants were likely to earn an average of five to six raffle tickets per putt, and we assumed that participants would get close to 75% of the action selection trials correct, giving them an average of 5 to 6 raffle tickets on each trial of the action selection task.[5] After being assigned to their respective condition, participants followed the same protocol as in the pilot study.

Day 2 and 3 of Data Collection. Days 2 and 3 of data collection were the same as in the pilot study.

Dependent Variables and Data Processing

The main dependent variables of interest and data processing were the same as in the pilot study (RE, BVE and action selection accuracy). For the IMI, a single score for each subscale was created by averaging the items within the subscales, all of which exhibited good reliability: interest/enjoyment ($\alpha = 0.896$); effort/importance ($\alpha = 0.813$); value/usefulness ($\alpha =$

[5] Supplemental material available at https://osf.io/9ufgb/ contains a spreadsheet with the number of tickets earned by each participant during the acquisition phase as well as figures summarizing the number of tickets gained/lost over time as a function of group and task.

0.870); pressure/tension (α = 0.777); perceived choice (α = 0.870); and perceived competence (α = 0.908). Consistent with previous studies (Grand et al., 2015, 2017a; Rhoads et al., 2019), we also created a composite measure of effort/motivation that could be used for exploratory analyses (e.g., investigating whether effort/motivation explains post-test performance). Specifically, we averaged across the interest/enjoyment, effort/importance, and value/usefulness subscales, which were moderately to strongly correlated (rs: .333 - .690, ps ≤ .005).

Statistical Analysis

To assess action selection task accuracy during the acquisition phase, we conducted a mixed-factor ANOVA with group (neutral/punishment/reward) serving as the between-subject factor and acquisition block (1/2/3/4/5/6) serving as the within-subject factor. To assess action execution task accuracy and precision during the acquisition phase, we conducted separate mixed-factor ANOVAs for RE and BVE with group serving as the between-subject factor and acquisition block (1/2/3/4/5/6) serving as the within-subjects factor[6]. To assess learning of the action selection task, we conducted a mixed-factor ANOVA with group serving as the between-subject factor and post-test (immediate post-test/24 hr post-test/7-day post-test) serving as the within-subject factor, and accuracy serving as the dependent variable. To assess learning of the action execution task, separate mixed-factor ANCOVAs were conducted for both RE and BVE with group serving as the between-subjects factor, post-test serving as the within-subject factor, and pretest serving as the covariate. Alpha was set to .05, and the Greenhouse-Geisser correction was applied when sphericity was violated.

Results

Pretest and practice performance.

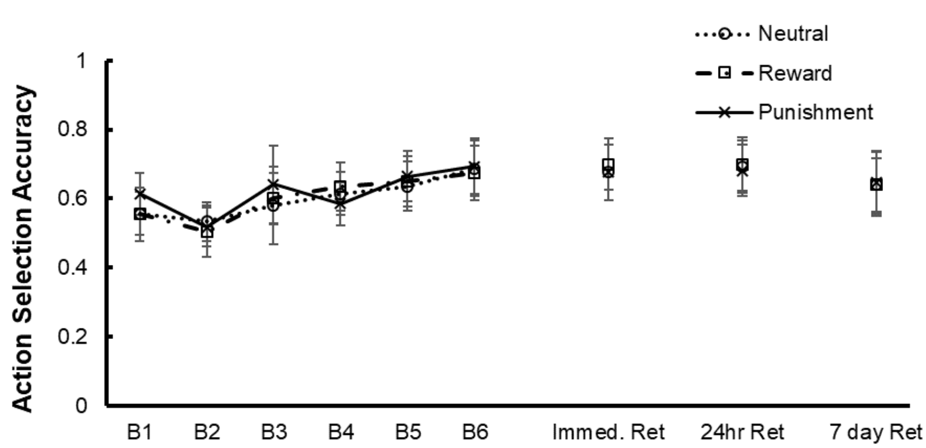
[6] We initially included pretest as a level in the within-subjects factor in these ANOVAs, as pre-registered. However, an anonymous reviewer suggested removing pretest, and we agreed with this suggestion to create clearer correspondence between motor execution and selection analyses.

Action selection accuracy. Figure 2 shows action selection accuracy for all three groups across acquisition phase and post-tests. The mixed-factor ANOVA revealed a significant effect of block ($F(5{,}330) = 8.24$, $p < .001$, $\eta_p^2 = .111$, $\varepsilon = .920$) best described by a linear function ($F(1{,}66) = 30.6$, $p < .001$, $\eta_p^2 = .317$; *p*s for other polynomial contrasts $\geq .105$) indicating that participants improved from block 1 to block 6. There were nonsignificant effects for group and Group x Block interaction during the acquisition phase (*Fs* ≤ 0.431).

Action execution accuracy and precision. Figure 3 shows RE and BVE for all three groups across all phases of the study. The mixed-factor ANOVA for RE revealed a main effect of block ($F(5{,}330) = 4.54$, $p < .001$, $\eta_p^2 = .064$, $\varepsilon = .881$) best described by a linear function ($F(1{,}66) = 22.3$, $p < .001$, $\eta_p^2 = .253$; *p*s for other polynomial contrasts $\geq .075$). The mixed-factor ANOVA for BVE showed a main effect of block ($F(5{,}330) = 4.91$, $p < .001$, $\eta_p^2 = .069$, $\varepsilon = .891$) best described by a linear function ($F(1{,}66) = 30.1$, $p < .001$, $\eta_p^2 = .313$; *p*s for other polynomial contrasts $\geq .257$).

Figure 2

Action Selection Accuracy



Note. Action selection accuracy (higher numbers indicate greater accuracy) as a function of phase (acquisition, immediate retention, 24 hr retention and 7-day retention) and group (neutral, reward and punishment). Error bars represent 95% *CIs*.

Post-test performance.

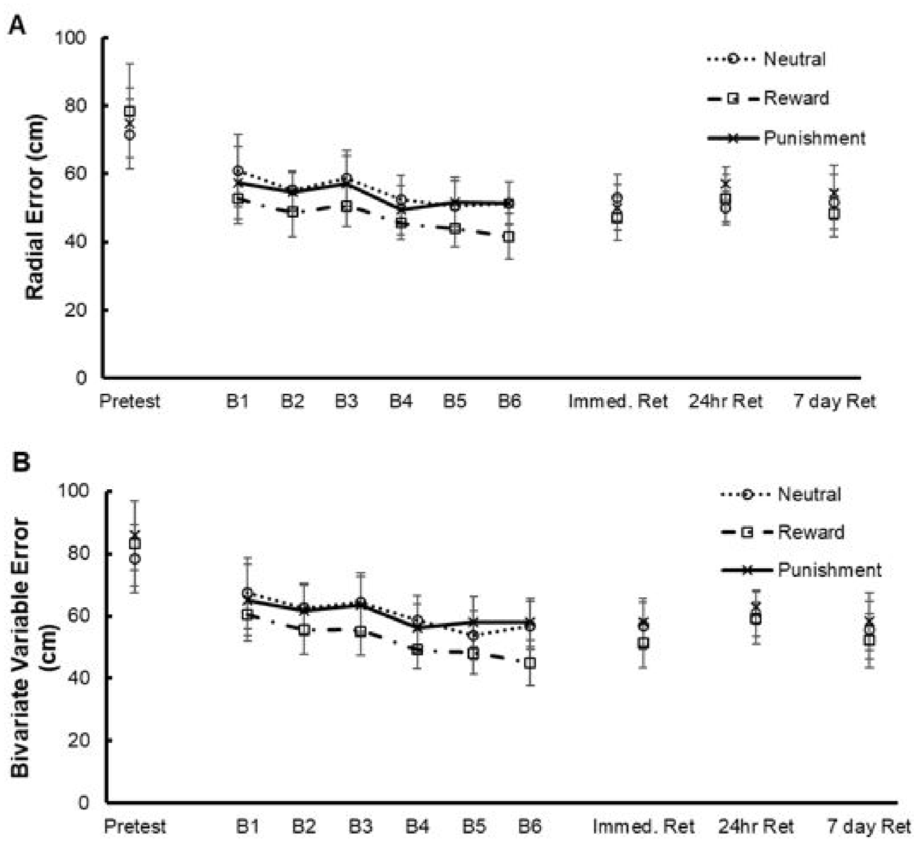
Action selection accuracy. The mixed-factor ANOVA for post-test action selection accuracy revealed nonsignificant effects for group, post-test and Group x Post-test interaction ($Fs \leq 2.57$). Next, we compared post-test target selection accuracy at each post-test to a test value of 0.5 in one-sample *t*-tests to determine whether participants performed significantly above chance. Results revealed significant effects for each test (immediate: $t(68) = 7.69$, $p < .001$, $M = .685$, 95% CI [.638, .732], $dz = 0.926$; 24 hr: $t(68) = 8.67$, $p < .001$, $M = .692$, 95% CI [.649, .735], $dz = 1.04$; 7-day: $t(68) = 5.48$, $p < .001$, $M = .643$, 95% CI [.592, .694], $dz = 0.660$), indicating that participants performed significantly above chance at each post-test with respect to action selection.

Action execution accuracy and precision. The ANCOVA for post-test action execution accuracy revealed nonsignificant effects for group, post-test and Post-test x Group interaction (Fs ≤ 1.24) after controlling for pretest. Next, we conducted three separate paired sample *t*-tests between pre-test and all three post-tests (immediate/24 hr/7-day). Results of the paired sample *t*-tests revealed a significant difference between pretest and immediate post-test ($t(68) = 7.40$, $p < .001$, $M = 25.3$, 95% CI [18.6, 32.0], $dz = 0.891$), pre-test and 24 hr post-test ($t(68) = 6.62$, $p < .001$, $M = 21.8$, 95% CI [15.4, 28.3], $dz = 0.797$) and pre-test and 7-day post-test ($t(68) = 7.63$, $p < .001$, $M = 23.8$, 95% CI [17.7, 29.9], $dz = 0.919$), indicating that participants improved from pretest to all post-tests with respect to action execution accuracy. The ANCOVA for post-test action execution precision revealed nonsignificant effects for group, post-test and Post-test x

Group interaction (Fs ≤ 1.84) after controlling for pretest. Since there was not a main effect of group, we again conducted three separate paired sample t-tests between pre-test and all three post-tests (immediate/24 hr/7-day). Results of the paired sample t-tests revealed a significant difference between pretest and immediate post-test ($t(68) = 7.84$, $p < .001$, $M = 27.3$, 95% CI [20.5, 34.1], $dz = 0.944$), pre-test and 24 hr post-test ($t(68) = 6.14$, $p < .001$, $M = 22.2$, 95% CI [15.1, 29.2], $dz = 0.739$) and pre-test and 7-day post-test ($t(68) = 7.89$, $p < .001$, $M = 27.3$, 95% CI [20.5, 34.1], $dz = 0.950$), indicating that participants improved from pretest to all post-tests with respect to action execution precision.

Figure 3

Action Execution Accuracy and Precision

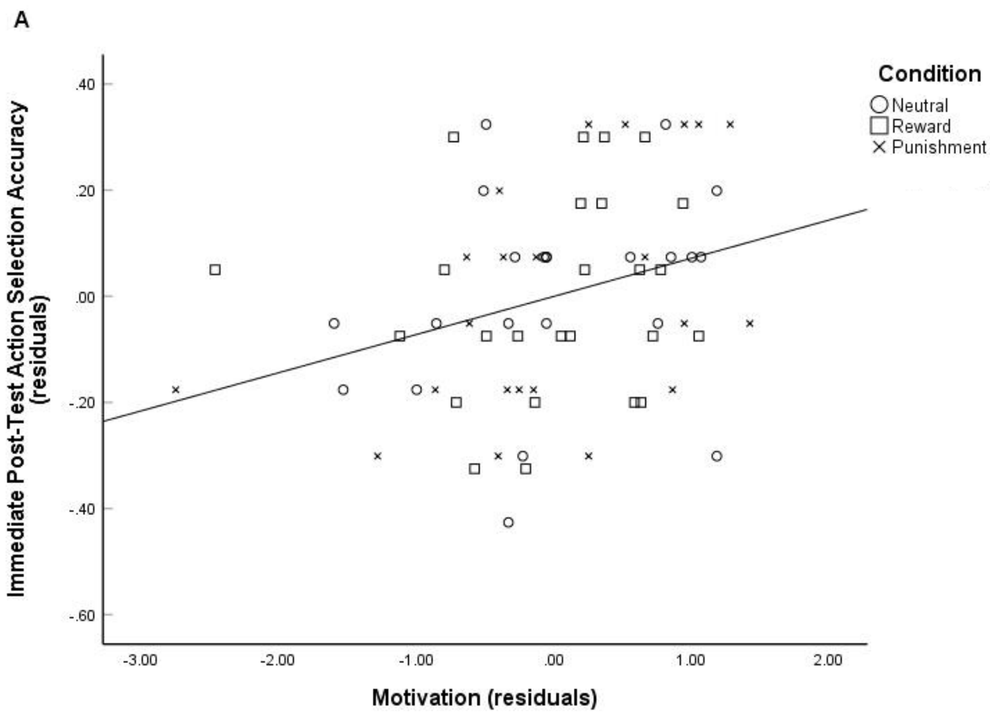


Note. A: Action execution accuracy (lower numbers indicate greater accuracy) as a function of phase (pre-test, acquisition, immediate retention, 24 hr retention and 7-day retention) and group (neutral, reward and punishment). Error bars represent 95% *CIs.* B: Action execution precision (lower numbers indicate greater precision) as a function of phase (pre-test, acquisition, immediate retention, 24 hr retention and 7-day retention) and group (neutral, reward and punishment). Error bars represent 95% *CIs.*

Exploratory analyses. We conducted a MANOVA to assess the effect of group on the six subscales of the IMI. The results revealed a nonsignificant effect of group for all subscales (interest/enjoyment; effort/importance; value/usefulness; pressure/tension; perceived choice; and perceived competence; Wilk's $\lambda = .778$, $F = 1.36$). Next, we conducted separate regressions to assess whether motivation predicted action selection and action execution post-test performance (immediate/24 hr/7-day), after controlling for group and pretest and correcting for multiple comparisons produced by conducting three separate regressions (significance level: $p < .017$). For the action selection task, we conducted three stepwise hierarchical regressions with contrast codes based on group serving as the predictor variables in the first step and the composite measure of motivation being added in the second step. The results of the regressions indicated that adding motivation in the second step of the regressions explained additional variance in immediate post-test action selection accuracy (change in F (1, 65) = 6.74, $p = .012$, $R^2 change =$ .094). Specifically, motivation explained nearly 10% of variance observed in the immediate post-test action selection accuracy performance (Figure 4). The unstandardized coefficient for motivation was $\beta = 0.072$, $p = .012$. However, motivation did not significantly explain additional variance in both 24 hr (change in $F = 3.86$, $p = .054$) and 7-day (change in $F = 4.28$, $p = .043$) post-tests, after correcting for multiple comparisons. For the action execution task, separate stepwise hierarchical regressions were conducted where RE and BVE served as the outcome variable with contrast codes (based on group) and pretest serving as the predictor variables in the first step and the composite measure of motivation being added in the second step. The results of the separate regressions indicated that adding motivation in the second step approached our corrected significance criterion in terms of explaining additional variance in immediate post-test accuracy (change in F (1, 64) = 5.86, p = .018, $R^2 change$ = .077) and met our corrected significance criterion in terms of explaining additional variance in immediate post-test precision (change in F (1, 64) = 7.57, p = .008, $R^2 change$ = .093) (Figure 5), but not in the two long-term post-tests (24 hr and 7-day) for both accuracy and precision (24 hr post-test RE: change in F = 0.254, p = .616; 7-day post-test RE: change in F = 0.041, p = .839; 24 hr post-test BVE: change in F < 0.001, p = .984; 7-day post-test BVE: change in F = 0.082, p = .776). More specifically, motivation explained nearly 8% and 10% of the variance observed in immediate action execution accuracy and precision performance, respectively, but was not able to explain long-term accuracy and precision performance, as indexed by the 24 hr and 7-day post-tests.

Figure 4

Immediate Post-Test Action Selection Accuracy as a Function of Motivation

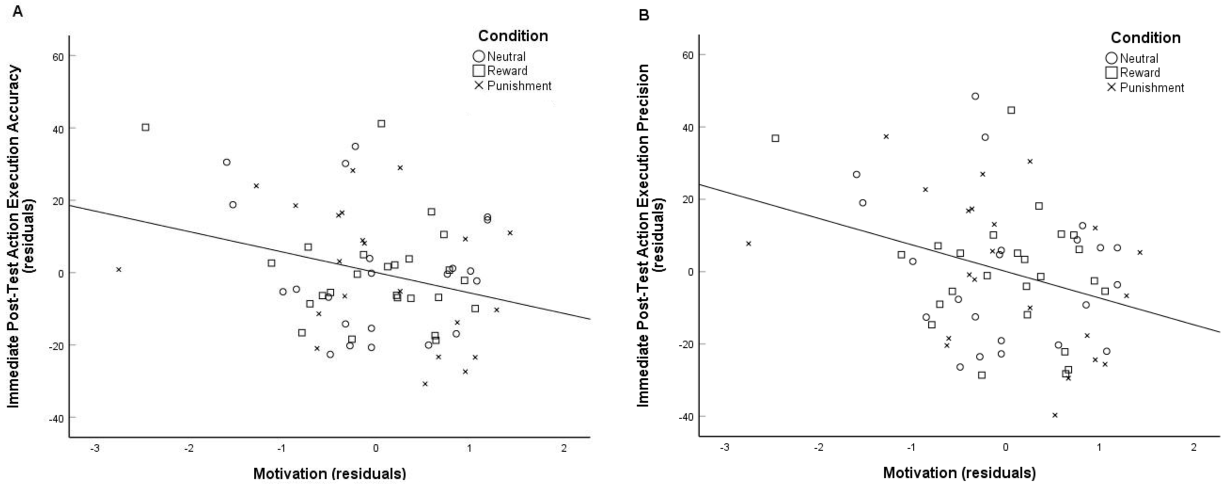


Note. This figure shows the relationship between immediate post-test action selection accuracy (residuals) and motivation (residuals) after controlling for condition (neutral, reward and punishment).

Participants also completed the single item engagement and motivation questionnaires (descriptive data presented in Table 2). However, since these self-reported measures would not contribute to elucidate why motivation, as indexed by IMI scores, was able to explain performance but not learning in both action selection and action execution tasks, they were not included in the exploratory analysis.

Figure 5

Immediate Post-Test Action Execution Accuracy and Precision as a Function of Motivation



Note. This figure shows the relationship between immediate post-test action execution accuracy (panel A) and precision (panel B) (residuals) and motivation (residuals) after controlling for pre-test and condition (neutral, reward and punishment).

Table 2

Descriptive Data by Group

	Neutral (*n* = 22, 7 males)	Reward (*n* = 25, 7 males)	Punishment (*n* = 22, 4 males)
	M [CI]	*M* [CI]	*M* [CI]
Age (years)	21.1 [19.2, 23.1]	20.6 [20.2, 21.1]	20.4 [20.0, 20.7]
Lifetime Putting Experience[a]	1.73 [1.32, 2.14]	1.96 [1.31, 2.61]	1.91 [1.07, 2.75]
Past-Year Putting Experience[a]	0.450 [0.237, 0.663]	0.80 [0.574, 1.03]	0.590 [0.379, 0.800]
Composite IMI	5.25 [4.90, 5.60]	5.6 [5.29, 5.91]	5.24 [4.84, 5.64]
Single-item Motivation Action Selection	7.59 [6.95, 8.23]	7.61 [6.76, 8.46]	7.95 [7.14, 8.76]
Single-item Engagement Action Selection	8.21 [7.69, 8.73]	7.89 [7.06, 8.72]	8.27 [7.60, 8.94]
Single-item Motivation Action Execution	8.45 [8.01, 8.89]	8.75 [8.34, 9.16]	8.89 [8.40, 9.38]
Single-item Engagement Action Execution	8.41 [8.02, 8.80]	8.55 [8.04, 9.06]	8.75 [8.23, 9.27]

Note. M = Mean, CI = 95% confidence interval. [a]Putting experience: 0 = Never putted; 1= Putted 1 – 10 times; 2 = Putted 11 – 20 times; 3 = Putted 21 – 30 times.

Discussion

The current study was designed to investigate whether rewards and/or punishments affect action selection and/or action execution components of a skill (Chen et al., 2018). Action selection accuracy and golf putting accuracy and precision were measured while 69 participants performed an information-integration category-learning task followed by a golf putting task. Based on previous studies (Abe et al., 2011; Hasson et al., 2015) we hypothesized that participants who received rewards throughout practice would exhibit superior learning when tested 24 hours and 7 days after acquisition, compared to participants who were punished throughout practice and participants who received neither rewards nor punishments during practice. Results revealed that participants showed learning effects for both tasks as indexed by post-test performance. However, even though the improvements were linear as opposed to non-linear with the reward group showing a numeric advantage over the other two groups in the action execution task (see Figures 2 and 3), groups did not significantly differ with respect to action selection accuracy, nor did they show significant differences in putting accuracy and precision. In other words, giving participants rewards during practice did not result in significantly better action selection or action execution post-test performance.

Results are inconsistent with past literature demonstrating that participants who received financial rewards showed superior learning and performance improvement in the absence of practice (offline gains) compared to participants who were punished or neither rewarded nor punished (Abe et al., 2011) as well as studies showing that the type of reinforcement learning schedule adopted during practice can enhance acquisition and retention of a motor skill (Dayan et al., 2014). For instance, in the study by Dayan et al. (2014), participants who practiced a sequential pinch force task under a higher level of uncertainty as to whether they would be financially rewarded after a successful trial showed superior performance during post-tests and performance improvement in the absence of practice (offline gains) from immediate to 7-day post-test compared to the other two groups in which rewards were either more predictable or occurred 100% of the time after a successful trial. In the present study, participants were rewarded 100% of the time after a successful trial, resulting in an environment with low levels of stochasticity. In other words, our reward system did not maximize reward-prediction errors because the rewards were highly predictable. Therefore, it is possible that the predictable nature of rewards contributed to the lack of superior performance of the reward group compared to the other two groups, although it is unlikely to have done so. This is because in the present study the type of reinforcement schedule was not manipulated. Instead, we compared groups in which rewards were either granted, punishments were given, or neither. According to the reinforcement learning theory, behaviors that are rewarded are more likely to occur in the future as opposed to behaviors that are punished. Thus, even though the reward group practiced under a low level of stochasticity, each of their successful behaviors was reinforced, which, in theory, should have contributed to motor skill learning.

Our findings are also inconsistent with the recent study by Galea et al. (2015) showing that punishments and rewards have dissociable effects on motor learning. Specifically, the authors claimed that punishments led to faster motor adaptation during a visuomotor task via activation of the sensory-prediction error system, whereas rewards favored skill retention via dopamine release and consequent memory trace strengthening. In the present study, neither rewards nor punishments affected performance and learning distinctively, which might be somewhat associated with the availability of sensory information. In the study by Galea et al., the availability of visual information varied between blocks, whereas in our study visual feedback was available throughout practice. Researchers argue that when accurate and reliable sensory information is present, reliance on other sources of information such as reward-prediction errors becomes less likely (Izawa & Shadmehr, 2011). In principle, since all three groups had access to visual feedback, they might have relied on their sensory-prediction error system to make movement adjustments, which might help explain the lack of group differences in the action execution task. Although interesting, the findings by Galea et al. (2015) need to be interpreted with caution. This is because the study did not include long-term retention tests (at least 24 hr), making it hard to generalize these results to long-lasting changes in performance.

Other possible explanations for the lack of experimental manipulation effect might be related to the raffle tickets system and the total number of trials used during acquisition. Regarding the former, in the present study, participants in the punishment group started off with a total of 960 raffle tickets, and were told that they would lose raffle tickets based on incorrect target selection and poor putting execution. Even though these participants could lose up to 17 raffle tickets (7 raffle tickets for choosing the incorrect target and 10 raffle tickets for a poor putt) in a single trial, they started the task with almost 1000 raffle tickets, which might have blunted the feeling of being punished after a bad outcome. On the other hand, it is worth mentioning that the reward group could also earn up to 17 raffle tickets per trial, in cases where they chose the correct target and their putt landed on the innermost circle, which should have been a large incentive. Future studies using the same paradigm could vary the magnitude of rewards and punishments in order to assess their effects on learning. With respect to total number of trials during acquisition, participants only performed 48 trials, which may have limited the amount of learning that could be moderated by practice condition. However, participants did exhibit linear improvement in performance throughout acquisition, suggesting that there was learning to be moderated. Nonetheless, future studies adopting the same protocol should consider a larger “dose” of practice to allow more room for effects of practice condition to occur.

Since motivation has been shown to play a role in motor skill acquisition (Wulf & Lewthwaite, 2016), we also conducted exploratory analyses to test for a main effect of condition on motivation. Results indicated that condition did not affect motivation. Following, separate regressions were carried out to investigate whether motivation predicted post-test performance for both tasks regardless of condition. Results showed that motivation predicted both action selection and action execution immediate performance, but not long-term retention, which is in line with recent findings (Grand et al., 2017a). Although it is possible that motivation uniquely affects performance and not learning, the absence of motivation effects on learning might be associated with the nature of the measure itself. Even though widely used in the motor learning domain, the IMI is a self-reported and, therefore, explicit measure of motivation. For instance, (Brunstein & Schmitt, 2004) showed that different measures of achievement motivation (i.e., explicit and implicit) predicted different aspects of a task (i.e., self- reported engagement scores and task performance, respectively). Therefore, future studies investigating motivation in the context of motor skill learning should consider using different measures of motivation to elucidate whether they predict performance and learning to differing extents.

To our knowledge, this is the first study to try to tease apart the effect of rewards and punishments on action selection and action execution components of a skill (Chen et al., 2018). Since complex sports such as soccer, football, baseball, and basketball consist of an interplay between action selection and action execution, it is useful to know whether extrinsic incentives affect these two components independently. Even though coaches adopt their own strategy to allocate external incentives in training sessions, these strategies lack empirical support. From a scientific perspective, there is a growing body of research investigating the use of external incentives to improve motor skill acquisition. However, the majority of these studies used laboratory-based tasks that do not resemble the reality faced by coaches and athletes. The present study attempted to shorten this distance between laboratory-based research and real-world skill learning as well as reinforced the need for future investigations on this matter.

Conclusion

In the present study, we designed an experiment to investigate whether rewards and punishments affect action selection and/or action execution components of a skill. The results showed that giving participants rewards during practice did not result in better action selection or action execution post-test performance. Exploratory analyses revealed motivation was able to explain changes in action selection and action execution performance, but not learning. These findings call into question the effectiveness of motivation in predicting learning and draw attention to the possibility of using different measures to assess people's drive toward a goal. More research is needed to further our knowledge on this matter. Despite the lack of significant difference between conditions, our paradigm was the first one to tease apart action selection and action execution and might be useful, with possible improvements (e.g., increase the "dose" of practice), in future studies aiming to investigate how other factors affect these two components independently.

Chapter 2: Meta-analyzing enhanced expectancies on motor learning: Positive effects but methodological concerns

Introduction

Motor skills are a crucial part of everyone's life. Being able to effectively perform a motor action is facilitated by a thorough understanding of how motor skills are acquired and, more importantly, retained over time. Past attempts to uncover the bases of motor learning and the mechanisms underlying a variety of practice conditions (e.g., random vs. blocked practice; infrequent vs. frequent augmented feedback) relied on a cognitive perspective mainly grounded on the role played by information processing (Guadagnoli & Lee, 2004; Lee et al., 1994). More recently, however, a growing body of studies have shown that attentional and motivational factors may also need to be considered when it comes to understanding and promoting motor learning (Lewthwaite & Wulf, 2010; Pascua et al., 2015; Sanli et al., 2013), which culminated in the proposition of a new theory entitled: 'Optimizing Performance Through Intrinsic Motivation and Attention for Learning (OPTIMAL) theory of motor learning' (Wulf & Lewthwaite, 2016).

According to this theory, learning is facilitated by practice conditions promoting enhanced expectancies, autonomy, and external focus of attention (i.e., focusing on the effects of one's movement). More specifically, practice conditions wherein one's expectancies for future positive outcomes are enhanced (e.g., Lewthwaite & Wulf, 2010), the feeling of autonomy is promoted (e.g., Sanli et al., 2013), and an external focus of attention is encouraged (e.g., Lohse et al., 2010) lead learners to focus on the task goal, which enhances motor performance and learning. Although each motivational and attentional factor outlined in the OPTIMAL theory has been shown to benefit performance and learning, here we focus on studies that investigated enhanced expectancies in a motor learning context.

Different manipulations have been used to enhance learners' expectancies for future success. One of the most studied approaches consists of providing learners with feedback after more accurate trials. This approach has been shown to be effective when contrasted with both neutral (Chiviacowsky et al., 2019) and negative feedback (Chiviacowsky & Wulf, 2007). In another frequently adopted paradigm, which might be considered a manifestation of feedback after good trials, learners are led to believe they are performing better than their peers via provision of positive (false) social-comparative feedback, typically in addition to veridical feedback (Avila et al., 2012). Manipulations of perceived task difficulty have also been used to influence learners' expectations. For instance, studies have reduced perceptions of task difficulty (i.e., made the task look easier) by implementing optical illusions (Bahmani et al., 2017) or changing task criterion of success (Chiviacowsky et al., 2012a). Other ways to enhance expectancies include influencing one's conceptions of ability (i.e., making one believe successful performance is achievable with practice as opposed to being a fixed capacity; e.g., Harter et al., 2019), the use of self-modeling strategies (i.e., showing edited videos with learners' best trials, e.g. Ste-Marie et al., 2011), and extrinsic rewards (e.g., provision of monetary compensation; Abe et al., 2011).

The goal of the present meta-analysis was to investigate the effect of enhancing learners' expectancies for future successful outcomes on motor learning. As a secondary goal, we aimed to estimate the effect of each of the aforementioned manipulations on motor learning. To our knowledge, this meta-analysis is the first quantitative synthesis of the growing body of studies indicating enhanced expectancies facilitate motor learning. Thus, this analysis should provide the best estimate of the effect of enhanced expectancies on motor learning to date. Additionally, we use funnel plot analysis to investigate the risk that inflated effects in small studies (small-study effects) are distorting the extant literature. Our results can inform future investigations, for example by revealing shortcomings in the present research (e.g., small sample sizes). Our findings may also guide motor skill instruction, for example by providing coaches and physical therapists with the state of evidence about recommendations that are easy to implement, such as reducing perceived task difficulty. Thus, our meta-analysis has implications for researchers and practitioners.

Methods

Prior to data collection, methods and main analyses were pre-registered and made available in the Open Science Framework (OSF) repository (Link). The PICO (Population, Intervention, Comparison, Outcome) model was used to define the meta-analysis objectives. The population of interest was human subjects of all ages. Studies investigating people with disabilities and/or impairments were not excluded from the meta-analysis. Interventions were those Wulf and Lewthwaite (2016) indicate have shown enhanced expectancies facilitate motor learning: feedback after good trials, comparative feedback, self-modeling, perceived task difficulty, extrinsic rewards, and conceptions of ability. The main comparison of interest was between enhanced expectancies and control/neutral groups. In the absence of a control/neutral group, a comparison between enhanced expectancies and diminished or negative expectancies groups (e.g., feedback after good trials vs. feedback after poor trials) was considered. The outcome of interest was objective behavioral performance on a delayed (≥ 24-hr) retention test, which is a common and recognized learning evaluation (Kantak & Winstein, 2012).

Study Eligibility Criteria

Studies published in English and Portuguese were considered eligible if they met the following inclusion criteria: (1) it had an experimental design; (2) it used a task requiring movement to accomplish a goal that is increasingly likely to be achieved with practice (R. A. Schmidt & Lee, 2020); (3) it included at least one delayed (≥ 24-hr) retention test; (4) it was published in a peer-reviewed journal; (5) it assessed an objective behavioral measure; and (6) it included at least a positive enhanced expectancies group and a control group or a diminished (negative) expectancies group. Studies were excluded if they failed to meet the inclusion criteria and/or had insufficient data (i.e., did not report mean, standard deviation, or number of participants per group).

Literature Search Strategy

The electronic databases PsycINFO, Web of Science, and PubMed were searched from May 30, 2020, until June 19, 2020 (date of last search). Search terms included a combination of 'motor learning' or 'skill acquisition' and 'expectancies' or 'positive feedback' or 'good trial' or 'successful trial' or 'accurate trial' or 'normative feedback' or 'comparative feedback' or 'comparison feedback' or 'self-model' or 'self-as-a-model' or 'self-video' or 'video model' or 'video edit' or 'conceptions of ability' or 'ability conception' or 'inherent ability' or 'entity theory' or 'incremental theory' or 'learnable skill' or 'natural capacity' or 'acquirable skill' or 'task difficulty' or 'target size' or 'visual illusion' or 'hypnosis' or 'perceived difficulty' or 'mindset' or 'large target' or 'easy goal' or 'easy objective' or 'superstition' or 'reward' or 'incentive' or 'financial reward' or 'money'. String search was adjusted based on electronic database and intervention of interest. A detailed description of the search strategy, including limits used in each database, can be found in the OSF repository. These terms were chosen based on the terms and studies listed in the Enhanced Expectancies section of the OPTIMAL theory paper (Wulf & Lewthwaite, 2016). Further relevant papers were identified by searching through reference lists of previously selected papers and consulting personal archives. Publication period was unrestricted.

Study Selection and Data Extraction

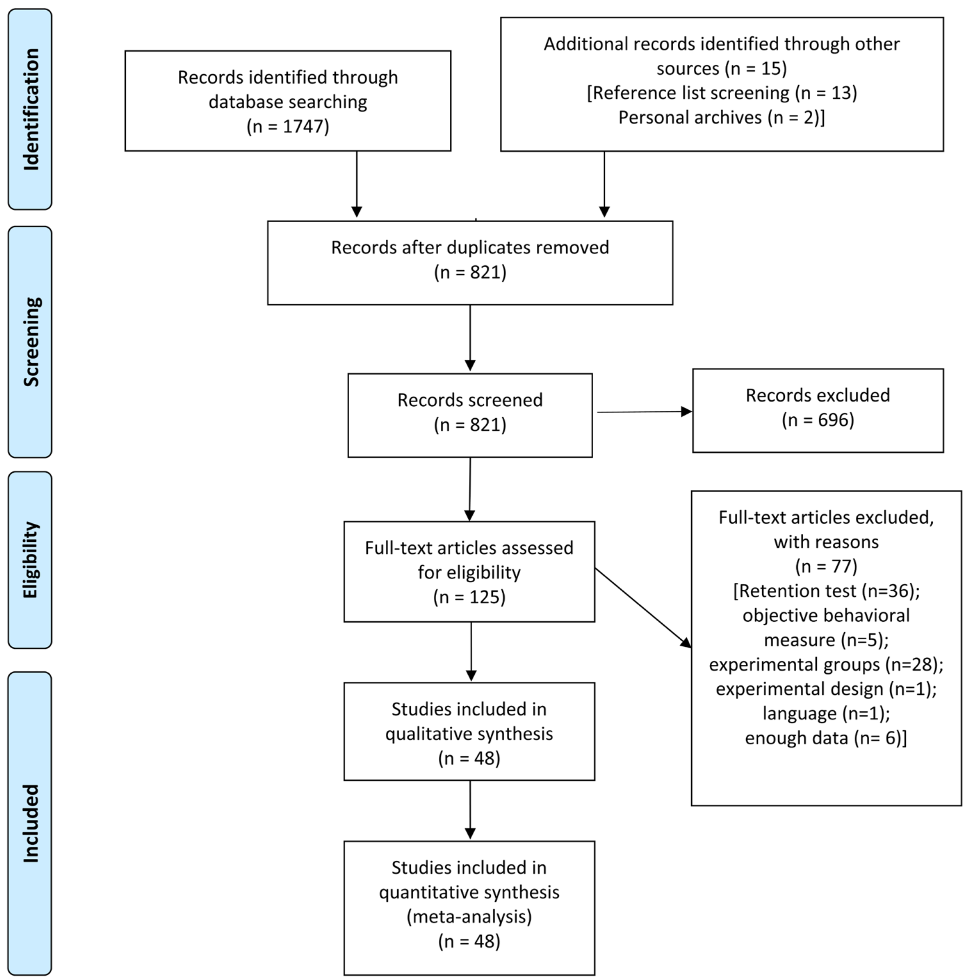
A PRISMA flow chart with a detailed description of the study selection process can be found in Figure 1. The authors M.F.B.B and J.O.P. independently searched for studies in the databases. After removing duplicates, 821 papers were screened by title and abstract. Next, the remaining 125 papers were fully assessed for eligibility according to the inclusion criteria. When there was a disagreement regarding study eligibility, the matter was discussed with the fourth author (MWM) until agreement was reached. At the end of the study selection process, 48 studies met the inclusion criteria and were included in the meta-analysis.

Risk of Bias Assessment

The revised version of the Cochrane risk-of-bias tool (RoB 2) for randomized trials was used to assess the risk of bias in the studies included in the meta-analysis (Sterne et al., 2019). The tool is comprised of five bias domains, namely bias arising from the randomization process, bias due to deviations from intended interventions, bias due to missing outcome data, bias in measurement of the outcome, and bias in selection of the reported result. For the bias due to deviations from intended interventions domain, we focused on the effect of assignment to interventions (intention-to-treat effect). MFBB and JOP independently assessed the five bias domains and classified each one as low risk of bias, some concerns, or high risk of bias for each study following the proposed Cochrane algorithm. Next, an overall judgment of risk of bias was obtained for each study. Specifically, studies were classified as overall low risk of bias if they were judged to be at low risk across all individual bias domains; as "some concerns" if they raised some concerns in at least one domain but were not at high risk in any individual domain; and as overall high risk of bias if they raised some concerns in multiple bias domains or were judged to be at high risk in at least one domain. The robvis tool (McGuinness & Higgins, 2021) was used to plot the risk-of-bias results.

Figure 1

PRISMA Flow Diagram



Note. Figure depicting the flow of information through the different steps of literature search and study selection (Moher et al., 2009).

Data Extraction, Synthesis, and Analysis

The main variable of interest was performance on delayed retention test[7]. Retention test is here defined as the test performed at least 24-hr after the end of the acquisition phase, wherein all groups are tested under identical conditions and perform a task similar to the one performed during the acquisition phase (Schmidt et al., 2018). Only objective measures of performance were considered. When studies did not have a 24-hr retention test or contained more than one retention test, the retention test closest to 24-hr was chosen to increase homogeneity among studies. For studies in which the 24-hr retention test was comprised of more than one block of trials, authors were contacted for data so an aggregate measure of retention test performance could be computed. In case of no response, we averaged across blocks (i.e., mean and standard deviation), which was the case for one study (Abbas & North, 2018). For studies that reported more than one behavioral measure, the measure more closely associated with accuracy (e.g., radial error as opposed to bivariate variable error; (Hancock et al., 1995) was chosen, since accuracy typically reflects the task objective (e.g., hitting a target). For studies in which the results of the retention test were presented as a set of individual trials as opposed to a single performance score, corresponding authors were contacted for data that would allow us to compute an aggregate measure of retention test performance. In cases where no response was obtained, we opted for the inclusion of the middle trial among a set of trials (e.g., the fourth of seven trials). The rationale behind the inclusion of the middle trial stems from the idea that this

[7] Two reasons guided our decision to focus on performance on delayed retention test. First, there is no theoretical explanation as to why enhanced expectancies may affect retention and transfer test performance differently. Second, given the significant variability in types of transfers tests found in this literature, adding performance on delayed transfer test to our meta-analysis would likely introduce unnecessary heterogeneity to our data.

trial is less susceptible to warm-up and online learning effects, compared to the first and last trial, respectively. (Considering that averaging across trials was also an option, in the supplementary material we present the results of a sensitivity analysis using an average of retention trials.) Two authors (M.F.B.B. and J.O.P.) were responsible for extracting sample sizes, means, and standard deviations from the selected papers and entering the information into an Excel spreadsheet (Excel 2016, Microsoft). When sample sizes, means, and standard deviations were unavailable in tables or throughout the text, the R package metaDigitise (Pick et al., 2018) was used to extract raw data and summary statistics from figures. Corresponding authors were contacted when sufficient data and/or relevant information was not provided in the article. Only one effect size was extracted per study, except when the study had more than one experimental and control group (Ghorbani & Bund, 2020; Pascua et al., 2015; Wulf et al., 2012a), was comprised of more than one experiment (Steel et al., 2016; Wulf et al., 2012a), and/or assessed different populations (e.g., older vs younger adults; Drews et al., 2013; Grealy et al., 2019). In these cases, the number of effect sizes extracted exceeded the ratio one per study, but the assumption of independency among effect sizes was still met as the same experimental and/or control group was not used in multiple comparisons (Englund et al., 1999). In addition to statistical data, relevant information regarding population characteristics, study protocol, and experimental manipulation was also extracted. Table S1 provides information about experimental manipulation checks, which were conducted for 30 studies and at least somewhat successful in 22.

Hedges' g was chosen as the effect size metric since it considers the sample size of each study, being therefore considered an unbiased or corrected effect size (Lakens, 2013). Variables in which lower scores indicate better outcomes (e.g., radial error) were reversed in sign to ensure that effects favoring the experimental manipulation were positive (Harrer et al., 2021.). Data were fitted into a random-effects model estimated using restricted maximum likelihood. Alpha level was set at .05 and effect size followed the standard guidelines (small = 0.2, medium = 0.5, large = 0.8) suggested by Cohen (1988). Heterogeneity was assessed using the Cochran's Q test. Since this test is influenced by sample size (Higgins et al., 2003), the I2 statistic, quantified as the percentage of total heterogeneity over total variability, was also computed. The presence of small-study effects was assessed via visual inspection of the funnel plot along with Egger's regression test (Egger et al., 1997), which statistically assesses funnel plot asymmetry by predicting effect size from standard error. A trim-and-fill analysis was used to examine the sensitivity of the results to reporting bias (Duval & Tweedie, 2000). This technique iteratively trims studies from one side of the funnel plot until a criterion for symmetry is met, then fills the studies back into the plot while imputing ones that are identical except on the opposite side of the mean along the horizontal axis. The trim-and-fill analysis was carried out using the default algorithm provided by the metafor package (Viechtbauer, 2010) in R (cran.r-project.org) software. Since the trim-and-fill analysis assumes the decision to publish a scientific finding depends solely on the size of an effect, but reporting bias is likely more influenced by whether the effect is significant (Fanelli, 2012), we planned to *p*-curve the studies that had significant results (Simonsohn et al., 2014). However, we opted not to after determining that only 10 studies met the criteria to be included in a *p*-curve, due to the others not containing specific hypotheses, not reporting the types of post-hoc tests performed, reporting significant interactions, etc. Pre-specified moderator analyses were conducted to investigate how the type of manipulation moderated the estimated effect, and to investigate the effect of enhanced expectancies on motor learning when contrasted with different types of comparison groups (control or diminished expectancies). An exploratory moderator analysis was also conducted to investigate the effect of enhanced expectancies on learning in different populations (young adults, older adults, children/adolescents, and special populations). Visual inspection of funnel plots, studentized deleted residuals, and hat values were used to identify outliers and/or overly influential points in the dataset (Viechtbauer & Cheung, 2010). To ensure the robustness of the results, models were run with and without the studies identified as outliers and/or overly influential cases. The present meta-analysis was carried out using the metafor package (Viechtbauer, 2010) in R (cran.r-project.org) software. R code and dataset are available in the OSF repository.

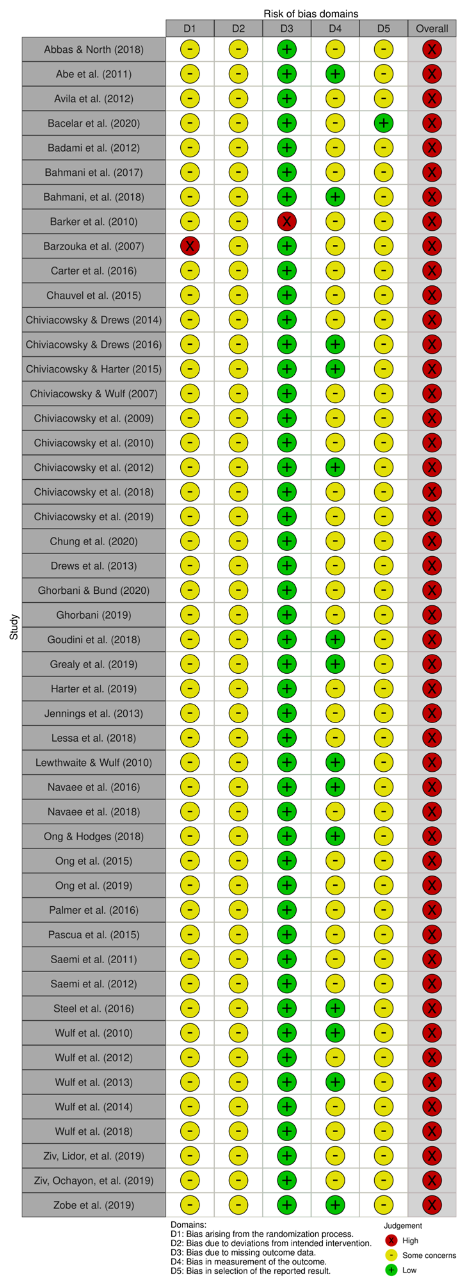
Results

Risk of Bias

Results of the risk of bias assessment are shown in Figure 2. All 48 studies included in the qualitative analysis were judged to be at high risk of bias. This was mainly due to some concerns being raised across all individual domains except for the bias due to missing outcome data domain. Specifically, some concerns were raised in the bias arising from the randomization process domain mostly due to studies not providing a detailed description of the randomization process; in the bias due to deviations from intended interventions domain due to experimenters responsible for delivering the intervention being likely aware of participants' group assignment; in the bias in measurement of the outcome domain due to the lack of information as to whether outcome assessors were aware of the intervention received by participants, which resulted in the assessment of outcome being possibility influenced by the assessors' knowledge of group assignment; and in bias in selection of the reported result domain due to the absence of pre-specified analysis plans. Except for one (Barker et al., 2010), studies were classified as being at low risk in the bias due to missing outcome data domain as there was no indication of missing data.

Figure 2

Risk of Bias Assessment Results



Note. Figure depicting all 48 studies included in the qualitative analysis and their respective risk of bias for each bias domain as well as overall risk of bias.

Descriptive Analysis

A summary of the main characteristics of the studies included in the meta-analysis can be found in Table 1. Forty-one studies contributed one data point each to the meta-analysis, whereas six contributed two data points each (Ghorbani & Bund, 2020; Grealy et al., 2019; Pascua et al., 2015; Steel et al., 2016; Wulf et al., 2012b, 2014), and one study contributed three data points (Drews et al., 2013), resulting in a total of 56 effect sizes. The oldest studies included in the meta-analysis were published in 2007 (Barzouka et al., 2007; Chiviacowsky & Wulf, 2007), whereas the most recent ones were published in 2020 (Bacelar et al., 2020; Chung et al., 2020; Ghorbani & Bund, 2020), resulting in a publication period range of 14 years. The average study sample size was 14.85/group (median = 14/group), ranging from 8 to 28 participants per group.

Of the 56 effect sizes included in the meta-analysis, 16 represent manipulations of feedback after good trials, 13 represent manipulations of perceived task difficulty, 15 represent manipulations of comparative feedback, 7 represent manipulations of conceptions of ability, 4 represent manipulations of extrinsic rewards/punishments, and 2 represent manipulations of self-modeling[8]. The effect sizes composing this meta-analysis were extracted from data pertaining to young adults (*n* = 34), older adults (*n* = 6), children and adolescents (*n* = 13), and special populations (*n* = 3) consisting of adults with a disability in at least one upper or lower extremity (Bahmani et al., 2018), adults with Parkinson's disease (Chung et al., 2020), and autistic children (Navaee et al., 2018). Most of the effect sizes refer to a 24-hr retention test (*n* = 44), whereas the

[8] If summed, the number of manipulations exceeds the total number of effect sizes included in the meta-analysis. This is because one effect size reflects two manipulations combined (i.e., feedback after good trials and conceptions of ability; Wulf et al., 2013).

remaining refer to a retention test carried out between 24-hr and one week after the acquisition phase (n = 7), or to a retention test carried out at least one week after the acquisition phase (n = 5).

Meta-analysis

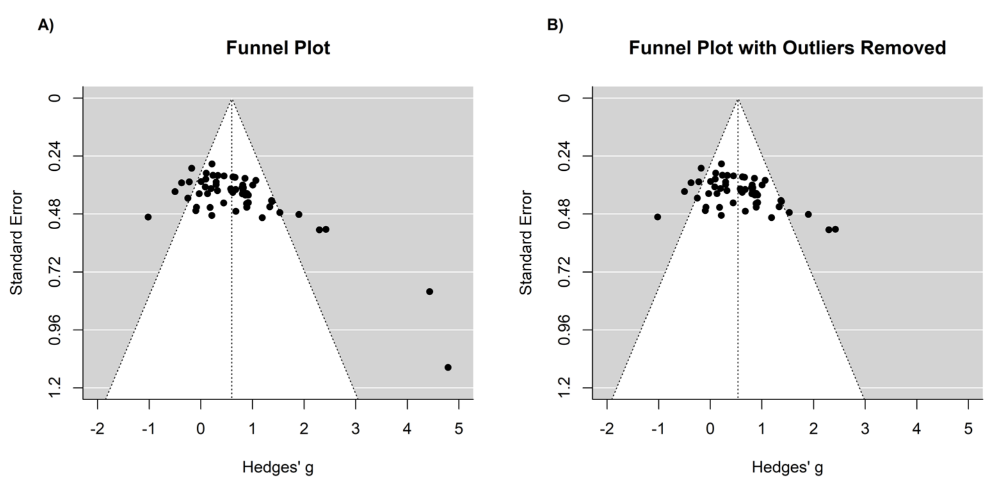
Before running the random-effects model to estimate the effect of enhanced expectancies on motor learning, funnel plot visual inspection and influence diagnostics statistics were carried out to identify the presence of outliers and/or overly influential cases in the dataset. Figure 3A shows a funnel plot depicting all 56 effect sizes as a function of their standard error distribution. Visual inspection indicated the presence of two outliers (see bottom right of plot), which was confirmed by inspection of studentized deleted residuals and hat values, resulting in the removal of the studies by Goudini et al. (2018; rstudent = 4.19, hat = 0.009) and Navaee et al. (2016; rstudent = 3.44, hat = 0.005) from the subsequent analyses. (Results of the main meta-analysis with all 56 effect sizes can be found in the supplementary material.) Figure 3B shows the funnel plot after removal of outliers/influential cases.

Figure 4 depicts a forest plot with the 54 effect sizes included in the main analyses as well as a summary of the estimated effect. Results of the random-effects model revealed an overall effect size of medium magnitude (Hedges' g = 0.54, 95% CI [0.38, 0.69], z = 6.85, p < .001), indicating a positive effect of enhanced expectancies on motor skill learning. The Cochran's Q test was also significant ($Q(53)$ = 118.27, p < .001), which suggests heterogeneity unlikely due to chance alone in the estimated effects across studies. This finding was corroborated by the results of the I^2 statistics, which revealed heterogeneity of I^2 = 55.27%. Funnel plot visual inspection indicated asymmetry even after outlier/overly influential case removal, which was confirmed by the results of the Egger's regression test (z = 3.49, p < .001).

Notably, asymmetry does not necessarily reflect small-study effects, but rather can occur by chance, sampling variation, and/or heterogeneity (Sterne et al., 2011). Since our funnel plot included 54 effect sizes, we reasoned chance and sampling variation were unlikely to have caused asymmetry. Thus, we were most concerned with exploring heterogeneity as an alternative to small-study effects as a cause of asymmetry, especially given the evidence of heterogeneity, possibly stemming from the use of studies implementing six different types of manipulations. If the asymmetry was mostly due to different types of manipulations having different effect sizes and standard errors, then funnel plots for each type of manipulation should be symmetrical. However, this does not seem to be the case, as described in the supplementary material, and depicted in Figure S2. Similarly, the asymmetry does not appear due to different populations (young adults, older adults, children/adolescents, and special populations) having different effect sizes and standard errors, as the funnel plots were not symmetrical for each population (see supplementary material and Figure S3). The trim-and-fill analysis failed to add studies to either side of the main funnel plot (Figure 3B).

Figure 3

Funnel Plot and Funnel Plot with Outliers Removed



Note. A: Funnel plot depicting cases as a function of effect size and standard error of all 56 effect sizes. B: Funnel plot depicting cases as a function of effect size and standard error after outlier removal (n = 54).

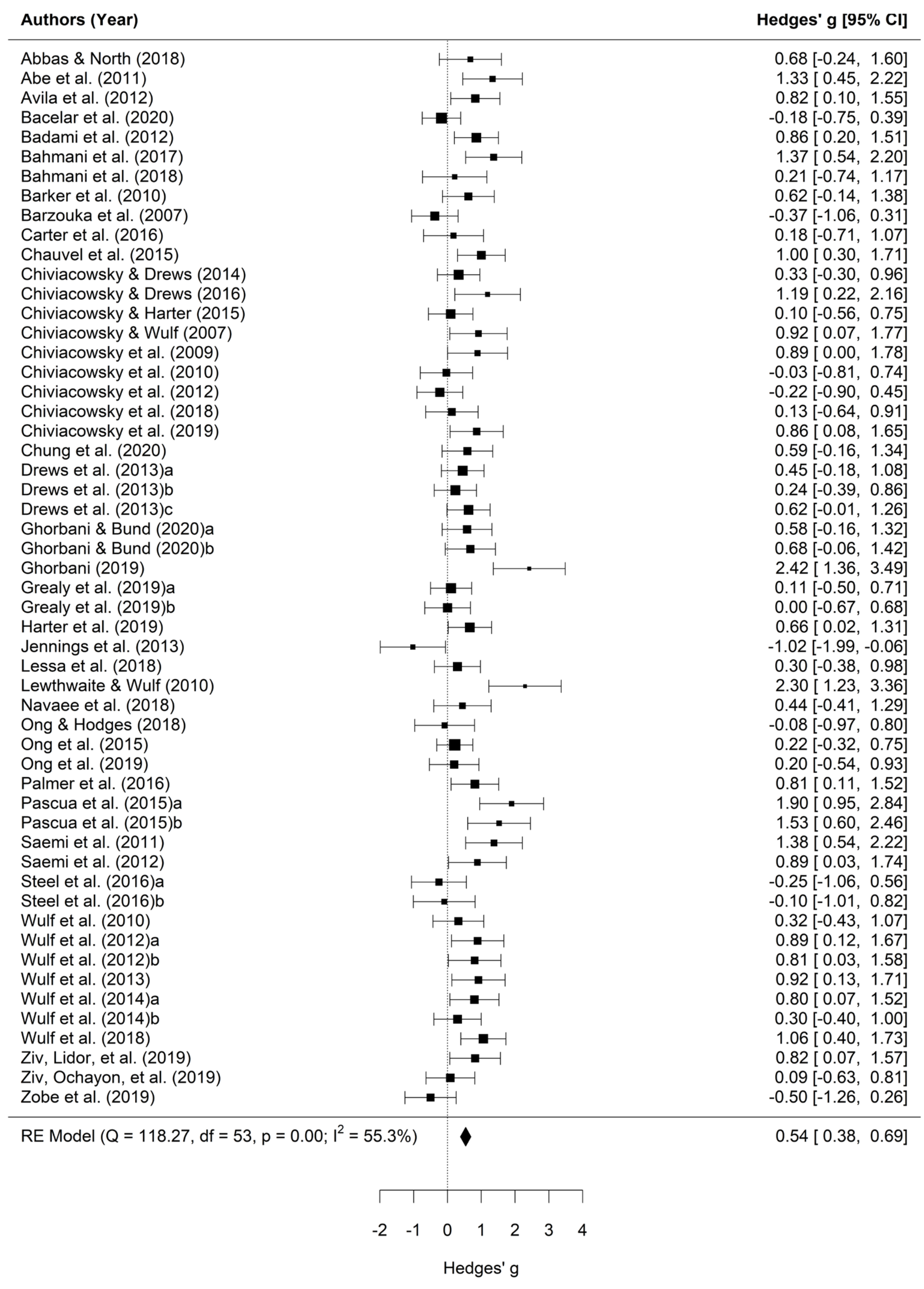
A moderator analysis was carried out to investigate the estimated effect size of enhanced expectancies on motor learning as a function of type of manipulation. (The same analysis with the 54 effect sizes included in the main analysis plus those considered outliers/overly influential can be found in the supplementary material.) Thus, type of manipulation (feedback after good trials, comparative feedback, self-modeling, perceived task difficulty, conceptions of ability, and extrinsic rewards) was entered into a mixed-effects model as a predictor. The moderator analysis revealed that at least one of the types of manipulations significantly differed from zero ($QM(6)$ = 66.15, $p < .001$). The estimated effect of feedback after good trials was of large magnitude (Hedges' g = 0.84, 95% CI [0.54, 1.14], z = 5.43, $p < .001$, n = 13), indicating a beneficial effect of feedback after good trials on motor learning. In the same direction, a medium effect of comparative feedback (Hedges' g = 0.61, 95% CI [0.34, 0.88], z = 4.39, $p < .001$, n = 15) and a small effect of perceived task difficulty (Hedges' g = 0.46, 95% CI [0.18, 0.74], z = 3.17, p = .002, n = 13) and conceptions of ability (Hedges' g = 0.39, 95% CI [0.023, 0.76], z = 2.083, p = .037, n = 7[9]) were found. Extrinsic rewards showed a trivial positive effect (Hedges' g = 0.15, 95% CI [-0.38, 0.68], z = 0.56, p = .577, n = 4), and self-modeling showed a moderate effect

[9] The study by Wulf et al. (2013) manipulated both conceptions of ability and feedback after good trials. Until this point, the effect size of this study reflected a combination of these two manipulations (acquirable-better group vs. inherent-worse group). However, for the purposes of this moderator analysis, we decided to categorize this study as 'conceptions of ability' by comparing the acquirable-worse group and inherent worse-group given that this manipulation had fewer cases (n = 6) than the feedback after good trials one (n = 13). (We chose to compare the acquirable- vs. inherent-*worse* groups because we reasoned the acquirable- and inherent-*better* groups may both have enhanced expectancies, with the latter believing they are naturally good at the task.) In the supplementary material, we present the results of a sensitivity analysis in which this study is classified as feedback after good trials (effect size reflecting the difference between the inherent-better and inherent-worse group).

favoring the comparison group (Hedges' g = -0.64, 95% CI [-1.40, 0.12], z = -1.64, p = .101, n = 2), thus failing to provide evidence that these manipulations improve motor learning.

Figure 4

Forest Plot



Note. Forest plot depicting all 54 effect sizes and their respective 95% confidence interval along with the overall Hedge's g effect size. Model summary is also presented on the bottom left side of the figure. Here, effect sizes favoring enhanced expectancies manipulations are presented on the right side of the zero Hedges' g line, whereas effect sizes not in favor of the manipulation in question are presented on the left side.

A second moderator analysis was conducted to identify the effects of enhanced expectancies as a function of the different types of comparison groups adopted (i.e., diminished expectancies group, $n = 25$; or control, $n = 29$). (The same analysis with the 54 effect sizes included in the main analysis plus those considered outliers/overly influential can be found in the supplementary material.) We did not find evidence that adding type of comparison to the model helped explain variability in effect sizes across studies ($QM(1) = 1.32$, $p = .251$). Specifically, we observed a medium positive effect when comparing enhanced expectancies to diminished expectancies (Hedges' $g = 0.63$, 95% CI [0.41, 0.86], $z = 5.53$, $p < .001$) and a small effect when comparing enhanced expectancies to control (Hedges' $g = 0.45$, 95% CI [0.24, 0.66], $z = 4.23$, $p < .001$), but the effect of enhanced expectancies was not significantly influenced by comparison group type ($\beta = -0.18$, 95% CI [-0.49, 0.13], $z = -1.15$, $p = .251$).

Finally, to explore the effect of enhanced expectancies as a function of different populations, type of population (young adults, older adults, children/adolescents, and special populations) was entered into a mixed-effect model as the predictor. (The same analysis with the 54 effect sizes included in the main analysis plus those considered outliers/overly influential can be found in the supplementary material.) The exploratory moderator analysis revealed that at least one of the populations significantly differed from zero ($QM(4) = 45.29$, $p < .001$). Specifically, a significant positive effect of medium magnitude was found for young adults

(Hedges' $g = 0.61$, 95% CI [0.40, 0.81], $z = 5.70$, $p < .001$, $n = 32$), older adults (Hedges' $g = 0.48$, 95% CI [0.01, 0.96], $z = 1.99$, $p = .046$, $n = 6$), and children/adolescents (Hedges' $g = 0.44$, 95% CI [0.12, 0.75], $z = 2.71$, $p = .007$, $n = 13$), suggesting enhanced expectancies has a beneficial effect for these populations. Although a medium positive effect was observed for special populations, we did not find sufficient evidence that enhanced expectancies improve learning in this population (Hedges' $g = 0.43$, 95% CI [-0.27, 1.14], $z = 1.20$, $p = .231$, $n = 3$).

Discussion

The present meta-analysis estimated that enhancing learners' expectancies for future successful outcomes has a medium-sized benefit on motor learning ($g = 0.54$, 95% CI [0.38, 0.69]). Specifically, when analyzing different methods of enhancing expectancies, we found that manipulating feedback after good trials ($g = 0.84$, 95% CI [0.54, 1.14]) results in large benefits, while comparative feedback ($g = 0.61$, 95% CI [0.34, 0.88]) entails medium-sized benefits, and perceived task difficulty ($g = 0.46$, 95% CI [0.18, 0.74]) as well as conceptions of ability ($g = 0.39$, 95% CI [0.023, 0.76]) result in small benefits to learning. We did not find evidence that manipulating extrinsic rewards or self-modeling affect motor learning (ps $\geq .101$), but few studies implemented these manipulations (ns ≤ 4), precluding reliable estimates of their effects. Thus, the effects of these manipulations should be estimated again when/if more studies in this line of investigation are conducted. (Since only 7 studies manipulated conceptions of ability and the effect of this manipulation has a wide CI that includes 0 when estimated among all 56 effect sizes (see Table S2), these results should be interpreted with caution.) Notably, enhanced expectancies benefitted motor learning similarly irrespective of whether the comparison group had diminished or neutral expectancies. This is consistent with Wulf and Lewthwaite (2016)'s suggestion that 'neutral' practice conditions are not really neutral, but rather likely elicit negative expectancies due to learners' concerns about having their performance assessed and compared with others'. Finally, we found that manipulating enhanced expectancies has a medium-sized positive effect on motor learning for young adults (g = 0.61, 95% CI [0.40, 0.81]), older adults (g = 0.48, 95% CI [0.01, 0.96]), and children/adolescents (g = 0.44, 95% CI [0.12, 0.75]). We did not find evidence to support the benefits of enhanced expectancies for special populations (p = .231), which in the present meta-analysis consist of adults with a disability in at least one upper or lower extremity (Bahmani et al., 2018), adults with Parkinson's disease (Chung et al., 2020), and children with autism (Navaee et al., 2018). However, only three studies examined these populations, preventing reliable estimates of effects in them. Future research should investigate the effect of enhanced expectancies on motor learning in these populations.

Results emphasize the role of enhanced expectancies in facilitating motor learning, so it is worth considering potential underlying mechanisms of this effect. Practice conditions that enhance expectations for successful outcomes are motivating, which increases dopamine release during motor skill practice, thereby facilitating the consolidation of motor memories (Wise, 2004). This is because successful outcomes are intrinsically rewarding, activating the dopaminergic reward system (Lutz et al., 2012), and humans are motivated to pursue rewards during motor skill practice (Moskowitz et al., 2020). Importantly, the mere expectation of dopamine release modulates the dopaminergic reward system (Schmidt et al., 2014), which is crucial for motivation (Wise, 2004).

The present meta-analysis also revealed evidence of small-study effects and underpowered studies, likely causing the effect of enhancing learners' expectancies on motor learning to be overestimated. Specifically, funnel plot visual inspection revealed asymmetry that was confirmed by a significant relationship between study effect size and standard error (Egger's regression test). We believe the asymmetry is unlikely caused by chance or sampling variation, since 54 effect sizes were used in the funnel plot. We explored the probability that asymmetry was due to different manipulations (feedback after good trials, comparative feedback, etc...) or different populations (young adults, older adults, etc…) having different effect sizes and standard errors by constructing funnel plots for each manipulation and population. We did not observe symmetry in each manipulation and population's funnel plot (Figures S2 and S3), making it unlikely that heterogeneity between manipulations or populations explains the asymmetry in the funnel plot with all manipulations and populations (Sterne et al., 2011). Evidence that small-study effects contribute to funnel plot asymmetry can be observed in the lack of relatively imprecise studies showing negative effects (Figure 3B). In particular, asymmetry may be due to inflated effect sizes in small studies, since the median sample size was n = 14/group, and such small studies are likely to have exaggerated effect sizes (Sterne et al., 2011). Notably, the combination of small samples and small-study effects may cause effect sizes to be severely overestimated in the extant literature. This follows because small studies are likely to be underpowered such that only those drastically overestimating an effect will be statistically significant and, consequently, published (Lohse et al., 2016). However, it is important to note that the present meta-analysis did not assess the gray literature, and, therefore, does not present direct evidence of reporting bias.

The risk of bias assessment also raises the possibility that the effect of enhancing learners' expectancies on motor learning could be misrepresented. Some concerns, such as those about the randomization process, may be due to authors not reporting procedures rather than not undertaking them (The Cochrane Collaboration, 2013), and other concerns are inherent to motor learning research, such as participant awareness of group assignment. However, certain concerns can be mitigated, such as those regarding bias in selection of the reported result. Thus, to estimate the effect of enhancing learners' expectancies on motor learning more accurately, we recommend researchers conduct pre-registered studies and registered reports with a priori sample size calculations (Caldwell et al., 2020; Lohse et al., 2016). Specifically, pre-registered studies and registered reports may reduce reporting bias by committing researchers to reporting specific analyses and outcomes and journal editors to publishing studies irrespective of their results. Researchers conducting a priori sample size calculations should consider this meta-analysis' effect sizes to be overestimated and are encouraged to power their studies to detect effects close to the lower bound of the 95% CI. According to G*Power 3.1.9.4 (Faul et al., 2007), a two-tailed independent sample *t*-test with $\alpha = .05$, $\beta = .20$, equal *n*/group, and a Cohen's $d = 0.38$ (lower bound of 95% CI) requires $n = 110$/group. This number is reduced to $n = 55$/group if a Cohen's *d* is set to 0.54, consistent with the effect size (likely overestimated) in the present study. Since these sample sizes will be large increases for most researchers, they are encouraged to consider ways to make their data collections more efficient, for example by using sequential analyses (Lakens, 2014).

The present results suggesting enhanced expectancies may facilitate motor learning, with the effect possibly overestimated due to small-study effects and small sample sizes, are somewhat like other recent meta-analyses of effects predicted by OPTIMAL theory. Jimenez-Diaz et al. (2020) investigated the effect of learner control of augmented feedback during acquisition, which may promote autonomy, on motor performance and learning. The authors reported learner-controlled feedback groups exhibited superior acquisition performance relative to experimenter-regulated feedback groups, and learner-controlled feedback groups demonstrated performance stability from acquisition to retention, whereas experimenter-regulated feedback groups showed performance decrement from acquisition to retention. However, learner-controlled feedback groups did not significantly differ in performance or learning in comparison to yoked feedback groups, which consisted of participants who received augmented feedback schedules matched to a counterpart in a learner-controlled group. Thus, results provide little support for the OPTIMAL theory prediction that promoting autonomy, via giving learners control of their augmented feedback, enhances motor performance or learning. Notably, the authors reported a small median sample size of approximately n = 12/group as well as funnel plot asymmetry and significant Egger's regression tests for both acquisition and retention data, indicating the possibility of small-study effects. Kim et al., (2017) examined the effect of external focus (on the effects of one's movement) vs. internal focus (on one's body movements) instructions on balance performance and learning. Consistent with OPTIMAL theory (Wulf & Lewthwaite, 2016), the authors reported external focus of attention groups exhibited superior balance during acquisition, retention, and transfer relative to internal focus of attention groups. The authors reported a small median sample size of approximately n = 14/group as well as funnel plot asymmetry and a significant Egger's regression test in the acquisition data but not in the retention data, indicating the possibility of bias in the former. (Funnel plot asymmetry was not assessed for transfer data.) Makaruk et al. (2020) investigated the effect of external vs. internal vs. control (no) attentional focus instructions on jumping performance but not learning. Consistent with OPTIMAL theory, the authors reported external focus of attention was superior to internal focus of attention and control conditions. The authors reported a median sample size of approximately n = 24 (14 of 15 studies were within-subjects), which is larger than the other meta-analyses. Unlike the other meta-analyses, the authors did not assess bias. Taken together, these meta-analyses and the present one suggest that the many individual studies reporting effects consistent with OPTIMAL theory (Wulf & Lewthwaite, 2016) exaggerate the supporting evidence, due to small-study effects and underpowered studies, which is common in motor learning (Lohse et al., 2016) and other fields (e.g., Button et al., 2013a). Aggregating individual studies to estimate effects more accurately with meta-analyses is an important endeavor, but the presence of bias and an environment conducive to questionable research practices (e.g., conducting many statistical tests) in motor learning (Lohse et al., 2016) makes it difficult for OPTIMAL theory-based or other motor learning meta-analyses to establish whether even medium-sized effects, such as the one observed in the present study, are truly different from zero (E. C. Carter et al., 2019).

An important question for future research is to what degree practitioners typically implement strategies that enhance expectancies in comparison to those that are neutral or diminish expectancies. If coaches/clinicians rarely create neutral practice conditions or those that diminish expectancies, then their adoption of strategies to enhance expectancies will have little added value. Notably, researchers have investigated whether coaches use external focus of attention instructions, as recommended by OPTIMAL theory, and revealed that they usually do not (Diekfuss & Raisbeck, 2016; Porter et al., 2010; Yamada et al., 2020). Thus, it is conceivable practitioners also fail to create practice conditions that enhance expectancies.

The present meta-analysis suggests that enhancing learners' expectancies for future successful outcomes may facilitate motor learning across young adults, older adults, and children/adolescents. The meta-analysis lacked studies manipulating extrinsic rewards and self-modeling (and, to a lesser degree, conceptions of ability), and studies investigating the effect in question in special populations, so these effects should be estimated again when/if more studies in this line of investigation are conducted. As the meta-analysis indicated small-study effects and small sample sizes, pre-registered analyses and/or registered reports with greater statistical power are recommended. This final recommendation is critical to develop a body of studies conducive to accurately estimating the effect of enhanced expectancies on motor learning as well as other effects predicted by OPTIMAL theory and other motor learning theories.

Table 1

Summary of the Main Characteristics of the Studies Included in the Meta-Analysis

Study	*Manipulation*	*Population*	*Sample Size*	*Comparison Group*	*Type of Task*	*Training Session*	*Retention*	*Chosen Performance Outcome*
Abbas & North (2018)	Feedback after good trials	Adults (Age: *M* = 29.67 years, *SD* = 9.36; 14 females)	KR-good: *n* = 10 KR-poor: *n* = 10 KR-neutral: *n* = 10 (Total: *N* = 30)	KR-good vs KR-neutral (Control)	Golf-putting	5 blocks of 6 trials at 2 meters 5 blocks of 6 trials at 5 meters	24-hr (1 block of 10 trials at 2 m and 1 block of 10 trials at 5 m) 1-week (1 block of 10 trials at 2 m and 1 block of 10 trials at 5 m)	Radial error
Abe et al. (2011)	Extrinsic rewards	Adults (Age: *M* = 24.3 years, *SD* = 5.2; 18 females)	Rewarded training: *n* = 13 Punished training: *n* = 12 Control training: *n* = 13 (Total: *N* = 38)	Rewarded training vs Control training (Control)	Tracking pinch force	4 blocks of 10 trials	24h and 30 days (1 block 20 trials)	Error (distance)
Ávila et al. (2012)	Comparative feedback	Children (Age: *M* = 10.4 years, *SD* =	Positive feedback: *n* = 16 Control: *n* = 16	Positive feedback vs Control (Control)	Non-dominant arm beanbag	6 blocks of 10 trials	24-hr (1 block of 10 trials)	Accuracy score

Study	*Manipulation*	*Population*	*Sample Size*	*Comparison Group*	*Type of Task*	*Training Session*	*Retention*	*Chosen Performance Outcome*
		0.36; 12 females)	(Total: N = 32)		throwing			
Bacelar et al. (2020) - Main exp.	Extrinsic rewards	Adults (Age: M = 20.7 years, SD = 2.63; 55 females)	Reward: n = 25 Punishment: n = 22 Neutral: n = 22 (Total: N = 69)	Reward vs Neutral (Control)	Golf-putting	6 blocks of 8 trials	24h and 1 week (1 block of 8 trials)	Radial error
Badami et al. (2012)	Feedback after good trials	Adults (Age: M = 19.5 years, SD = 1.9; all females)	More Accurate: n = 20 Less Accurate: n = 20 (Total: N = 40)	More accurate vs Less accurate (diminished expectancies)	Golf-putting	10 blocks of 6 trials	24 h (1 block of 10 trials)	Putting accuracy scores
Bahmani et al. (2017)	Perceived task difficulty	Children (Age: M = 10.66 years, SD = 0.41; all males)	Perceived large hole: n = 15 Perceived small hole: n = 15 (Total: N = 30)	Perceived large hole vs Perceived small hole (Diminished expectancies)	Golf-putting	5 blocks of 10 trials	48-hr (1 block of 10 trials)	Deviation
Bahmani et al. (2018)	Perceived task difficulty	Adults with disability in ≥1 upper or lower extremity (Age: M = 37.7 years, SD =	Large illusion: n = 9 Small illusion: n = 8 (Total: N = 17)	Large illusion vs Small illusion (Diminished expectancies)	Aiming task (shooting - 10-m air pistol and air rifle)	5 blocks of 10 trials	24-hr (1 block of 10 trials)	Shooting accuracy

Study	*Manipulation*	*Population*	*Sample Size*	*Comparison Group*	*Type of Task*	*Training Session*	*Retention*	*Chosen Performance Outcome*
		9.8; 11 females)						
Barker et al. (2010)	Perceived task difficulty	Adults (Age: *M* = 21.50 years, *SD* = 3.25; 4 females)	Hypnosis: *n* = 14 Video attention control: *n* = 14 (Total: *N* = 28)	Hypnosis vs Video attention control (Control)	Soccer Wall-Volley	3 sessions each comprising soccer practice (3 trials), manipulation (45 min), and soccer practice (3 trials)	4 weeks (1 block of 3 trials)	Performance score
Barzouka et al. (2007)	Self-modeling	Adolescents (Age: *M* = 13.1 years, *SD* = 0.9; all females)	Other-modeling: *n* = 18 Self-modeling: *n* = 16 Control: *n* = 19 (Total: *N* = 53)	Group 2 vs Group 1 (Control)	Volleyball reception	12 practice sessions at a frequency of 2x/week; four kinds of drills with 10 repetitions each	1-week (1 block of 10 trials)	Performance outcome (score)
Carter et al. (2016)	Feedback after good trials	Adults (Age: *M* = 22.72 years, *SD* = 1.65; 22 females)	KR-good-aware: *n* = 10 KR-good-unaware: *n* = 10 KR-poor-aware: *n* = 10 KR-poor-unaware: *n* = 10	KR-good-unaware vs KR-poor-unaware (Diminished	Mini Koosh-ball tossing	10 blocks of 6 trials	24h (2 blocks of 6 trials)	Radial error

Study	***Manipulation***	***Population***	***Sample Size***	***Comparison Group***	***Type of Task***	***Training Session***	***Retention***	***Chosen Performance Outcome***
			(Total: $N = 40$)	expectancies)				
Chauvel et al. (2015)	Perceived task difficulty	Adults (Age: M = 21.7 years, SD = 1.24; 20 females)	Perceived large hole: n = 18 Perceived small hole: n = 18 (Total: $N = 36$)	Perceived large hole vs Perceived small hole (Diminished expectancies)	Golf-putting	5 blocks of 10 trials	24-hr (1 block of 10 trials)	Deviation
Chiviacowsky & Drews (2014) – Exp. 2	Conceptions of ability	Children (Age: M = 10.5 years, SD = 0.51; 20 females)	Generic feedback: $n = 20$ Non-generic feedback: n = 20 (Total: $N = 40$)	Generic feedback vs Non-generic feedback (Diminished expectancies)	Non-dominant arm beanbag throwing	4 blocks of 10 trials	Retention 1: 24-hr (1 block of 10 trials)[10] Retention 2: 24-hr (1 block of 10 trials)	Accuracy score
Chiviacowsky & Drews (2016)	Comparative feedback	Adults (Age: M = 21.6 years, SD = 1.98; 4 females)	Positive self-comparison feedback: $n = 10$ Negative self-comparison feedback: $n = 10$ (Total: $N = 20$)	Positive self-comparison feedback vs Negative self-comparison feedback	Anticipatory coincident timing	4 blocks of 10 trials	24h (1 block of 10 trials)	Absolute error

[10] For the purposes of the present meta-analysis only Retention 1 was used.

Study	Manipulation	Population	Sample Size	Comparison Group	Type of Task	Training Session	Retention	Chosen Performance Outcome
				(Diminished expectancies)				
Chiviacowsky & Harter (2015)	Perceived task difficulty	Adults (Age: *M* = 24.4 years, *SD* = 6.73; 24 females)	High experience of success: *n* =18 Low experience of success: *n* = 18 Control: *n* = 18 (Total: *N* = 54)	High experience of success vs Control (Control)	Anticipatory coincident timing	6 blocks of 5 trials	24-hr (1 block of 10 trials)	Absolute error
Chiviacowsky & Wulf (2007)	Feedback after good trials	Adults (Age: *M* = 21.1 years, *SD* = NA; 18 females)	KR good: *n* = 12 KR poor: *n* = 12 (Total: *N* = 24)	KR good vs KR poor (Diminished expectancies)	Non-dominant arm beanbag tossing	10 blocks of 6 trials	24 h (1 block of 10 trials)	Accuracy score
Chiviacowsky et al. (2009)	Feedback after good trials	Older adults (Age: *M* = 65.9 years, *SD* = NA; all females)	KR-good: *n* = 11 KR-poor: *n* = 11 (Total: *N* = 22)	KR-good vs KR-poor (Diminished expectancies)	Non-dominant arm beanbag tossing	10 blocks of 6 trials	72-hr (1 block of 10 trials)	Accuracy score
Chiviacowsky et al. (2010)	Feedback after good trials	Children (Age: *M* = 10 years, *SD* = NA; *ratio males/females*	CRB (KR after good trials): *n* = 13 CRM (KR after poor	CRB vs CRM (Diminished expectancies)	Pedalo	8 blocks of 4 trials (7 meters)	24h (1 block of 4 trials)	Time

Study	*Manipulation*	*Population*	*Sample Size*	*Comparison Group*	*Type of Task*	*Training Session*	*Retention*	*Chosen Performance Outcome*
		not reported	trials): n = 13 (Total: N = 26)					
Chiviacowsky et al. (2012)	Perceived task difficulty	Adults (Age: M = 21.8 years, SD = 3.36; 24 females)	Self-30: n = 17 Self-4: n = 17 Self: n = 17 (Total: N = 51)	Self-30 vs Self (Control)	Anticipatory timing	3 blocks of 10 trials	24h (1 block of 10 trials)	Absolute error
Chiviacowsky et al. (2018)	Perceived task difficulty	Older adults (Age: M = 66.1 years, SD = 4.78; all females)	Negative stereotype: n = 13 Positive stereotype: n =13 Control: n = 13 (Total: N = 39)	Positive stereotype vs Control (Control)	Stabilometer	1 block of 10 trials	24h (1 block of 5 trials)	Time in balance
Chiviacowsky et al. (2019)	Comparative feedback	Adults (Age: M = 23.2 years, SD = 6.71; 14 females)	Positive temporal-comparative feedback: n = 14 Control: n = 14 (Total: N = 28)	Positive temporal-comparative feedback vs Control (Control)	Golf-putting	5 blocks of 10 trials	24h (1 block of 10 trials)	Deviation
Chung et al. (2020)	Conceptions of ability	Individuals with Parkinson's Disease (Age: M = 62.36	Incremental theory: n = 15 Incremental theory plus	Incremental theory vs Control (Control)	Stabilometer	1 block of 14 trials (30-s trial)	24-hr (1 block of 7 30-s trials)	Time in balance

Study	*Manipulation*	*Population*	*Sample Size*	*Comparison Group*	*Type of Task*	*Training Session*	*Retention*	*Chosen Performance Outcome*
		years, *SD* = 9.80; 18 females)	success criteria: *n* = 15 Control: *n* = 14 (Total: *N* = 44)					
Drews et al. (2013)	Conceptions of ability	Children (Age 6: *M* = 6.2 years, *SD* = 0.24; Age 10: *M* = 10.1 years, *SD* = 0.30; Age 14: *M* = 14.4 years, *SD* = 0.34; 54 females)	Acquirable-skill-6: *n* = 20 Inherent-ability-6: *n* = 20 Acquirable-skill-10: *n* = 20 Inherent-ability-10: *n* = 20 Acquirable-skill-14: *n* = 20 Inherent-ability-14: *n* = 20 (Total: *N* = 120)	Acquirable-skill-6 vs Inherent-ability-6 (Diminished expectancies – Drew et al. (2013[a])) Acquirable-skill-10 vs Inherent-ability-10 (Diminished expectancies – Drew et al., 2013[b]) Acquirable-skill-14 vs Inherent-ability-14 (Diminished expectancies – Drew et al., 2013[c])	Overhand non-dominant arm beanbag throwing	4 blocks of 10 trials	24-hr (1 block of 10 trials)	Accuracy score
Ghorbani & Bund	Feedback after	Adults (Age: *M* =	Good KR and High Self-	Good KR and High	Non-dominant	10 blocks of 6	24h (1 block of	Accuracy scores

Study	*Manipulation*	*Population*	*Sample Size*	*Comparison Group*	*Type of Task*	*Training Session*	*Retention*	*Chosen Performance Outcome*
(2020)	good trials	21.35 years, *SD* = 1.86; all males)	Efficacy (SE): *n* = 15 Poor KR and High SE: *n* = 15 Good KR and Low SE: *n* = 15 Poor KR and Low SE: *n* = 15 (Total: *N* = 60)	SE vs Poor KR and High SE (Diminished expectancies- Ghorbani & Bund., 2020[a]) Good KR and Low SE vs Poor KR and Low SE (Diminished expectancies- Ghorbani & Bund., 2020[b])	arm beanbag throwing	trials	10 trials)	
Ghorbani (2019) – Exp. 1	Feedback after good trials	Adults (Age range: 18-24 years; all males)	KR-good: *n* = 12 KR-bad: *n* = 12 Control: *n* = 12 (Total: *N* = 36)	KR-good vs KR-bad (Diminished expectancies)	Underarm dart-throwing	10 blocks of 6 trials	24-hr (1 block of 10 trials)	Accuracy score
Goudini et al.	Feedback after	Adults (Age: *M* =	KR after good trials: *n* = 9	KR after good trials	Line tracking	11 blocks of 6	48h (1 block of	Duration of errors

Study	*Manipulation*	*Population*	*Sample Size*	*Comparison Group*	*Type of Task*	*Training Session*	*Retention*	*Chosen Performance Outcome*
(2018)	good trials	24.66 years, *SD* = 1.35; 4 females)	KR after poor trials: *n* = 9 (Total: *N* = 18)	vs KR after poor trials (Diminished expectancies)		trials (15-s trial)	10 trials)	
Grealy et al. (2019)	Comparative feedback	Adults (Age: *M* = 22.38 years, *SD*: 2.32; 28 females – Grealy et al., 2019[a]) Older adults (Age: *M* = 71.65 years, *SD*: 4.28; 23 females – Grealy et al., 2019[b])	Young false positive: *n* = 21 Young veridical: *n* = 21 (Total: *N* = 42; Grealy et al., 2019[a]) Older false positive: *n* = 17 Older veridical: *n* = 17 (Total: *N* = 34; Grealy et al., 2019[b])	Young false positive vs Young veridical (Control – Grealy et al., 2019[a]) Older false positive vs Older veridical (Control – Grealy et al., 2019[b])	Inhibitory-action task (Simon task)	18 blocks of 50 trials completed over 6 training sessions (3 blocks/session)	Two-week (3 blocks of 50 trials)	Inhibition time
Harter et al. (2019)	Conceptions of ability	Children (Age: *M* = 9.6 years, *SD* = 0.11; all females)	Acquirable-skill: *n* = 20 Inherent-ability: *n* = 20 (Total: *N* = 40)	Acquirable-skill vs Inherent-ability (Diminished expectancies)	Pirouette en dehors	3 blocks of 5 trials	24-hr (1 block of 5 trials)	Punctuation scores
Jennings et al. (2013)	Self-modeling	Adolescents (Age: *M* = 13.6 years, *SD*:	Traditional approach: *n* = 10	Traditional approach vs Self-	Cycling standing start	4 one-hour training sessions	48-hr (1 trial)	Standing start time

Study	*Manipulation*	*Population*	*Sample Size*	*Comparison Group*	*Type of Task*	*Training Session*	*Retention*	*Chosen Performance Outcome*
		1.6; 7 females)	Self-modeling intervention: n = 9 (Total: N = 19)	modeling intervention (Control)		over a 2-week period		
Lessa et al. (2018)	Comparative feedback	Older adults (Age: M = 66.14 years, SD = 4.63; 30 females)	Positive temporal-comparative feedback: n = 17 Control: n = 17 (Total: N = 34)	Positive temporal-comparative vs Control (Control)	4-meter walking speed	4 blocks of 10 trials	24 h (1 block of 10 trials)	Absolute error
Lewthwaite & Wulf (2010)	Comparative feedback	Adults (Age: M = 23.0 years, SD = 2.26; 24 females)	Better: n = 12 Worse: n = 12 Control: n = 12 (Total: N = 36)	Better vs Control (Control)	Stabilometer	2 days with 7 trials (90-s trials)	24h (1 block of 7 trials)	Root Mean Square Error
Navaee et al. (2016)	Comparative feedback	Adults (Age: M = 22.60 years, SD = 1.89; *information about gender not reported*)	Normative positive feedback: n = 10 Normative negative feedback: n = 10 Control: n = 10 (Total: N = 30)	Normative positive feedback vs Control (Control)	Balance	16 blocks of 10 trials for 4 consecutive days (40 trials/day)	24-hr (*number of trials not reported)*	Overall stability

Study	***Manipulation***	***Population***	***Sample Size***	***Comparison Group***	***Type of Task***	***Training Session***	***Retention***	***Chosen Performance Outcome***
Navaee et al. (2018)	Comparative feedback	Autistic children (Age[11] range: 6-10, *M* = NA, *SD*: NA; *information about gender not reported*)	Normative feedback: *n* = 10 Control: *n* = 10 (Total: *N* = 20)	Normative feedback vs Control (Control)	Non-dominant arm overhead beanbag throwing	6 blocks of 10 trials	24-hr (1 block of 10 trials)	Mean score
Ong & Hodges (2018) - Exp 2a.	Comparative feedback	Adults (Age: *M* = 21.1 years, *SD* = 3.4; all females)	Positive: *n* = 10 Positive-control: *n* = 10 (Total: *N* = 20)	Positive vs Positive-control (Control)	Stabilometer	1 block of 7 trials (60-s trial)	24 h (1 block of 7 trials)	Root Mean square Error
Ong et al. (2015)	Perceived task difficulty	Adults (Age: *M* = NA, *SD* = NA; all females)	Large target: *n* = 28 Small target: *n* = 27 (Total: *N* = 55)	Large target vs Small target (Diminished expectancies)	Dart-throwing	10 blocks of 9 trials	1-week (block of 9 trials)	Radial error
Ong et al. (2019)	Perceived task difficulty	Adults (Age: *M* = 21.4 years, Age range: 18-31 years;	Large-target: *n* = 14 Small-target: *n* = 15 (Total: *N* = 29)	Large-target vs Small-target (diminished	Dart-throwing	10 blocks of 9 trials	24-hr (1 block of 6 trials – no-vision retention	Absolute error

[11] This paper reported mean and SD by group as follows: Normative feedback: 8.40±0.96, Control: 8.50±0.84.

Study	*Manipulation*	*Population*	*Sample Size*	*Comparison Group*	*Type of Task*	*Training Session*	*Retention*	*Chosen Performance Outcome*
		all females)		expectancies)			test)[12] 24-hr (1 block of 9 trials – with vision)	
Palmer et al. (2016)	Perceived task difficulty	Adults (Age: M = 24.6 years, SD = 5.20; 22 females)	Large-target: n = 17 Small-target: n = 17 (Total: N = 34)	Large-target vs Small-target (Diminished expectancies)	Golf-putting	5 blocks of 10 trials	24-hr (1 block of 12 trials)	Deviation
Pascua et al. (2015)	Comparative feedback	Adults (Age: M = 21.5 years, SD = 1.22; 31 females)	External focus/enhanced expectancy: n = 13 External focus: n = 13 Enhanced expectancy: n = 13 Control: n = 13 (Total: N = 52)	Enhanced expectancy vs Control (Control – Pascua et al. 2015[a]) & External focus/enhanced expectancy vs External focus (Control - Pascua et	Non-dominant arm overarm throwing (tennis ball)	6 blocks of 10 trials	24-hr (1 block of 10 trials)	Throwing accuracy scores

[12] For the purposes of the present meta-analysis only the 24-hr retention test with no vision was used.

Study	*Manipulation*	*Population*	*Sample Size*	*Comparison Group*	*Type of Task*	*Training Session*	*Retention*	*Chosen Performance Outcome*
				al. 2015[b])				
Saemi et al. (2011)	Feedback after good trials	Children (Age: *M* = 10.61 years, *SD*: 0.88; *information about gender not reported*)	KR-good: *n* = 14 KR-poor: *n* = 14 (Total: *N* = 28)	KR-good vs KR-poor (Diminished expectancies)	Overhand non-dominant arm beanbag throwing	10 blocks of 6 trials	24-hr (1 block of 10 trials)	Accuracy score
Saemi et al. (2012)	Feedback after good trials	Adults (Age: *M* = 19.51 years, *SD* = 1.09; all males)	KR after good trials: *n* = 12 KR after poor trials: *n* = 12 (Total: *N* = 24)	KR after good trials vs KR after poor trials (Diminished expectancies)	Non-dominant arm tennis ball tossing	10 blocks of 6 trials	24h (1 block of 10 trials)	Accuracy scores
Steel et al. (2016)	Extrinsic rewards	Adults (Age: *M* = 25 years, *SD* = 4.25; 47 females)[13]	Serial Reaction Time Task (SRTT) (Steel et al., 2016[a]): Reward: *n* = 12 Punishment: *n* = 12 Control: *n* = 12	Reward vs Control (Control)	SRTT FTT	SRTT (Steel et al., 2016[a]): Training: 6 blocks of 96 trials FTT (Steel et al., 2016[b]):	SRTT (Steel et al., 2016[a]): 24-hr and 30-day (3 blocks of 96 trials; sequence: random-fixed-	SRTT (Steel et al., 2016[a]): Reaction time FTT (Steel et al., 2016[b]): Squared error

[13] Authors did not provide information about age (mean and standard deviation) and gender separately for each task. Thus, the information presented is based on the total sample size of 72 participants.

Study	*Manipulation*	*Population*	*Sample Size*	*Comparison Group*	*Type of Task*	*Training Session*	*Retention*	*Chosen Performance Outcome*
			(Total: N = 36) Force-Tracking Task (FTT) (Steel et al., 2016[b]): Reward: n = 9 Punishment: n = 11 Control: n = 10 (Total: N = 30)			Training: 6 blocks of 8 trials (12-s trial)	random) FTT (Steel et al., 2016[b]): 24-hr and 30-day (3 blocks of 8 trials; sequence: random-fixed-random)	
Wulf et al. (2010)	Comparative feedback	Adults (Age: M = 20.8 years, SD = 3.53; 12 females)	Better: n = 14 Worse: n = 14 (Total: N = 28)	Better vs Worse (Diminished expectancies)	Computerized sequential timing	8 blocks of 10 trials	24-hr (1 block of 10 trials)	Overall timing error
Wulf et al. (2012) – Exp 1.	Comparative feedback	Older adults (Age: M = 71.1 years, SD = 5.25; all females)	Normative feedback: n = 15 Control: n = 14 (Total: N = 29)	Normative feedback vs Control (Control – Wulf et al., 2012[a])	Stabilometer	1 block of 10 trials (30-s trial)	24-hr (1 block of 5 trials)	Time in balance
Wulf et al. (2012) – Exp 2.	Perceived task difficulty	Older adults (Age: M = 63.6 years, SD = 3.40; all females)	Enhanced expectancies: n = 14 Control: n = 14	Enhanced expectancies vs Control (Control -	Stabilometer	1 block of 10 trials (30-s trial)	24-hr (1 block of 5 trials)	Time in balance

Study	*Manipulation*	*Population*	*Sample Size*	*Comparison Group*	*Type of Task*	*Training Session*	*Retention*	*Chosen Performance Outcome*
			(Total: *N* = 28)	Wulf et al., 2012[b])				
Wulf et al. (2013)	Conceptions of ability and feedback after good trials	Adults (Age: *M* = 22.3 years, *SD* = 2.25; 36 females)	Inherent-ability better: *n* = 14 Inherent-ability worse: *n* = 14 Acquirable-skill better: *n* = 14 Acquirable-skill worse: *n* = 14 (Total: N = 56)	Acquirable-skill better vs Inherent-ability worse (Diminished expectancies)	Stabilometer	2 days with 7 trials (90-s trials)	24h (1 block of 7 trials)	Root Mean Square Error
Wulf et al. (2014)	Comparative feedback	Adolescents (Age: *M* = 16.7 years, *SD* = 1.14; 28 females)	Autonomy support/enhanced expectancies: *n* = 16 Autonomy support: *n* = 16 Enhanced expectancies: *n* = 16 Control: *n* = 16 (Total: *N* = 64)	Enhanced expectancies vs Control (Control – Wulf et al. 2014[a]) & Autonomy support/enhanced expectancies vs Autonomy support	Non-dominant arm overhand throwing (beach tennis ball)	6 blocks of 10 trials	24-hr (1 block of 10 trials)	Accuracy score

Study	***Manipulation***	***Population***	***Sample Size***	***Comparison Group***	***Type of Task***	***Training Session***	***Retention***	***Chosen Performance Outcome***
				(Control - Wulf et al. 2014[b])				
Wulf et al. (2018)	Comparative feedback	Adults (Age: *M* = 22.8 years, *SD* = 3.87; 20 females)	Enhanced expectancy and autonomy support: *n* = 15 Enhanced expectancy and external focus: *n* = 15 Autonomy support and external focus: *n* = 15 Enhanced expectancy, autonomy support, and external focus: *n* = 15 (Total: *N* = 60)	Enhanced expectancy, autonomy support, and external focus vs Autonomy support and external focus (Control)	Beach tennis-ball throwing	6 blocks of 10 trials	24h (1 block of 10 trials)	Accuracy scores
Ziv, Lidor, et al. (2019)	Perceived task difficulty	Adults (Age: *M* = 23.90 years, *SD* = 2.7; 32 females)	Large circle: *n* = 15 Small-circle: *n* = 15 Control: *n* = 15 (Total: *N* = 45)	Large circle vs Control (Control)	Golf-putting	5 blocks of 10 trials	48-hr (1 block of 12 trials)	Radial error
Ziv, Ochayon, et al. (2019)	Perceived task difficulty	Adults (Age: *M* = NA, *SD* = NA; all males)	Large-circle: *n* = 15 Small-circle: *n* = 15 Control: *n* = 15	Large-circle vs Control (Control)	Golf-putting	5 blocks of 10 trials	48-hr (1 block of 12 trials)	Absolute error

Study	*Manipulation*	*Population*	*Sample Size*	*Comparison Group*	*Type of Task*	*Training Session*	*Retention*	*Chosen Performance Outcome*
			(Total: $N = 45$)					
Zobe et al. (2019)	Comparative feedback	Adults (Age: M = 22.5 years, SD = 2.8; 20 females)	Normative-Positive-Group: $n = 14$ Normative-Negative-Group: $n = 14$ Passive-Control-Group: $n = 14$ (Total: $N = 42$)	Normative-positive-group vs Normative-negative-group[14] (Diminished expectancies)	Elbow-extension-flexion sequence with three movement reversals at 70°, 20°, and 70°	5 sessions (15 blocks total): session 1 was comprised of 3 blocks of 38 trials and sessions 2-5 were comprised of 3 blocks of 48 trials per session	48-72-hr (1 block of 6 trials)	Absolute error

Note. *NA* indicates information was not available. *KR* indicates knowledge of results. The retention test closest to 24-hr was used in the meta-analysis.

[14] The Passive-Control-Group did not go through the training session, hence our decision to compare the Normative-Positive- Group to the Normative-Negative-Group.

Chapter 3: Dissociating the contributions of motivational and information processing factors to the self-controlled feedback learning benefit

Introduction

Giving learners control over the delivery of augmented feedback has been shown to enhance motor skill learning (Sanli et al., 2013). This self-controlled feedback benefit has been found across a variety of tasks such as timed key-pressing (Chiviacowsky & Wulf, 2002), walking (Huet et al., 2009), and throwing tasks (Chiviacowsky et al., 2008). Typically, self-controlled feedback paradigms have at least an experimental group (referred to as self-control) that is allowed to choose whether they want to receive feedback after each trial, and a control group (referred to as yoked) that receives feedback in a matched schedule to a self-control counterpart. That is, learners in the yoked group are paired with learners in the self-control group and receive feedback in the same temporal order as requested by their self-control counterpart. Thus, any potential differences between groups found in post-test assessments (retention and/or transfer tests) can be more reliably attributed to the exertion of choice over feedback delivery rather than feedback delivery schedule.

Although there have been more than 40 self-controlled feedback experiments reported since the initial one (Janelle et al., 1995; McKay et al., in press) there is no consensus on the mechanisms underlying the self-controlled feedback benefit. Currently, two explanations dominate the discussion around what is driving the benefit (Sanli et al., 2013). The motivational explanation argues that motor learning is facilitated in self-controlled protocols due to the fulfillment of the basic psychological need for autonomy (Ryan & Deci, 2000). As postulated by the Optimizing Performance Through Intrinsic Motivation and Attention for Learning (OPTIMAL) theory (Wulf & Lewthwaite, 2016), practice conditions that provide learners with an opportunity for choice support their need for autonomy, thereby increasing their intrinsic motivation, which ultimately leads to better learning. Furthermore, self-control conditions might also increase learners' perception of competence, another basic psychological need with a direct positive impact on intrinsic motivation. This is evidenced in past research showing that self-control learners typically ask for feedback after perceived good as opposed to bad trials (Chiviacowsky & Wulf, 2002), and experience higher levels of perceived competence and self-efficacy due to receiving feedback about these trials (Chiviacowsky et al., 2012b).

The information processing explanation, on the other hand, argues that the critical factor behind the self-controlled feedback benefit is the learner's provocation to estimate their error to decide the functional value of requesting feedback, for example to confirm they performed the trial well. So far, part of the evidence in support of this explanation comes from studies investigating the timing associated with the decision to receive feedback. Chiviacowsky & Wulf (2005), for instance, found that learners choosing after trial execution whether they wanted to receive feedback about that trial performed better in a delayed transfer test compared to learners who had to make the choice before the trial was initiated. Since both groups were allowed to choose their feedback schedule, the authors concluded that choice per se could not have driven the results. Instead, they proposed learners choosing whether to receive feedback after trial completion were presumably engaging in error estimation to determine the functional value of the feedback and decide whether to request it, which culminated in the observed learning advantages. This proposal was consistent with research beyond the self-controlled feedback domain showing that performance estimation during acquisition can benefit learning (Guadagnoli & Kohl, 2001; Liu & Wrisberg, 1997).

These findings were later replicated and expanded upon by Carter et al. (2014) who conducted an experiment that included groups allowed to choose whether to receive feedback before (self-before) or after (self-after) movement execution, in addition to a group that made the decision about the receipt of feedback before the trial was initiated but was allowed to change the original decision after movement execution (self-both). The rationale behind including the self-both group stems from the potential benefit its participants could receive from being motivated to perform well on trials for which they pre-selected feedback and from being encouraged to engage in error estimation to determine whether they should change their original decision about receiving feedback. Noteworthy, this study also included yoked groups to match each self-control condition to address a major limitation in Chiviacowsky and Wulf (2005)'s study, which did not include yoked conditions, thus limiting the conclusions about the self-controlled feedback benefit. Results revealed no performance differences between the self-after and self-both groups during post-tests (retention and transfer), ruling out the potential enhanced benefits in the latter group. However, both groups outperformed the self-before, yoked-after, and yoked-both groups in both post-tests. Moreover, no difference between self-before and yoked-before groups for either post-test was found, suggesting that deciding about feedback delivery before trial execution had no advantage over the absence of choice. Notably, participants were asked to estimate their error after each trial during both retention and transfer test, and the self-after and self-both groups showed more accurate estimation than the other groups, indicating they had developed superior error detection ability. The authors speculated this was due to these groups being stimulated to estimate errors while deciding whether to request feedback during practice, and this speculation was taken as evidence for the information processing explanation of the self-controlled feedback benefit.

Recently, after considering the evidence presented by Chiviacowsky and Wulf (2005), Carter et al. (2014), and other studies that followed (e.g., Carter & Ste-Marie, 2017a), Barros et al. (2019) reported two experiments that examined whether engagement in error estimation during practice could counteract the disadvantage associated with a yoked feedback schedule. The authors argued that if error estimation is driving the self-controlled feedback benefit, yoked learners encouraged to estimate their performance might show learning comparable to self-control learners. As such, in addition to the traditional self-control and yoked groups, the experiments included a second yoked group (yoked error estimation group) in which participants received feedback in a matched schedule to a self-control counterpart but were asked to estimate their performance, before receiving feedback, after every trial (first experiment) or only after trials wherein they received feedback (second experiment). In the first experiment, the authors found that the yoked error estimation group outperformed both the self-control traditional and yoked traditional groups during post-tests, with the latter groups not significantly differing. However, in the second experiment, there were no significant group differences during post-tests. Notably, in the second experiment, all participants were asked to estimate their errors during post-tests, and the yoked error estimation group exhibited more accurate error estimation. Further, regardless of training condition, participants who more accurately estimated their errors during post-test exhibited more accurate performance during post-test. Conversely, neither experiment revealed group differences in self-reported intrinsic motivation, autonomy, or perceived competence, and no relationships between these variables and post-test performance were observed when they were examined in Experiment 2. Considering results from both experiments, Barros and colleagues (2019) concluded that information processing (error estimation) likely contributes to the self-controlled feedback benefit more so than motivational factors.

Although Barros et al. (2019) sheds light on the mechanisms that may explain the self-controlled feedback benefit, the study's ability to illuminate the processes is limited by not observing the effect and the lack of a fully crossed experimental design. Regarding the first limitation, it is possible that the study was underpowered to detect the self-controlled feedback benefit, since the sample size was n = 20/group, but a sample size of n ~ 50/group is likely required for 80% power to detect a self-controlled practice effect based on a recent meta-analysis estimating the effect of self-controlled practice on motor learning to be Hedges' g = 0.44 (Faul et al., 2007; McKay et al., in press).[15] To address this shortcoming, we conducted the largest self-controlled feedback study (N = 200 [4 groups x 50 participants]) to date. Regarding the second limitation, the absence of a fully crossed design prevents the dissociation between the contributions of motivational and information processing factors to the self-controlled feedback benefit. To address this shortcoming, we crossed self-controlled feedback and error estimation in the same experimental design by creating four training conditions in which feedback schedule was either controlled by the participant (self-control) or matched to a counterpart (yoked) and error estimation was either mandatory (error estimation) or not enforced (traditional). We then assessed the effect of these manipulations on learning by carrying out a retention and a transfer test approximately 24-hr after the acquisition phase. This fully crossed design is key to address the motivational and informational accounts of the self-controlled feedback benefit. For instance, finding a main effect of self-control on learning (i.e., self-control traditional and self-control

[15] The sample size calculation was conducted with G*Power 3.1.9.4's ANOVA: Repeated measures, between factors test (Faul et al., 2007). Effect size was set to f = 0.22, corresponding to g = 0.44, which may be an overestimate due to selection bias (McKay et al., in press). Alpha was set to .05, power to .8, number of groups = 3, number of measures = 2, and correlation among measures to .5.

error estimation groups outperforming the other two groups during post-test) would favor the motivational account. This stems from the assumption that, if error estimation is a determining factor, the yoked error estimation group would perform similar to the self-control traditional and self-control error estimation groups, resulting in an interaction between self-control and error estimation, thus offering support to the informational explanation. Alternatively, a scenario where the self-control error estimation group exhibits better learning than both self-control traditional and yoked error estimation groups, which are similar and show better learning relative to the yoked traditional group, would suggest motivational and informational factors have an additive effect on motor learning. In addition to the crossed design, the present study included a self-reported measure of engagement in spontaneous error estimation given to participants in the traditional conditions, which complemented the self-reported measures of motivation given to all participants.

Methods

Prior to data collection, data processing and main analyses were pre-registered and made available at the Open Science Framework repository (Link).

Sample Size Calculation

Sample size was determined with a priori power calculation using G*Power 3.1.9.2 (Faul et al., 2007). For the statistical test, we used ANOVA: Fixed effects, special, main effects and interactions, and the following input parameters: Number of groups = 4 (self-control error estimation, self-control traditional, yoked error estimation, yoked traditional), Numerator df = 1 (based on a 2 (Self-Control/Yoked) x 2 (Error Estimation/Traditional) ANOVA), Power = .8, α = .05, and effect size f = .20. The effect size estimate was based on personal communication with Brad McKay who was conducting a meta-analysis on the effect of self-control on motor skill learning with Zachary D. Yantha, Julia Hussien, Michael J. Carter, and Diane M. Ste-Marie (B. McKay, personal communication, August 16, 2019). Subsequently, a preprint describing the meta-analysis was uploaded on PsyArXiv (https://psyarxiv.com/8d3nb). In personal communication, McKay estimated an effect of Hedges' $g = 0.1 – 0.4$, and we used the upper limit ($g = 0.4 \cong f = .2$) because we planned to control for pretest, therefore increasing our power by accounting for variance not explained by self-controlled practice, which was not always the case in studies identified by McKay et al.'s meta-analysis (in press). The power calculation yielded a total sample size of 199, which was rounded up to 200. To improve data collection efficiency, we decided to conduct a sequential analysis, a method commonly used in large medical trials that allows the use of interim analyses while also controlling for false positive (Type 1) error rate (Armitage et al., 1969; Dodge & Romig, 1929; Lakens, 2014). We established one interim analysis at 100 participants, when the main analyses would be carried out using the Pocock boundary (interim and final $\alpha = .0294$) and contrasted against our criteria to terminate data collection. The interim analysis at 100 participants revealed no statistically significant main effects of self-control feedback ($p = .228$, $\eta^2_p = .015$, 95% CI[0, 0.092]), or error estimation ($p = .252$, $\eta^2_p = .014$, 95% CI [0, 0.088]) and no statistically significant interaction ($p = .345$, $\eta^2_p = .009$, 95% CI [0, 0.078]). Since these results did not meet our pre-established criteria to stop collecting data (i.e., the upper bounds of the 95% CI(s) of the effects were not less than $\eta^2_p = .038$), we decided to proceed with data collection up to 200 participants. Details regarding stopping rules can be found in the pre-registration form.

Participants

The final sample was comprised of 200 participants (females = 148, $M_{age} = 20.64$, $SD = 1.60$ years). Recruitment was done through Auburn University's research credit system (College of Education Research Participation System – SONA) and by word-of-mouth. Four course credits were offered in exchange for participation when applicable. Participants did not have any previous experience with the experimental task nor did they report having any neuromuscular impairment that would affect performance of a nondominant arm bean bag tossing task. All participants reported using their right hand to throw. The present study was approved by Auburn University Institutional Review Board under the research protocol # 19-046 EP 1902 in agreement with the 1964 Declaration of Helsinki. All participants provided written consent prior to Day 1 of data collection and verbal consent prior to Day 2 of data collection.

Task

Participants practiced a nondominant arm bean bag tossing task similar to the one used by Grand et al. (2017). The goal of the task was to make the bean bag land as close to the center of the target as possible. Participants sat in a chair positioned in front of a table located 3 m away from the center of the target. Chair position was adjusted based on participant's arm length. Specifically, participants were asked to sit back in the chair and stick their arm out in a way that their fingers would touch the edge of the table. Next, the chair was moved back 25 cm to give participants enough room to perform the movement without hitting the table. The table also accommodated a computer monitor (38.5 cm screen size) positioned at eye level along with 10 bean bags distributed across the table in pairs and served as a support for a pasteboard used to occlude participants' vision of the target. The computer monitor was used to deliver feedback. To make the task more challenging and thus avoid a ceiling effect (R. A. Schmidt & Lee, 2020), we instructed participants to grasp each bean bag with their left hand pronated and toss it over the occlusion board by elevating their arm and flicking their wrist. The movement was first demonstrated by the experimenter, who then asked the participant to repeat the motion to ensure proper understanding of the movement. The target, which was taped to the floor, consisted of a grid (140cm x 140cm) comprising 49 evenly distributed squares (side length = 20 cm). Each square was assigned a letter and a number indicating the square position (e.g., A1 represented the first square on the top left of the target and D4 represented the center of the target). Finally, a standard computer keyboard was placed on a small table next to the participant's right arm. This keyboard was used for performance estimation and to initiate feedback delivery, as explained below.

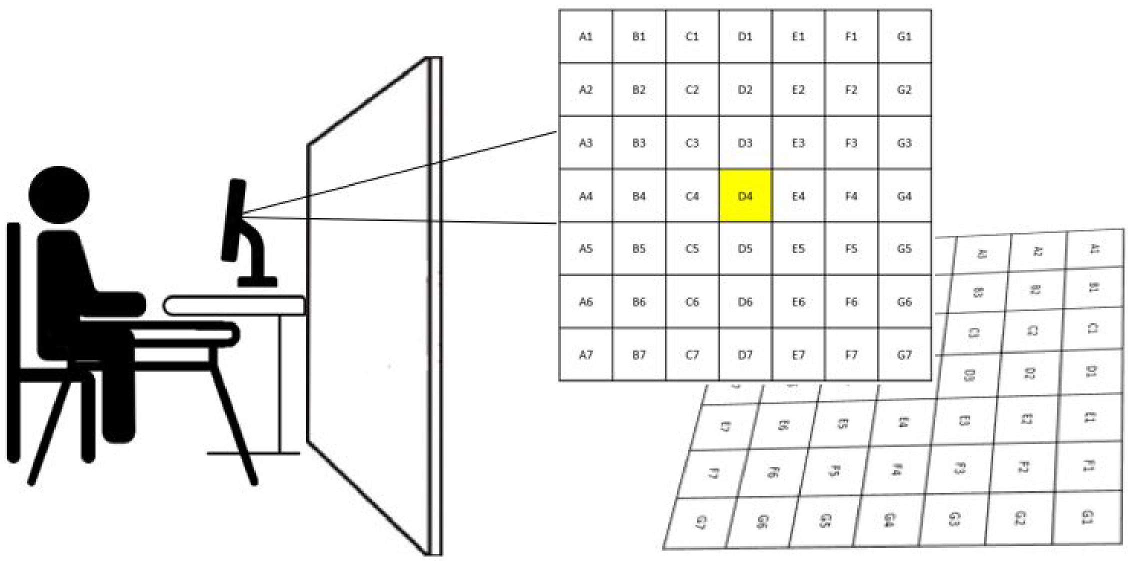
Procedures

Day 1 of data collection

Experimental set-up is shown in Figure 1. Prior to the beginning of the experiment, participants provided written consent and filled out a demographics questionnaire, which asked questions about age, sex, and previous experience playing cornhole (i.e., a bean bag game). Next, they were directed to the task area, where they had their chair position adjusted and were given instructions about the goal of the task and how to execute the movement. To determine baseline skill level, participants performed a pretest consisting of one block of ten trials without feedback. (Participants were allowed to see the target for 10s before initiating pretest.) After pretest, participants were quasi-randomly assigned (based on sex) to one of four groups: self-control traditional, yoked traditional, self-control error estimation or yoked error estimation. Participants in the self-control condition were allowed to choose whether they wanted to receive feedback after a trial, whereas participants in the yoked condition received feedback in a matched schedule to a self-control counterpart (e.g., participants in the yoked traditional condition received feedback corresponding to the same trials as requested by their self-control traditional counterpart). Participants in the error estimation condition were asked to estimate where the bean bag landed after each trial regardless of feedback delivery, whereas participants in the traditional condition were not. Detailed instructions given to each group can be found in the OSF repository.

Figure 1

Representation of Task and Experimental Set-up



Note. The left side of the figure shows the participant in a sitting position and a pasteboard blocking their vision of the target. The upper-right side of the figure illustrates how feedback was delivered throughout the experiment.

After being assigned to a training condition, participants performed the acquisition phase, which consisted of 10 blocks of 10 trials with a 1-min break between blocks. (Participants were allowed to see the target for 10s before initiating the acquisition phase.) Feedback was presented 50% of the time (i.e., participants received feedback in 5 of 10 trials per block) according to training condition. Specifically, after each trial, participants in the self-control condition were informed of the number of feedback requests remaining in the block and asked whether they wanted to receive feedback about that trial. If yes, participants were instructed to press "enter" on the keyboard when the word *"ready"* appeared on the computer screen in front of them. Next, participants saw an image of the target on the screen for 2000 ms, then the square where the bean bag landed was highlighted in yellow for 1000 ms, as shown on the upper-right side of Figure 1. After feedback delivery, participants moved on to the next trial. Important to mention, participants were informed that they would have to request five feedbacks per block. In situations wherein the number of trials remaining in the block matched the number of feedback requests available, the remaining trials defaulted to feedback trials (e.g., if there were three feedback requests available and three trials remaining in the block, participants were prompted to ask feedback after each of the three remaining trials). For participants in the yoked condition, after each trial they were informed of the number of feedbacks remaining in the block and whether it had been determined they would receive feedback about that trial. As mentioned previously, participants in the error estimation condition estimated their performance after each trial. Specifically, after each throw, participants were instructed to estimate where they thought the bean bag landed. To initiate performance estimation, participants pressed "enter" on the keyboard when the word "ready" appeared on the computer screen in front of them. Next, participants saw an image of the target on the screen and were asked to press on the keyboard the letter and the number of the square that matched their prediction. For example, if they thought the bean bag landed on D4, they would press the letter "D" followed by the number "4". For trials wherein they predicted the bean bag landed outside the target, participants were instructed to enter "XX". Feedback was presented right after performance estimation according to training condition. During the resting period between acquisition blocks, regardless of condition, participants filled out a single-item engagement and motivation questionnaire, which asked participants to rate on a scale of 0 to 10 how engaged and motivated they were towards the bean bag task (Leiker et al., 2018; Pathania et al., 2019). In addition, participants in the self-control error estimation and yoked error estimation groups were asked to report on a scale of 0 to 10 how confident they were in their ability to predict the bean bag position.

After completing the last block of acquisition, all participants were asked to fill out the Intrinsic Motivation Inventory (IMI; McAuley, Duncan, and Tammen, 1989) consisting of 35 questions answered on a 7-point Likert scale ranging from "not true at all" to "very true". This questionnaire is divided into six subscales (i.e., interest/enjoyment; effort/importance; value/usefulness; pressure/tension; perceived choice; and perceived competence) intended to measure participants' experience with the experimental task (i.e., bean bag task). For the present study, questions corresponding to the perceived choice subscale were replaced with questions designed to measure perception of autonomy. This modified version was similar to the one used by used by Carter & Ste-Marie (2017b). In addition, participants in the self-control and yoked traditional conditions were asked to report on a scale of 0 to 100% what percentage of the time they were estimating their performance (e.g., 75% of the time).

Day 2 of data collection

Participants returned approximately 24-hr after Day 1 to complete a retention and a transfer test. The retention test consisted of the same bean bag tossing task practiced the day before, whereas the transfer test consisted of a variation of the original task in which participants were moved back one meter from their original position. Therefore, participants had to adjust the force parameter to meet the task goal. We decided to include a transfer test since previous studies on the same topic have shown the benefits of self-controlled feedback during transfer but not retention tests (Fairbrother et al., 2012). Moreover, the ability to generalize a skill can be assessed when a variation of the skill is performed (R. A. Schmidt & Lee, 2011). Both post-tests consisted of 1 block of 10 trials and were carried out in a counterbalanced order. Participants were allowed to look at the target for 10s before each post-test, but no feedback was provided throughout the post-tests.

Dependent Variables and Data Processing

The main dependent variable of interest was radial error (RE), which is a measure of accuracy (Hancock et al., 1995), but we also computed bivariate variable error (BVE) as a measure of precision. For the first 100 participants, the software Dartfish® was used to record the magnitude of the error along the x- and y-axis. More specifically, an iPad mounted to the ceiling right above the center of the target recorded where each bean bag landed during the entire data collection session (Days 1 and 2). Next, recorded videos were imported into Dartfish where x and y measures were obtained. For the remaining participants, the program LabView® equipped with the virtual instrument *ScorePutting* (Neumann & Thomas, 2008) was used to compute the magnitude of the error along both x and y dimensions, after confirming the correlation between Dartfish® and LabView® in obtaining these measures was high ($r \geq .995$) among four participants. RE and BVE were calculated for the pretest, all blocks of acquisition phase (10 blocks) and post-tests (retention and transfer tests). For the secondary exploratory analyses, we computed single scores for the IMI subscales Interest/Enjoyment (Cronbach's α = .93), Perceived Choice (Cronbach's α = .62), and Perceived Competence (Cronbach's α = .91) by averaging across all seven, five, and six items within the scales, respectively. The Interest/Enjoyment subscale was included in the exploratory analyses as it is considered the subscale most directly associated with intrinsic motivation (Deci et al., 1994). Perceived choice and perceived competence scores were also analyzed as levels of perception of autonomy and competence have been shown to be increased in self-control protocols (e.g., Chiviacowsky et al., 2012; McKay & Ste-Marie, 2020b). Finally, we computed the measure error estimation percentage by extracting the scores from the questionnaire that asked participants in the self-control traditional and yoked traditional groups to report the percentage of the time they were engaging in performance estimation.

Statistical Analysis

For the primary confirmatory analysis of interest, we assessed the effect of training condition on learning as indexed by post-test accuracy by conducting a 2 (Self-control) x 2 (Error Estimation) x 2 (Post-test: retention test/transfer test) mixed-factor ANCOVA with repeated-measures on the last factor, post-test RE serving as the dependent variable, and pretest RE serving as the covariate. We also conducted non-preregistered analyses on acquisition and post-test data. Specifically, to investigate the effect of training condition on tossing accuracy and precision during the acquisition phase, RE and BVE served as the dependent variable in two separate 2 (Self-control: self-control/yoked) x 2 (Error Estimation: error estimation/traditional) x 10 (Block: 1:10) mixed-factor ANCOVAs with repeated-measures on the last factor and pretest (pretest RE and pretest BVE, respectively) serving as the covariate. To assess the effect of training condition on post-test precision, the same ANCOVA was conducted with post-test BVE serving as the dependent variable and pretest BVE serving as the covariate. Alpha was set to .05 and the Greenhouse-Geisser correction was applied when sphericity was violated. Tukey HSD was used for post-hoc tests when applicable.

Results

Statistical assumptions were met for all analyses and no influential data points (Cook's distances greater than or equal to 1.00) were observed for any analysis.

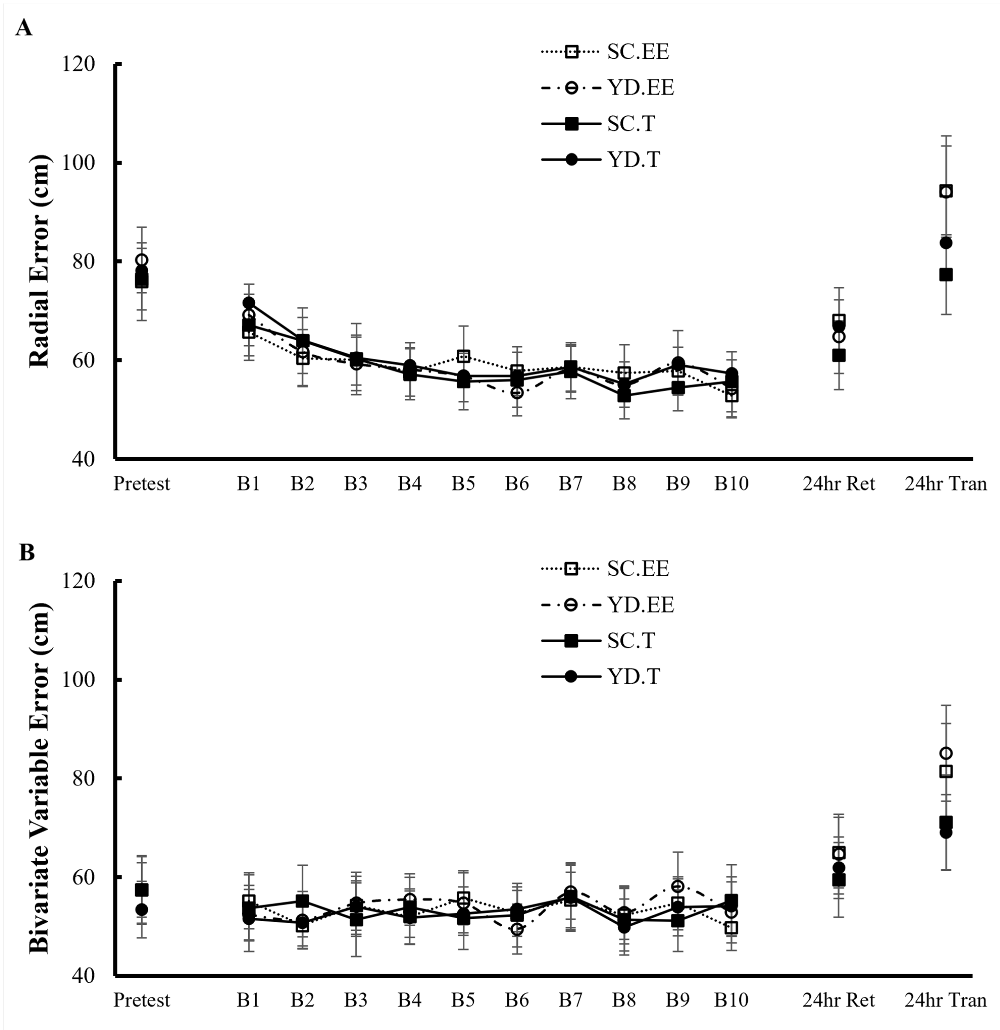
Preregistered Analyses

Retention and Transfer

Figure 2 shows tossing accuracy (RE) and precision (BVE) as a function of study phase (pretest, all 10 blocks of the acquisition phase and post-tests). Before running the mixed-factor ANCOVA on the variable post-test RE, we ran a 2-tailed paired samples *t*-test to assess whether there was a difference in performance between pretest RE and retention RE regardless of training condition. Results showed that participants performed significantly better during the retention test (M = 65.18 cm, SD = 24.39) compared to the pretest (M = 77.71 cm, SD = 25.35) ($t(199) = 6.05$, $p < .001$, Cohen's $d = 0.50$, 95% *CI* [0.36, 0.65]) indicating learning. For the primary confirmatory analysis of interest, the mixed-factor ANCOVA assessing the effect of training condition on post-test RE revealed a main effect of pretest ($F(1, 195) = 17.30$, $p < .001$, $\eta^2_p = .08$), a main effect of post-test ($F(1, 195) = 12.25$, $p = .001$, $\eta^2_p = .06$) indicating participants performed worse on the transfer test compared to the retention test, and a main effect of error estimation ($F(1, 195) = 4.56$, $p = .034$, $\eta^2_p = .02$), which was superseded by a Post-test x Error Estimation interaction ($F(1,195) = 6.66$, $p = .011$, $\eta^2_p = .03$). To follow-up this interaction, separate one-way (error estimation) ANCOVAs were conducted for the retention and transfer tests, with pretest RE serving as the covariate. Results showed no significant effect for retention ($p = .500$), but a significant effect for transfer ($F(1, 197) = 7.38$, $p = .007$, $\eta^2_p = .04$), such that participants in the error estimation condition (M = 94.21 cm, SD = 36.63) performed worse than participants in the traditional condition (M = 81.15 cm, SD = 31.33) on this post-test. There were nonsignificant effects for self-control ($p = .769$), Post-test x Pretest ($p = .901$), Self-control x Error Estimation ($p = .255$), Self-control x Post-test ($p = .750$) as well as Self-Control x Error Estimation x Post-test ($p = .661$) interactions.

Figure 2

Tossing Accuracy and Precision



Note. A: Tossing accuracy (lower numbers indicate greater accuracy) as a function of study phase (pretest, acquisition, 24-hr retention test, and 24-hr transfer test) and group (SC.EE: self-control error estimation; YD.EE: yoked error estimation; SC.T: self-control traditional; YD.T: yoked traditional). Error bars represent 95% CIs. B: Tossing precision (lower numbers indicate greater precision) as a function of study phase (pretest, acquisition, 24-hr retention test, and 24-hr transfer test) and group (SC.EE: self-control error estimation; YD.EE: yoked error estimation; SC.T: self-control traditional; YD.T: yoked traditional). Error bars represent 95% CIs.

Preregistered Exploratory Analyses

Acquisition Phase

Results of the mixed-factor ANCOVA for the variable RE revealed a main effect of pretest ($F(1, 194) = 53.92$, $p < .001$, $\eta^2_p = .22$), which was superseded by a Block x Pretest interaction ($F(6.71, 1300.90) = 6.58$, $p < .001$, $\eta^2_p = .03$). Specifically, the relationship between pretest and blocks of acquisition became weaker over time (Table 1). There was no main effect of block ($p = .062$), self-control ($p = .935$), error estimation ($p = .805$), and no Block x Self-control ($p = .701$), Block x Error Estimation ($p = .373$), Self-control x Error Estimation ($p = .454$), or Block x Self-control x Error Estimation interaction ($p = .954$). Similar results were found for the variable BVE. There was a main effect of pretest ($F(1, 194) = 78.38$, $p < .001$, $\eta^2_p = .29$), but no main effect of block ($p = .554$), self-control ($p = .706$), error estimation ($p = .870$), and no Block x Pretest ($p = .406$), Block x Self-control ($p = .789$), Block x Error Estimation ($p = .344$), Self-control x Error Estimation ($p = .443$), or Block x Self-control x Error Estimation interaction ($p = .789$).

Retention and Transfer

For the variable post-test BVE, results of the mixed-factor ANCOVA showed a main effect of pretest ($F(1, 195) = 55.30$, $p < .001$, $\eta^2_p = .22$), a main effect of post-test ($F(1,195) = 15.64$, $p < .001$, $\eta^2_p = .07$) indicating participants were less precise during the transfer test, and a main effect of error estimation ($F(1,195) = 5.18$, $p = .024$, $\eta^2_p = .03$), which was superseded by a Post-test x Error Estimation interaction ($F(1,195) = 4.72$, $p = .031$, $\eta^2_p = .02$). To follow-up this interaction, separate one-way (error estimation) ANCOVAs were conducted for the retention and transfer tests, with pretest BVE serving as the covariate. Results showed no significant effect for retention ($p = .399$), but a significant effect for transfer ($F(1, 197) = 7.25$, $p = .008$, $\eta^2_p = .04$), such that participants in the error estimation condition (*M* = 83.24 cm, *SD* = 35.00) performed worse than participants in the traditional condition (*M* = 70.09 cm, *SD* = 31.20) on this post-test. There were nonsignificant effects for self-control (p = .578) and Self-control x Error Estimation (p = .532), Post-test x Pretest (p = .107), Self-control x Post-test (p = .913), as well as Self-control x Error Estimation x Post-test (p = .366) interactions.

Table 1.

Correlation Coefficients Between Covariates Used in Models to Test Performance (Acquisition Blocks 1 – 10) and Learning (Retention and Transfer Tests) For Radial Error and Bivariate Variable Error

Covariate	B1	B2	B3	B4	B5	B6	B7	B8	B9	B10	Ret	Tran
Pretest RE	.57	.39	.36	.30	.31	.41	.25	.28	.28	.28	.31	.21
Pretest BVE	.49	.36	.41	.33	.39	.43	.38	.34	.37	.35	.53	.31

Non-preregistered Exploratory Analyses

Self-controlled feedback is hypothesized to increase perceptions of autonomy by giving learners choice about when to receive feedback and perceived competence by giving learners the opportunity to request feedback after their better trials (Chiviacowsky et al., 2012; Sanli et al., 2013). Increased perceptions of autonomy and competence are associated with intrinsic motivation which is predicted to improve motor learning (Wulf & Lewthwaite, 2016). To examine these motivational effects of self-controlled feedback, we conducted 2 (Self-control) x 2 (Error Estimation) ANOVAs for the Perceived Choice (autonomy), Perceived Competence, and Interest/Enjoyment (intrinsic motivation) subscales of the IMI. To determine whether self-control participants requested feedback after their better trials, we conducted a 2 (Self-control) x 2 (Error Estimation) x 2 (Trial Type: feedback/no-feedback trial) mixed-factor ANOVA with repeated measures on the last factor. Finally, to assess whether intrinsic motivation explained learning irrespective of experimental group, we conducted a mixed-effects regression with post-test RE as the dependent variable and fixed-effects of pretest RE, condition (self-control, error estimation and their interaction), post-test type (retention/transfer), the interaction between condition and post-test type, intrinsic motivation (IMI Interest/Enjoyment subscale score), and the interaction between intrinsic motivation and post-test type as well as a random-effect of participant. For the mixed-effects analysis, all continuous variables were mean-centered, and all categorical variables were contrast-coded.

For perceived autonomy, results showed a main effect of self-control ($F(1,196) = 4.27$, $p = .04$, $\eta^2_p = .02$) such that participants in the self-control condition had higher levels of perceived choice ($M = 5.31$, $SD = 0.88$) compared to participants in the yoked condition ($M = 5.01$, $SD = 1.12$), and no main effect of error estimation ($p = .933$) or Self-control x Error Estimation interaction ($p = .780$). For perceived competence, results did not demonstrate significant effects for self-control ($p = .141$), error estimation ($p = .527$), or a Self-control x Error Estimation interaction ($p = .063$). However, RE on feedback vs. no-feedback trials did significantly differ as a function of group. Specifically, a main effect of trial type ($F(1, 196) = 171. 59$, $p < .001$, $\eta^2_p = .47$) indicated that trials for which participants received feedback had lower RE ($M = 54.13$ cm, $SD = 15.18$) than trials for which they did not ($M = 63.86$ cm, $SD = 18.27$), and this effect was superseded by a Self-control x Trial Type interaction ($F(1, 196) = 207.47$, $p < .001$, $\eta^2_p = .51$). To follow-up this result, we conducted separate 2-tailed paired-samples *t*-tests (trial) for self-control and yoked participants. Results showed a significant effect for self-control participants ($t(99) = 15.74$, $p < .001$, $d = 1.57$, 95% CI [1.28, 1.87]) such that their feedback trials had lower RE ($M = 48.31$ cm, $SD = 11.73$) than their no-feedback trials ($M = 68.73$ cm, $SD = 19.89$), whereas no such effect was observed for yoked participants (p = .195). The initial ANOVA also revealed an Error Estimation x Trial Type interaction ($F(1, 196) = 3.96$, $p = .048$, $\eta^2_p = .02$), and follow-up 2-tailed paired-samples *t*-tests showed that both error estimation and traditional participants had lower RE on feedback trials than no feedback trials (error estimation: ($t(99)$ = 5.65, $p < .001$, d = 0.57, 95% *CI* [0.35, 0.78], feedback trial RE: M = 54.63 cm, SD = 14.61 vs. no-feedback trial RE: M = 62.87 cm, SD = 17.39; traditional: ($t(99)$ = 7.28, $p < .001$, d = 0.73, 95% *CI* [0.51, 0.95], feedback trial RE: M = 53.64 cm, SD = 15.78 vs. no-feedback trial RE: M = 64.84 cm, SD = 19.14). The initial ANOVA did not reveal a significant Trial Type x Self-control x Error Estimation interaction (p = .416).

For intrinsic motivation, results demonstrated no effect for self-control (p = .832), error estimation (p = .817), or Self-control x Error Estimation (p = .214). However, the mixed-effects model (Table 2) revealed that intrinsic motivation predicted post-test performance controlling for group such that participants with higher Interest/Enjoyment scores had lower RE during post-tests (β = -5.50, SE = 1.46, $p < .001$).

The information processing explanation for the self-controlled feedback learning effect hypothesizes that learners in self-controlled feedback conditions engage in spontaneous error estimation more often than participants in yoked conditions. Thus, to assess whether this was the case in the present experiment, we carried out a Mann-Whitney U test on the variable Error Estimation Percentage. (The nonparametric test choice was due to the non-normal distribution of these scores; skewness = -1.30 (SE = 0.24), kurtosis = 1.88 (SE = 0.48)). Results showed that participants in the self-control traditional group (*Mdn* = 90.00% of the time) estimated their performance significantly more often than participants in the yoked traditional group (*Mdn* =

80.00% of the time; $U(N_{self\text{-}control\ traditional} = 50, N_{yoked\ traditional} = 50) = 926.00$, z = -2.26, p = .024), as predicted by the information processing explanation.

Some of the exploratory analyses included in the pre-registration form (e.g., electroencephalography, error estimation accuracy, and implicit achievement motivation analyses) will be reported in separate publications.

Table 2

Fixed and Random Effects for the Analysis of the Effect of Intrinsic Motivation on Post-Test Performance

Random Effects					
Group	*Effect*		*Variance*		*SD*
Participants	Intercept		372.8		19.31
Residual			372.8		20.45
Fixed Effects					
Effects	*β*	*SE*	*df*	*t-value*	*p-value*
Intercept	<0.001	1.71	194	<0.001	>0.999
Pretest RE	0.29	0.07	194	4.327	< 0.001***
Self-control	-0.84	3.42	194	-0.245	0.807
Error Estimation	7.74	3.41	194	2.268	0.024*
Post-test Type	<0.001	2.04	195	0.00	>0.999
Intrinsic Motivation	-5.50	1.46	194	-3.78	< 0.001***
Self-control x Error Estimation	5.76	6.85	194	0.841	0.401
Self-control x Post-test Type	-1.20	4.09	195	-0.293	0.770
Error Estimation x Post-test Type	10.69	4.09	195	2.613	0.010*
Post-test Type x Intrinsic Motivation	-2.37	1.74	195	-1.356	0.177
Self-control x Error Estimation x Post-test Type	-4.58	8.21	195	-0.557	0.578

Note. Self-control was coded as self-control = 0.5; yoked = -0.5. Error estimation was coded as error estimation = 0.5; traditional = -0.5. Post-test Type was coded as retention = -0.5; transfer = 0.5. Random effects: Number of observations: 400; Groups: Participants.

Discussion

Motivational and information processing explanations have been proposed as possible mechanisms underlying the self-controlled feedback learning benefit. Evidence in support of one explanation over the other is still mixed (M. J. Carter et al., 2014; M. J. Carter & Ste-Marie, 2017a; Chiviacowsky & Wulf, 2005; Wulf & Lewthwaite, 2016) and might be influenced by important methodological limitations such as the lack of a fully crossed experimental design (Barros et al., 2019), measures of engagement in spontaneous error estimation, and adequate statistical power to detect the effect of interest (Lohse et al., 2016; McKay et al., in press). To dissociate the contributions of motivation and information processing to the self-control feedback learning benefit while addressing the previously identified methodological concerns, we sampled 200 participants in a study that crossed self-controlled feedback and error estimation in the same experimental design and included self-reported measures of engagement in performance estimation, intrinsic motivation, perceived choice, and perceived competence. Learning was assessed in a retention and transfer test carried out approximately 24-hr after the acquisition phase.

Overall, participants showed a significant improvement in throwing accuracy from pretest to retention test, indicating learning. However, we failed to replicate the self-controlled feedback learning benefit as no differences between self-control and yoked conditions were found in the post-tests. This is consistent with recent studies investigating the role played by autonomy support in motor skill learning (Grand et al., 2017b; McKay & Ste-Marie, 2020a, 2020b; St. Germain et al., 2021). For instance, Grand et al. (2017) and McKay and Ste-Marie (2020a, 2020b) found that control over the color of the object used during the acquisition phase (bean bag, golf ball, and darts, respectively) did not result in enhanced learning as indexed by delayed post-test performance. St. Germain and colleagues (2021) also failed to show learning differences between self-control participants allowed to choose the frequency and speed of video demonstrations and yoked participants. Together, these and the present study's findings contradict the OPTIMAL theory claim that offering learners a chance to make decisions enhances learning and join a recent meta-analysis in questioning the effectiveness of self-controlled practices (McKay et al., in press). Specifically, McKay et al. (in press) estimated the effect of self-controlled practice on motor learning to be trivially small, ranging from Hedges' $g = 0.04 - 0.11$, depending on the selection bias-correction technique employed. Moreover, even among published studies showing statistically significant results in favor of the self-control learning benefit, no evidential value was found following the results of a *p*-curve analysis (Simonsohn et al., 2014). Assuming the selection-adjusted effect is accurate (Hedges' $g = 0.11$), extremely large sample sizes (~1300 participants/group[16]) would be needed to detect an effect of self-controlled feedback on learning. Considering the median sample size of the studies included in the meta-analysis was 36 participants/group, previous studies showing the self-controlled feedback learning benefit might have reported overestimated effects likely resulting from false positives (Button et al., 2013b) and prior studies failing to show the self-controlled feedback benefit may have gone unreported (i.e., the file drawer problem), which would explain the results of the *p*-curve analysis. Altogether, failure in replicating the self-control practice learning benefit by studies with pre-registered statistical plans, a priori power calculations, and large sample sizes (e.g., the present study; Grand et al., 2017; McKay & Ste-Marie, 2020a; St. Germain et al., 2021) corroborated by the results of the aforementioned meta-analysis challenge the claim that the self-control practice learning effect is "very robust and generalizable" (Wulf & Lewthwaite, 2016; p.

[16] The sample size calculation was conducted with G*Power 3.1.9.4's *t*-test – Means: Difference between two independent means (Faul et al., 2007). Effect size was set to $d = 0.11$ (McKay et al., in press). Alpha was set to .05, power to .8, and allocation rate N2/N1 to 1.

1393). It is worth mentioning that the number of test trials (pretest, retention, and transfer test) adopted may have been a limitation of the present study but likely was not. The decision to adopt only 10 trials per test was based on previous self-controlled feedback studies that have shown the learning advantages associated with this manipulation (e.g., Chiviacowsky et al., 2008). Moreover, our results revealed a general learning effect from pretest to retention, suggesting the number of test trials adopted was likely sufficient to show any moderation of the learning effect by self-control over feedback. Nonetheless, increasing the number of trials per test may increase statistical power (Maxwell et al., 1991) and should be considered in future research.

The motivational explanation of the self-controlled feedback benefit is based on the concept that giving learners control over practice fulfills their psychological need for autonomy (Ryan & Deci, 2000). Although we did not observe a self-controlled feedback benefit, we found that self-control control participants reported higher levels of perceived autonomy compared to yoked participants, although this effect was small ($\eta^2_p = .02$) (J. T. E. Richardson, 2011) and should be interpreted with caution given the poor reliability of the measure (Cronbach's $\alpha = .62$). The motivational explanation of the self-controlled feedback benefit has also been supported by research showing that self-control learners typically request feedback after their better trials, consequently boosting their perceived competence, which is another psychological need for intrinsic motivation (Chiviacowsky et al., 2012b). We observed self-control participants requested feedback on substantially more accurate trials than those for which they did not request feedback ($d = 1.25$), whereas the trials on which yoked participants received feedback were statistically like those on which they did not, a finding consistent with some past research (Chiviacowsky & Wulf, 2002). However, self-control and yoked participants did not significantly differ in their perceived competence, which is congruous with some studies (Barros et al., 2019; Leiker et al., 2016; St. Germain et al., 2021) and incongruous with others (Chiviacowsky, 2014; Chiviacowsky et al., 2012b; Ste-Marie et al., 2013). The fact that participants in the present and past studies (e.g., Chiviacowsky & Wulf, 2002) requested feedback mainly after good as opposed to bad trials deserves further attention. Specifically, the nature of the self-control paradigm may bring an inherent confounding variable into the experiment, namely feedback after good trials (Wulf & Lewthwaite, 2016). This is because yoked participants, being deprived of choice, might receive feedback after poor as opposed to good trials. Self-control participants, on the other hand, might experience the opposite considering they are allowed to choose their feedback schedule, which might lead to a significant confounding factor between self-control and yoked groups. Thus, future studies investigating the self-controlled learning benefit should consider how feedback after good trials might affect the research findings.

As self-controlled feedback had only a tenuous effect on perceived autonomy and no significant effect on perceived competence, it is unsurprising that it did not significantly affect intrinsic motivation. Similar results were observed in past self-control studies in which motor learning occurred but intrinsic motivation remained comparable at the group-level (Barros et al., 2019; Grand et al., 2017b; Post et al., 2016; St. Germain et al., 2021). One may argue that having participants fill out the IMI at the end of data collection reduced the sensitivity of the questionnaire in capturing participants' true level of motivation. However, recent studies investigating a similar motor learning paradigm and measuring intrinsic motivation using IMI subscales at multiple time points throughout the experiment also failed to show differences between experimental and control groups (e.g., Barros et al., 2019; St. Germain et al., 2021). Notably, in our study, intrinsic motivation predicted learning controlling for pretest accuracy and training condition such that participants exhibiting greater levels of intrinsic motivation also showed better post-test performance. Thus, the relationship between intrinsic motivation and learning at the individual level supports the perspective that motivation plays an important role in motor learning, but, crucially, giving learners control over feedback did not enhance intrinsic motivation nor motor learning, which is incompatible with OPTIMAL theory (Wulf & Lewthwaite, 2016). Together, these findings have important theoretical and practical implications. From a theoretical standpoint, results from the present and past studies (Ste-Marie et al., 2016) suggest that psychological variables such as perceived autonomy, perceived competence, and intrinsic motivation may not explain the self-controlled learning benefit and might need to have their role reconsidered by OPTIMAL theory. From a practical standpoint, given the lack of association between the opportunity to exert choice during practice and significant changes in psychological variables, practitioners might need to more carefully evaluate the implementation of self-control strategies when the goal is to influence levels of motivation to promote learning.

The information processing explanation for the self-controlled feedback benefit posits that self-controlled learners spontaneously estimate their errors to decide the functional value of requesting feedback. Consistent with this explanation, our results show that self-control traditional participants reported estimating their performance more frequently (*Mdn* = 90.00% of the time) than yoked traditional participants (*Mdn* = 80.00% of the time), but with no benefit to learning. Moreover, participants who were asked to explicitly estimate their performance during acquisition did not incur a learning benefit, but rather exhibited a small learning disadvantage (Richardson, 2011), as revealed by worse accuracy (and precision) in the transfer test (η^2_ps = .04). This result is inconsistent with past studies demonstrating a learning advantage for participants who explicitly estimated their performance after each trial (Guadagnoli & Kohl, 2001; Liu & Wrisberg, 1997). There are differences between these past studies and the present one, such as the comparison groups. In the past studies, the comparison groups included participants who were encouraged to estimate their performance by limiting the frequency or delaying the delivery of augmented feedback and participants who were discouraged from estimating their performance by giving feedback immediately after each trial. In the present study, the comparison groups included participants who were encouraged to estimate their performance (self-control) and neither encouraged nor discouraged from estimating their performance (yoked), and both groups reported estimating their performance frequently (~85% of the time). Future research considering the effect of explicit performance estimation should consider and measure the degree to which comparison groups are spontaneously estimating their errors. It is also worth considering how participants are estimating their performance. For example, the present study's traditional participants may have been roughly estimating their errors ("I missed far and left), thereby encouraging general performance adjustments ("Throw shorter and to the right") that may have promoted the generalizable knowledge assessed by the transfer test. Conversely, the error estimation participants were asked to make relatively precise estimations ("[The beanbag landed in] B2"), thereby encouraging more specific performance adjustments that may have hindered the accrual of the generalizable knowledge assessed by the transfer test.

Conclusion

The present experiment failed to replicate the self-controlled feedback learning benefit and showed little evidence that self-controlled feedback is associated with increases in motivational factors. These results challenge OPTIMAL theory's claim that allowing learners to exercise control results in superior motor learning and meaningful psychological benefits. Although the present study suggests intrinsic motivation is linked to motor learning, it joins recent research in questioning the robustness and generalizability of the self-control learning effect and whether self-control protocols influence motivation.

Chapter 4: Reinforcement learning in motor skill acquisition: Using the Reward Positivity to understand the mechanisms underlying short- and long-term behavior adaptation

Introduction

Reinforcement learning is one of the most dominant modes of learning (Rescorla & Wagner, 1972; Sutton & Barto, 1998), and an important model to understand skill acquisition (Lohse et al., 2019). According to this theory, humans make behavior adaptations based on *reward-prediction errors*, the difference between actual and anticipated rewards (Holroyd & Coles, 2002; Schultz, 2017). Behaviors that lead to better- or worse-than-expected outcomes result in positive and negative reward-prediction errors, respectively. At the neural level, reward-prediction errors convey information that is used to guide future adaptations (Seidler et al., 2013). More specifically, within the brain, positive reward-prediction errors increase the value of behaviors that resulted in better-than-expected outcomes, making the re-occurrence of these behaviors more likely in the future. Conversely, negative reward-prediction errors decrease the value of behaviors that resulted in worse-than-expected outcomes, making the re-occurrence of these behaviors less likely in the future.

In theory, reinforcement learning principles can explain the changes in performance observed during the *motor* skill acquisition process (i.e., power law of practice, Lohse et al., 2019; Newell & Rosenbloom, 1981). Specifically, the rapid improvements seen early in learning can be explained by the large reward-prediction errors that are also common at this stage whereas the smaller adjustments typically observed later in learning can be attributed to the smaller reward-prediction errors that arise once actual and expected performance are more closely aligned. Consider a novice trying to learn how to putt. Early on, her lack of familiarity with the task and ability to detect and correct errors may lead to frequent, large negative reward-prediction errors due to her badly missed putts. Thus, to find the movement pattern that will get her closer to sinking a putt, she needs to explore different movement strategies (i.e., implement large performance adjustments). Her lack of practice and experience also make successful performance (i.e., sinking the putt) less likely to occur, so her expectations for future rewards are low. Thus, when she unexpectedly sinks her first putt, this leads to an outcome that is way better than anticipated or a large positive reward-prediction error. As previously mentioned, positive reward-prediction errors facilitate movement repetition and, as a consequence, the behavior that precipitated success is likely to be repeated, leading to rapid improvements. Toward the later stages of learning, she may have already found the movement strategy that more closely aligns with the optimal movement pattern. At this point, she begins to exploit that movement strategy to find *her* optimal movement pattern by implementing smaller adjustments. Also, as she becomes more skillful and knowledgeable about the task, her actual performance starts to match her expected performance, leading to smaller reward-prediction errors, which in turn would explain the smaller performance adjustments seen at that stage.

The consistency between reinforcement learning predictions and motor learning phenomena (i.e., power law of practice) serves as a strong theoretical argument in support of the use of this theory to explain motor behavior adaptation. However, stronger empirical evidence for the role of reinforcement learning in motor skill acquisition would be provided by investigating the mechanisms underlying reinforcement learning and its primary driver, reward-prediction errors. In human research, reward-prediction errors have been studied through the measure of the reward positivity (RewP), an event-related potential (ERP) component derived from the electroencephalogram (EEG). Methodologically, the RewP is characterized as a positive deflection in the ERP waveform that peaks between 230 ms and 350 ms after feedback onset and exhibits a frontal-central scalp topography, typically maximal at electrode FCz (Krigolson, 2018). Compelling evidence suggests that the RewP is sensitive to reward magnitude (i.e., larger vs smaller rewards) and likelihood (i.e., unexpected vs expected rewards)(Sambrook & Goslin, 2015).

Evidence from past research suggests that RewP amplitude changes as a function of practice (Williams et al., 2018) and is correlated with subsequent behavior (Holroyd & Krigolson, 2007), although there is also evidence that behavioral changes are not always accompanied by changes in RewP amplitude, and that, sometimes, they can occur independently (Cockburn & Holroyd, 2018; Walsh & Anderson, 2012). Notably, studies investigating the underlying mechanisms of reinforcement learning using the RewP often do not use learnable tasks, instead relying on those where performance and feedback are based on chance (e.g., reward gambling tasks) to make inferences about reward-prediction errors and behavior adaption. In these paradigms, feedback is usually binary (i.e., correct versus incorrect response; Meadows et al., 2016) and the frequency and/or probability of receiving correct/incorrect feedback is controlled by the experimenter (e.g., probability of making a correct response and receiving positive feedback is set at 50%). However, real-world skill acquisition involves learnable tasks, and feedback probability varies as a function of performance improvement and is usually presented in a more graded manner (e.g., "you overshot the target by 35 cm"). Augmented feedback plays a major role in performance improvement (R. A. Schmidt & Lee, 2020), especially at the earlier stages of learning (Newell, 1976) and, from a motor learning perspective, graded feedback is more advantageous as it provides learners with more information that can be used to flexibly make performance adjustments.

Very few studies have investigated the relationship between RewP and graded feedback processing (e.g., Ulrich & Hewig, 2014) and fewer have done so using a motor learning paradigm. One exception is the study by Frömer et al. (2016) in which participants performed a virtual throwing task and received visual graded feedback about where each throw landed relative to the target's bullseye. Results showed that, at the trial level, RewP amplitude was positively associated with performance accuracy such that more accurate throws resulted in larger RewP amplitude, which is in line with the reinforcement learning prediction that larger rewards (more accurate performance) lead to larger positive reward-prediction errors. Additionally, RewP amplitude was larger following trials where participants missed as opposed to hit the target, suggesting that they lowered their expectation for future positive outcomes after unsuccessful trials, which in turn led to a larger positive-reward prediction error when the outcome was better than anticipated. Results also showed that RewP amplitude decreased as participants' hit frequency increased, which is expected under a reinforcement learning framework since participants with higher accuracy expect to receive rewards more frequently, lowering their positive reward-prediction error for successful performances. Finally, the effect of hit frequency on RewP amplitude was weaker after misses as opposed to hits, likely because more accurate participants lower their expectations for success after an unexpected miss.

The study by Frömer et al. (2016) allowed feedback processing, an important aspect of motor skill acquisition, to be investigated in a more realistic setting, wherein feedback was based on participants' performance and provided in a graded manner. Furthermore, the task choice allowed reinforcement learning predictions to be applied to a more complex motor skill, expanding our understanding of reward processing under different task demands. However, this study did not explore the relationship between RewP and delayed post-test. This is one of the gaps in the literature as past research has focused on changes over short timescales (Bellebaum & Daum, 2008; Reinhart & Woodman, 2014), making it unclear whether reward-prediction errors relate to more long-lasting changes in performance. This is particularly pertinent when it comes to applying reinforcement learning principles to comprehend how motor skills are acquired and retained over time. From a reinforcement learning perspective, reward-prediction errors experienced during a training session drive acute behavior adaptation, leading to better practice performance. In theory, better practice performance should positively correlate with long-lasting changes in performance. However, motor learning studies have shown that performance during a training session does not necessarily correlate and, in some cases, is inversely correlated with performance on delayed post-tests (Kantak & Winstein, 2012).

Although the relationship between RewP and long-term changes in performance have not been examined with respect to a complex motor skill, Lohse et al. (2020) investigated this relationship in a perceptual category learning task, and the authors found that the RewP did not predict long-term learning as indexed by performance on one-week retention and transfer tests. Lohse et al. also examined the relationship between RewP and trial-to-trial behavior adjustments, which Frömer et al. (2016) did not do. Lohse et al. found that, during the acquisition phase, larger RewP amplitude was associated with a greater probability of changing response the next time a stimulus from the same category was presented. This is incompatible with the RewP serving as a reward mechanism that reinforces behavior, because this would have been manifested as larger RewPs being associated with a smaller probability of changing a response. Based on the results from Lohse et al., it is possible that the RewP represents reward-prediction errors that reflect a violation of reward expectation (i.e., an epiphenomenal representation of internal model update) instead of reward-prediction errors that drive behavior adaptation (i.e., a reward signal that stamps in behavior to maximize the likelihood of receiving a reward).

Building off past research (Frömer et al., 2016; Lohse et al., 2020), we conducted an exploratory study that included a complex motor learning task, graded feedback, and delayed post-tests and used mixed-effects regression models to test reinforcement learning predictions and their underlying mechanisms in short- and long-term motor behavior adaptation. Specifically, we investigated (1) the effect of performance accuracy on the RewP. According to reinforcement learning, more accurate performance is associated with more positive reward-prediction errors (Frömer et al., 2016). Thus, we predicted that, at the within-subject level, RewP would be more positive for more accurate compared to less accurate trials. Additionally, we examined the effect of participants' average accuracy (at the between-subject level) on the RewP, since Frömer et al. (2016) found that participants' cumulative accuracy influenced RewP amplitude. We also tested (2) whether RewP predicted trial-to-trial performance adjustments during acquisition. Following reinforcement learning predictions, larger RewPs should lead to smaller adjustments in performance (i.e., repetition of previously rewarded behavior). Thus, we predicted that a large RewP on the previous trial would be associated with a small adjustment in performance. Finally, we investigated (3) whether aggregate RewP (averaged across practice trials) predicted long-term behavior adaptation as indexed by performance on 24-hr post-tests (i.e., retention and transfer). One prediction from reinforcement learning is that accrual of larger RewPs (more positive reward-prediction errors) during practice should result in a larger aggregate RewP and better learning. Thus, controlling for pretest, we predicted a positive correlation between aggregate RewP amplitude and post-test performance.

Methods

Prior to data analysis, research questions and main statistical models were pre-registered and made available at the Open Science Framework repository (Link).

Participants

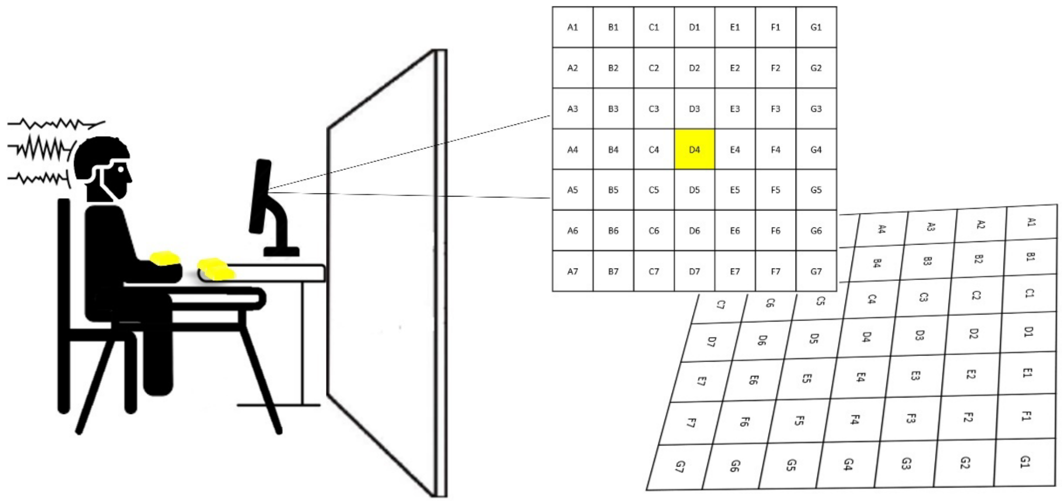
Data from 134 participants (females = 100, M_{age} = 20.72, SD = 1.64 years) were used in the present study. All participants were right-handed ($M_{\text{handedness score}}$ = 77.30, SD = 27.24; Oldfield, 1971) or reported having a strong preference for using their right hand to throw and reported not having any neuromuscular impairment that would affect performance of the experimental task. This dataset was collected during a larger, university-approved (Auburn University research protocol # 19-046 EP 1902) motor learning study (Bacelar et al., 2022). All participants gave written consent prior to day 1 of data collection and verbal consent prior to day 2 of data collection. Given the exploratory nature of the present study, no a-priori power calculation was carried out.

Task

Participants performed a nondominant arm bean bag tossing task. The goal of the task was to make the bean bag land as close to the center of the target as possible (i.e., D4, Figure 1). The target consisted of a grid comprising 49 equal-sized squares, each one assigned a letter and a number indicating the square position (e.g., D4: square located in the center of the target). Participants sat in front of a table located three meters away from the center of the target. The table accommodated ten bean bags and a computer monitor used to deliver feedback, and the table served as a support for a pasteboard used to occlude participants' vision of the target (Figure 1). Another small table was placed next to participants' right arm to serve as a support for a keyboard used to initiate feedback presentation. From a sitting position, participants were instructed to grasp a bean bag with their left hand pronated and toss it over the occlusion board by elevating their arm and flicking their wrist. (For more details about the task see Bacelar et al., 2022.)

Figure 1

Experimental Set-up



Note. The left side of the figure shows the participant in a sitting position while having their brain activity recorded. A pasteboard is blocking their vision of the target. The right side of the figure illustrates how feedback was delivered throughout the experiment.

Procedures

Acquisition Phase

To determine baseline skill level, a 10-trial pretest without feedback was carried out before the acquisition phase. (Participants were allowed to see the target for 10s before initiating the pretest.) Next, participants were quasi-randomly assigned (based on sex) to one of four experimental conditions that varied according to whether feedback schedule was self-selected by the participant (self-control) or determined by a counterpart (i.e., another participant; yoked) and whether performance estimation (i.e., estimating where the bean bag landed after each trial) was required (error estimation) or not (traditional)[17]. Participants then underwent the acquisition phase, which consisted of 10 blocks of 10 trials with a 1-minute break between blocks. As in the pretest, participants were allowed to see the target for 10s before initiating the acquisition phase. Feedback was presented in 5 of 10 trials per block (i.e., 50% of the time) for all participants. Specifically, for feedback trials, feedback was initiated as soon as the participant pressed the "enter" key on the keyboard after being prompted by the word "ready" on the computer screen. First, participants saw an image of target on the screen for 2000ms and then the square where the bean bag landed changed to being highlighted in yellow, as shown on the upper-right side of Figure 1; the latter image remained on the screen for 1000ms. For trials that landed off target, participants saw an image of the target on the screen for 2000ms followed by a red X presented for 1000ms.

Post-tests (Retention and Transfer)

Approximately 24 hours after acquisition phase, participants returned to the laboratory to perform a retention and a transfer test. For the retention test, participants performed the same bean bag tossing task practiced on Day 1, whereas for the transfer, participants were positioned farther away from the target (i.e., four instead of three meters away). Post-tests consisted of one block of ten trials each and were carried out in counterbalanced order. Participants were allowed to see the target for 10s before initiating each post-test, but no feedback was presented during the post-test.

EEG Recording

[17] These experimental conditions were created to test predictions made by a motor learning theory (OPTIMAL theory; Wulf & Lewthwaite, 2016) and the results of these manipulations have been presented in another publication (Bacelar et al., 2022). We account for these manipulations in our statistical models even though they are not of primary interest in the present study.

EEG was recorded during the acquisition phase. EEG activity was recorded from 14 scalp electrodes using a 64-channel BrainVision actiCAP system (Brain Products GmbH, Munich, Germany) labeled in accord with an extended 10-20 international system (Oostenveld & Praamstra, 2001). The left earlobe served as the online reference and the FPz electrode site served as the common ground. Electrode impedances were maintained below 25kΩ throughout the experiment. A high-pass filter set at 0.016 Hz was applied and the sampling rate was set at 250 Hz. A BrainAmp DC amplifier (Brain Products GmbH) linked to BrainVision Recorder software (Brain Products GmbH) was used to amplify and digitize the EEG signal.

EEG Processing

EEG data processing was conducted with BrainVision Analyzer 2.2 software (Brain Products GmhB). First, raw data was visually inspected and malfunctioning electrodes were interpolated. Next, data were re-referenced to the average of both left and right ears. A 1 – 40 Hz band-pass filter with 4th order roll-offs and a 60 Hz notch filter was applied to the re-referenced data in preparation for the independent component analysis (ICA) step. Non-stereotypical artifacts were then marked in the interval between the beginning of block 4 and the end of block 5 of the acquisition phase. This interval was chosen as it minimizes the presence of non-stereotypical artifacts that are either due to the participant's adjustment to the task (i.e., earlier blocks) or tiredness (i.e., toward the end of practice). After this step, an ICA was conducted to identify components representing stereotypical artifacts (e.g., saccades and blinks), which were subsequently removed from the *unfiltered* data. Finally, the cleaned data were filtered using an infinite impulse response band-pass filter between 0.1 and 30 Hz with 4th order roll-offs and a 60 Hz notch filter.

Measures

Psychophysiological Measures

Single-trial RewP. First, filtered EEG data were segmented into epochs beginning 200 ms before and ending 800 ms after feedback stimulus onset (square highlighted in yellow or red X). During the segmentation step, participants' 20 best trials were selected (i.e., 20 trials closest to the center of the target; Marco-Pallares et al., 2011). Some participants ended up with more than 20 trials as trial selection was carried out in a stepwise manner to ensure that all trials equidistant to the center of the target were included. For example, trials that landed on the innermost square, D4, were included first. If the number of trials included did not add up to a minimum of 20, all trials that landed on the second group of squares equidistant to the center of the target (i.e., trials landing on C3, C4, C5, D3, D5, E3, E4, and E5) were included. This process continued until the minimum number of 20 trials was achieved. After segmentation, epochs were baseline corrected from -200 ms to 0 ms. Next, epochs were automatically rejected if they contained a change of more than 50 μV from one data point to the next, a change of 100 μV or greater within a moving 200-ms window, or a change of less than 0.5 μV within a moving 200-ms window in any of the midline electrodes (Fz, FCz, Cz, and Pz). Then, to determine the time window for RewP quantification, epochs were averaged. Considering that RewP peak latency may vary across individuals (e.g., Lohse et al., 2020), each participant's RewP time window was adapted based on the participant's RewP peak latency at the electrode FCz (Clayson et al., 2013). The most positive deflection within the 230 – 350 ms time window that exhibited a frontocentral scalp distribution was recorded. If no component exhibited a frontocentral scalp distribution, the most positive deflection within the 230 - 350 ms time window was recorded. After determining the RewP peak for each participant, data were re-segmented to include all feedback trials (i.e., 50 feedback trials). The next steps included baseline correction and epoch automatic artifact rejection following the specifications described above. Additionally, the first author visually inspected all 50 epochs, and removed the ones that exhibited marked artifact but that were not removed in the automatic rejection step. Next, a 40 ms time window was centered on each participant's previously recorded peak amplitude at FCz, Fz, and Cz on *each epoch*, and then mean amplitude in this time window for these electrodes was computed. Finally, we averaged across FCz, Fz and Cz to obtain the single-trial RewP for each feedback trial.

Aggregate RewP. Aggregate RewP was obtained by averaging across all single-trial RewP trials for each participant. This method of RewP quantification diverges from the difference wave approach (Luck, 2005) mainly due to the task and experimental design adopted. Specifically, for the difference wave approach, we would need at least 20 good trials and 20 bad trials to reliably compute aggregate RewP (Marco-Pallares et al., 2011). However, many participants do not have 20 trials that are closer to the center of the target than the remaining trials, because of the number of trials that are equidistant from the target (see above).

Behavioral Measures

Absolute Change in Constant Error (CE). Absolute change in CE was obtained by computing the difference between CE on the current trial and on the previous trial and then taking the absolute value of that difference as follows: Absolute Change in CE = |CE.c – CE.p|, where CE.c represents CE on the current trial and CE.p represents CE on the previous trial (see Lee & Carnahan (1990) for a similar use of this measure). CE is a measure of accuracy that indexes the magnitude of the error along one axis (R. A. Schmidt & Lee, 2020). The formula to calculate constant error is simply the difference between the magnitude of the error in one axis (e.g., x-axis) and the goal, as follows: CE = X – T, where x is the magnitude of the error along the axis of interest minus the goal (e.g., bullseye = 0). In the present study, absolute change in

CE was computed separately for x- and y-axis and for the second through 100th trial of the acquisition phase.

Single-trial and Average Radial Error. Radial error (RE) is a measure of accuracy for two-dimensional performance tasks (Hancock et al., 1995). The formula to obtain RE is as follows: $(x^2 + y^2)^{1/2}$, where X and Y correspond to the magnitude of the error along the x- and y-axis, respectively. In the present study, we computed RE on a trial-by-trial basis for the acquisition phase (here referred to as single-trial RE) and as an aggregate measure for pretest, acquisition phase, retention test and transfer test (here referred to as average RE). The software Dartfish® was used to record the magnitude of the error along the x- and y-axis for the first 100 participants. More specifically, an iPad mounted to the celling right above the center of the target recorded the entire data collection session (i.e., pretest, acquisition, and post-tests). The video captured exactly where each bean bag landed during task performance. Next, recorded videos were imported onto Dartfish where x and y measures were obtained. For the remaining participants, the program LabView® equipped with the virtual instrument *ScorePutting* (Neumann & Thomas, 2008) was used to compute the magnitude of the error along the x- and y-axis. We used data from three participants to confirm the consistency between Dartfish® and LabView® in obtaining these measures ($r \geq .995$).

Data Analysis

In the pre-registration form, we stated that only usable feedback trials as defined by trials that were not marked with an artifact (i.e., EEG processing stage) and trials that landed on target and, therefore, received meaningful feedback[18] would be included in the statistical models.

[18] The rationale behind considering trials that landed off target as non-usable stems from the idea that for off-target trials, participants only received a red X as feedback, with no additional information that would allow them to distinguish between a near miss or a throw that missed the target by a considerable amount. Since graded feedback

However, due to the experimental design, participants received feedback in only 50% of the total number of trials, and excluding trials that landed off target would result in a considerable amount of data loss (1478 trials landed off target). Thus, supplemental analyses using a subset of the data (n = 64) were conducted to gather evidence that could justify the inclusion of off-target trials in the statistical analyses. First, we computed a new measure of accuracy based on feedback delivery (i.e., feedback bands). Specifically, trials that landed on D4 comprised feedback band 1, trials that landed on C3, C4, C5, D3, D5, E3, E4, and E5 comprised feedback band 2, and so on. Next, we assessed the relationship between single-trial RewP amplitude (centered around each participant's mean) and accuracy as indexed by feedback band. Results showed that RewP amplitude decreased as a function of accuracy (i.e., lower RewP amplitude for trials farther away from the center of the target). For trials that landed off target (band 5), specifically, RewP amplitude was negative (M = -0.80μV), suggesting that these trials were processed as an error (see the Appendix 1 for more details). Based on these assessments, we decided to include all feedback trials in the statistical analyses.

During the single-trial RewP extraction process, 2.37% (n = 159) of the total number of RewP trials (N = 6700) were lost due to data artifacts. Moreover, prior to statistical analyses, we visually inspected the distribution of errors (i.e., CE) along the x- and y-axis, which led to the identification of extreme values in the y-axis. To mitigate the influence of these extreme cases on other subsequent variables (e.g., RE is computed from CE) and the models, we decided to exclude errors equal or greater than 140cm in both directions since errors of that magnitude imply that the participant missed the center of the target by more than the length/width of the

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was the main focus of the experiment, keeping trials that landed off target in the analyses could result in more noise being added to the models as opposed to information, hence the initial decision to exclude these trials.

target (140cm x 140cm). Exclusion of extreme values in both x- and y-axis led to the loss of 577 data points (4.31% of the data; 577 of 13400 data points).

All analyses were conducted in R (cran.r-project.org) using the following packages: *tidyverse* (Wickham et al., 2019), *lme4* (Bates et al., 2015), and *lmerTest* (Kuznetsova et al., 2017). All figures were created with *ggplot2* (Wickham, 2016). Alpha level was set at .05. For each model, residual plots were visually inspected to check assumptions of normality and homogeneity of variance. To account for non-normal residual distributions, we ran a non-parametric bootstrapping procedure to ensure we had robust confidence intervals. All processed data and code to run the analyses are available online at OSF repository (Link). Discrepancies between the information presented here and in the pre-registration form are explained in detail in Appendix 2.

Performance Accuracy and its Effect on the RewP

To investigate the effect of performance accuracy on the RewP, we ran a linear mixed-effects regression model with fixed effects of training condition (self-control, error estimation and their interaction), trial, current single-trial RE (accuracy on the current trial), average RE, and their interactions. The model included random effects of participant and Participant x Single-trial RE[19]. The dependent variable was single-trial RewP on the current trial. Single-trial RE was centered around each participant's mean, representing a measure of within-subject variability, whereas average RE was centered around the sample mean, representing a measure of between-subject variability. All continuous variables were z-transformed and the categorical variable training condition was contrast-coded.

RewP and Performance Adjustments During Acquisition

[19] In the pre-registration form, this model only included the random effect of participant. However, we decided to adopt a more conservative approach and add the random slopes of single-trial RE.

To investigate whether RewP predicted performance adjustments during the acquisition phase, we ran a linear mixed-effects model with fixed effects of training condition (self-control, error estimation and their interaction), trial, previous absolute error (accuracy on the previous trial), dimension (whether the error was along the x- or y-axis), previous single-trial RewP (RewP on the previous trial) and their interactions. The model included random effects of participant and Participant x Trial[20]. The dependent variable was absolute change in CE. The variable trial was z-transformed and all categorical variables (training condition and dimension) were contrast-coded. Absolute change in CE and previous absolute error were used in their raw units to facilitate result interpretation.

RewP and Long-term Behavior Adaptation

To assess whether RewP predicted long-term behavior adaptation, we ran a linear mixed-effects model with fixed effects of pretest RE, post-test type, and their interaction, training condition (self-control, error estimation and their interaction), the interaction between training condition and post-test type, aggregate RewP, and the interaction between aggregate RewP and post-test type. The model included random effects of participant. The dependent variable was average post-test RE. All continuous variables were z-transformed and the categorical variables (training condition and post-test type) were contrast-coded.

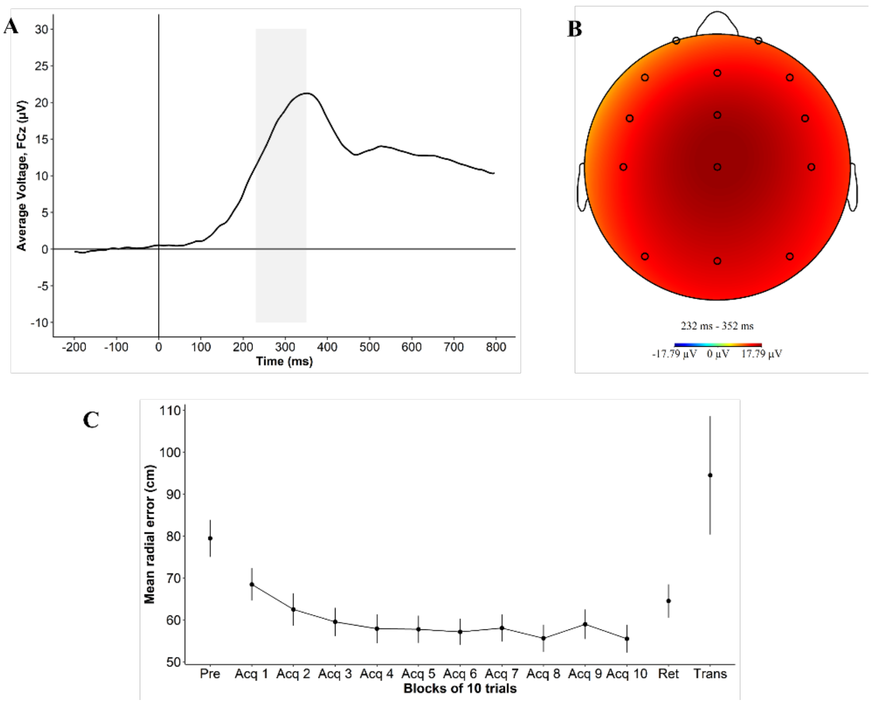
Results

Figure 2A depicts the grand average ERP at electrode FCz, Figure 2B depicts RewP topography, and Figure 2C depicts performance accuracy as indexed by radial error across study phases.

[20] Initially, we used a conservative approach and included random effects of participant and random slopes of trial, previous absolute error, and previous single-trial RewP since previous absolute error and previous single-trial RewP vary within subjects. However, the model did not converge. Thus, after inspecting the correlation and the variance accounted for by each random effect, we decided to drop the random slopes of absolute error and previous single-trial RewP.

Figure 2

Psychophysiological and Behavior Data



Note. A: Grand average waveform for the RewP time-locked to the onset of augmented feedback (time 0) at electrode FCz. Shaded area represents the RewP time window (230ms-350ms). B: Topography of the RewP averaged across trials and training conditions. C: Radial error in cm (lower numbers indicate better performance) averaged across training conditions and as a function of study phase (pretest, acquisition, retention, and transfer). Error bars represent 95% CIs.

Performance Accuracy and its Effect on the RewP

Results of the analysis of the effect of performance accuracy on the RewP are presented in Table 1. The analysis revealed a significant positive main effect of trial (p = .005), indicating that RewP amplitude increased throughout the acquisition phase (on average), and a significant negative main effect of current single-trial RE (p = .001), indicating that more accurate throws were associated with more positive RewPs (Figure 3). No main effect of average RE was found (p = .479). There were no other main effects or interactions ($ps \geq .105$).

Table 1

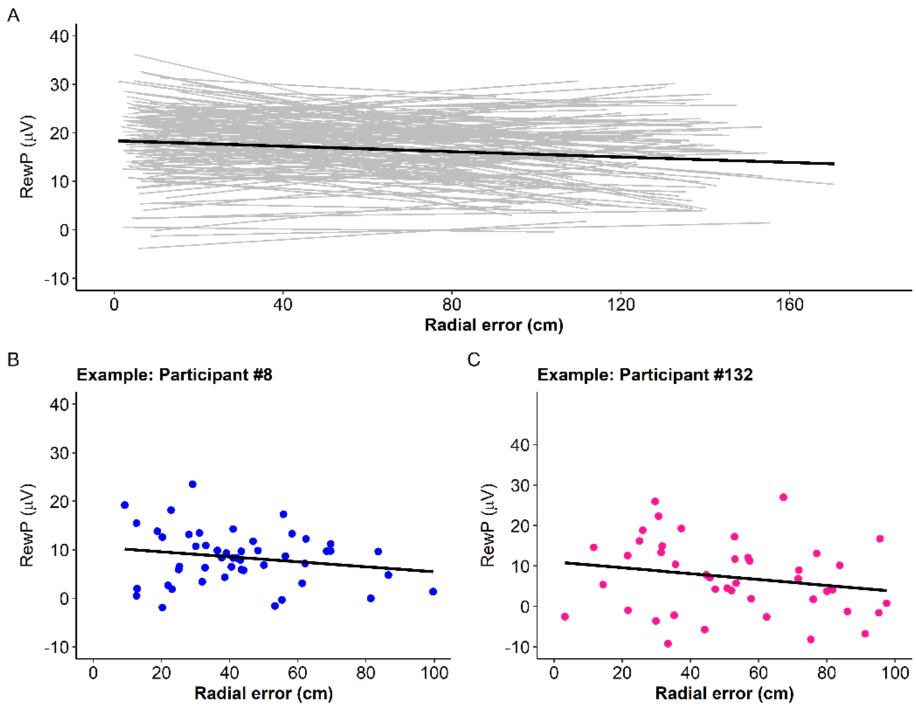
Random and Fixed Effects for the Analysis of the Effect of Performance on the RewP

Random Effects				
Group	*Effect*	*Variance*	*SD*	*Corr*
Participant	Intercept	0.31	0.56	
	Single-trial RE	0.01	0.12	0.08
Residual		0.68	0.82	
Fixed Effects				
Effects	*β*	*95% CI*	*t*-value	*p*-value
Intercept	-0.01	[-0.10; 0.01]	-0.11	.912
Self-control	-0.13	[-0.33; 0.05]	-1.32	.188
Error Estimation	-0.12	[-0.32; 0.06]	-1.27	.207
Trial	0.03	[0.01; 0.05]	2.82	.005**
Single-trial RE	-0.05	[-0.08; -0.02]	-3.34	.001**
Average RE	-0.03	[-0.14; 0.06]	-0.71	.479
Self-control x Error Estimation	0.08	[-0.31; 0.48]	0.41	.683
Trial x Single-trial RE	0.02	[-0.00; 0.04]	1.62	.105
Trial x Average RE	0.00	[-0.02; 0.02]	-0.41	.684
Single-trial RE x Average RE	-0.02	[-0.05; 0.01]	-1.17	.245
Trial x Single-trial RE x Average RE	0.00	[-0.02; 0.02]	0.33	.744

Note. Number of observations: 6327, groups: Participant, 134. All variables were z-transformed prior to analysis. Single-trial RE was mean-centered around each participant's mean; average RE was mean-centered around the sample's mean. Self-control was coded as self-control = -0.5; yoked = 0.5. Error estimation was coded as error estimation = -0.5; traditional = 0.5.

Figure 3

Single-trial RewP as a Function of Single-trial Radial Error



Note. A: Figure shows the relationship between single-trial RewP and single-trial RE. The black line represents the negative relationship between single-trial RewP and single-trial RE for the sample, whereas the gray lines represent the slope for each participant. B and C: Figures show the relationship between single-trial RewP and single-trial RE at the within-subject level for two random participants.

RewP and Performance Adjustments During Acquisition

To analyze whether RewP predicted performance adjustments during the acquisition phase, we modeled absolute change in CE as a function of trial, previous absolute error, dimension (x-axis, y-axis), previous single-trial RewP, and their interactions, controlling for training condition (self-control, error estimation and their interaction). Results revealed two three-way interactions of Trial x Previous Absolute Error x Dimension (p = .002) and Trial x Dimension x Previous Single-trial RewP (p = .028) (see Appendix 3 for details). Since both three-way interactions included dimension, we analyzed the relationship between RewP and performance adjustments separately for each axis.

For performance adjustments over the x-axis, we ran a linear mixed-effects regression model with absolute change in CE serving as the dependent variable and fixed effects of training condition (self-control, error estimation and their interaction), trial, previous absolute error, previous single-trial RewP and their interactions. The model included random effects of participant and Participant x Trial. This analysis, summarized in Table 2, revealed a significant positive main effect of previous absolute error ($p < .001$), indicating that larger errors on the previous trial led to larger performance adjustments, and a significant Trial x Previous Single-trial RewP interaction ($p = .029$), suggesting that later in practice larger RewPs (following a successful throw) were associated with smaller adjustments in performance (Figure 4). No other main effects or interactions were found (*ps* ≥ .235).

Table 2

Random and Fixed Effects for the Analysis of the Effect of RewP on Performance Adjustments (x-axis)

Random Effects				
Group	*Effect*	*Variance*	*SD*	*Corr*
Participant	Intercept	14.44	3.80	
	Trial	5.18	2.28	-0.08
Residual		492.54	22.19	
Fixed effects:				
Effects	*β*	*95% CI*	*t*-value	*p*-value
Intercept	18.56	[17.43; 19.62]	32.81	<.001***
Self-control	0.77	[-1.00; 2.50]	0.89	.373
Error Estimation	0.78	[-1.01; 2.41]	0.90	.368
Trial	0.09	[-0.91; 1.09]	0.17	.866
Previous Absolute Error	0.50	[0.46; 0.53]	30.93	<.001***
Previous Single-trial RewP	-0.09	[-0.98; 0.80]	-0.21	.837
Self-control x Error Estimation	-0.25	[-3.95; 3.36]	-0.14	.887

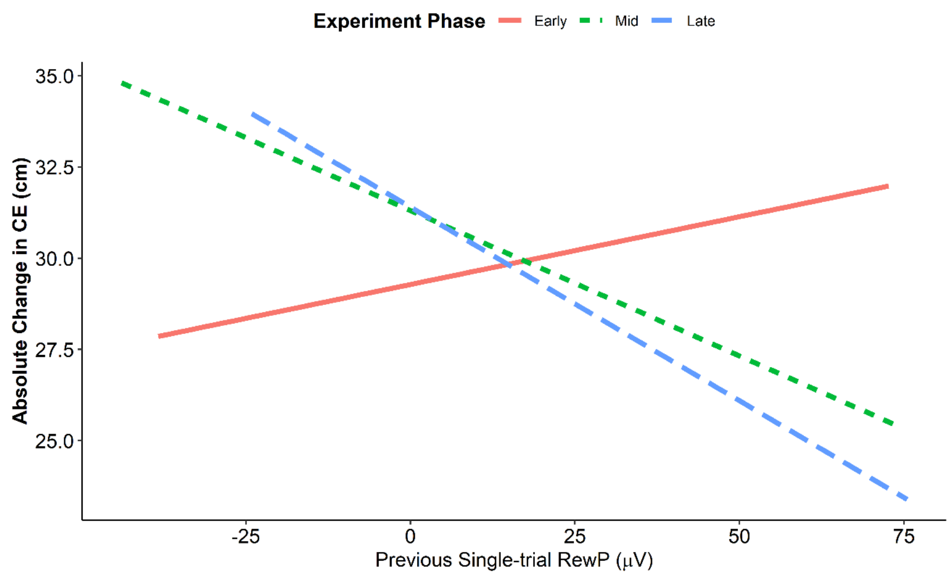
Trial x Previous Absolute Error	0.01	[-0.03; 0.04]	0.31	.754
Trial x Previous Single-trial RewP	-1.01	[-1.88; -0.12]	-2.18	.029*
Previous Absolute Error x Previous Single-trial RewP	0.00	[-0.03; 0.03]	-0.13	.897
Trial x Previous Absolute Error x Previous Single-trial RewP	0.02	[-0.01; 0.05]	1.19	.235

Note. Number of observations: 6244, groups: Participant, 134. All variables were z-transformed prior to the analysis except for absolute change in CE and previous absolute error. Self-control was coded as self-control = -0.5; yoked = 0.5. Error estimation was coded as error estimation = -0.5; traditional = 0.5.

For performance adjustments over the y-axis, we ran a linear mixed-effects regression model with the same fixed and random effects as specified in the previous model (x-axis). Results (Table 3) showed significant main effects of trial ($p = .033$) and previous absolute error ($p < .001$), which were superseded by a significant Trial x Previous Absolute Error interaction ($p < .001$), indicating that larger errors on the previous trial led to larger performance adjustments, and this relationship was stronger later in practice (Figure 5). There was no main effect of previous single-trial RewP ($p = .143$). No other main effects or interactions were found (*ps* ≥ .106).

Figure 4

Absolute Change in CE as a Function of Single-trial RewP and Trial



Note. Figure represents the interaction between single-trial RewP and trial for the x-axis. Lines represent the experiment phase. Specifically, the solid line (cherry) represents early in the experiment, the dotted line (green) represents half-way through the experiment, and the dashed line (blue) represents later in the experiment.

Table 3

Random and Fixed Effects for the Analysis of the Effect of RewP on Performance Adjustments (y-axis)

Random Effects				
Group	*Effect*	*Variance*	*SD*	*Corr*
Participant	Intercept	82.40	9.08	
	Trial	19.86	4.46	-0.10
Residual		1252.60	35.39	
Fixed Effects				
Effects	*β*	*95% CI*	*t*-value	*p*-value
Intercept	33.87	[31.71; 36.12]	30.30	<.001***
Self-control	-0.40	[-4.07; 2.94]	-0.22	.824
Error Estimation	1.39	[-2.07; 4.86]	0.77	.443
Trial	-1.90	[-3.53; -0.30]	-2.14	.033*
Previous Absolute Error	0.36	[0.33; 0.39]	22.13	<.001***
Previous Single-trial RewP	1.16	[-0.27; 2.74]	1.46	.143
Self-control x Error Estimation	-2.66	[-10.18; 4.39]	-0.74	.462

Trial x Previous Absolute Error	0.08	[0.05; 0.11]	5.30	<.001***
Trial x Previous Single-trial RewP	1.09	[-0.57; 2.69]	1.35	.179
Previous Absolute Error x Previous Single-trial RewP	-0.02	[-0.05; 0.01]	-1.62	.106
Trial x Previous Absolute Error x Previous Single-trial RewP	-0.02	[-0.05; 0.01]	-1.33	.183

Note. Number of observations: 6244, groups: Participant, 134. All variables were z-transformed prior to the analysis except for absolute change in CE and previous absolute error. Self-control was coded as self-control = -0.5; yoked = 0.5. Error estimation was coded as error estimation = -0.5; traditional = 0.5.

RewP and Long-term Behavior Adaptation

Results of the analysis of the relationship between aggregate RewP and long-term behavior adaptation revealed a main effect of pretest (p = .003) such that participants with better baseline skill level performed better during the post-tests (Table 4). There was no main effect of aggregate RewP (p = .166) and no Aggregate RewP x Post-test Type interaction (p = .857), indicating that aggregate RewP did not predict post-test performance. No other main effects or interactions were found ($ps \geq .152$).

Table 4

Random and Fixed Effects for the Analysis of the Effect of RewP on Long-term Behavior Adaptation

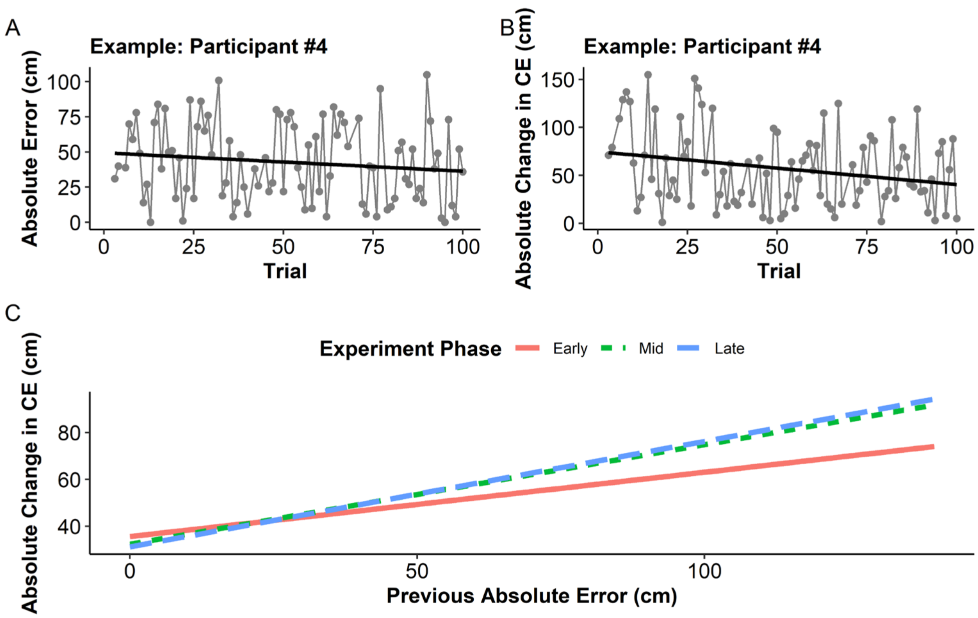
Random Effects				
Group	*Effect*	*Variance*	*SD*	
Participant	Intercept	0.42	0.65	
Residual		0.54	0.73	
Fixed Effects				
Effects	*β*	*95% CI*	*t*-value	*p*-value
Intercept	0.00	[-0.14; 0.14]	0.00	1.000
Pretest	0.22	[0.08; 0.37]	3.00	.003**
Post-test Type	0.00	[-0.20; 0.16]	0.00	1.000
Self-control	0.18	[-0.12; 0.45]	1.22	.226

Error Estimation	-0.18	[-0.45; 0.11]	-1.22	.226
Aggregate RewP	0.10	[-0.03; 0.25]	1.39	.166
Pretest x Post-test Type	-0.13	[-0.31; 0.04]	-1.44	.152
Self-control x Error Estimation	0.26	[-0.30; 0.86]	0.88	.381
Self-control x Post-test Type	-0.01	[-0.36; 0.34]	-0.08	.939
Error Estimation x Post-test Type	-0.23	[-0.59; 0.17]	-1.26	.210
Post-test Type x Aggregate RewP	-0.02	[-0.19; 0.16]	-0.18	.857
Self-control x Error Estimation x Post-test Type	0.21	[-0.47; 0.94]	0.58	.562

Note. Number of observations: 268, groups: Participant, 134. All variables were z-transformed prior to the analysis. Self-control was coded as self-control = -0.5; yoked = 0.5. Error estimation was coded as error estimation = -0.5; traditional = 0.5. Post-test type was coded as retention = -0.5; transfer = 0.5.

Figure 5

Relationship Between Absolute Change in CE, Previous Absolute Error, and Trial



Note. A: Figure represents the relationship between previous absolute error (averaged across participants) as a function of trial for a random participant. B: Figure represents the relationship between absolute change in CE (averaged across participants) as a function of trial for the same participant. C: Figure represents the interaction between Trial and Previous Absolute Error for the y-axis. Lines represent the experiment phase. Specifically, the solid line (cherry) represents early in the experiment, the dotted line (green) represents half-way through the experiment, and the dashed line (blue) represents later in the experiment.

Discussion

Reinforcement learning offers a strong theoretical framework to understand how we adapt our behavior based on the consequences of our actions. Despite the theory's prominence, few studies have tested its predictions in a motor learning context and fewer have investigated the mechanisms underlying the predictions. In the present study, we aimed to provide empirical evidence of how reinforcement learning predictions apply to motor skill acquisition and their underlying mechanisms in short- and long-term behavior adaptation. Specifically, we implemented mixed-effects regression models to explore a 134-participant dataset consisting of learners' feedback-evoked EEG activity (i.e., RewP) as well as their short- and long-term performance. Our goal was to investigate the (1) the effect of performance accuracy on the RewP, (2) whether RewP predicted trial-to-trial performance adjustments during acquisition, and (3) whether aggregate RewP predicted long-term behavior adaptation. Based on reinforcement learning theory, we predicted that, at the within-subject level, more accurate performances would be associated with more positive RewPs. We also predicted that a large RewP on the previous trial would result in a smaller adjustment in performance. Finally, we predicted a positive relationship between aggregate RewP and post-test performance.

Results from the first model (Table 1) support our prediction that better outcomes lead to more positive reward-prediction errors, as evidenced by RewP amplitude being more positive for more accurate trials compared to less accurate trials. This finding also supports the claim that RewP reflects reward-prediction errors and is sensitive to reward magnitude (Sambrook &

Goslin, 2015), and is in line with previous research investigating the psychophysiological correlates of graded feedback (Frömer et al., 2016; Luft et al., 2014; Ulrich & Hewig, 2014). For instance, Frömer et al., (2016) found a positive relationship between RewP amplitude and performance accuracy in a virtual throwing task where participants received graded feedback about performance. Interestingly, this gradual increase in RewP amplitude as a function of accuracy was found among on-target trials since only these were analyzed. The present study expands upon these results by showing the effect across successful and unsuccessful trials (i.e., trials that landed on and off board), which suggests that processing of correct and incorrect performance feedback in a motor learning context is similar to processing of correct graded feedback (Frömer et al., 2016) and categorical feedback (Hajcak et al., 2006). We also found that, controlling for accuracy on the current trial, RewP amplitude was larger later in practice for a given level of error, probably because toward the end of the acquisition phase predictions about performance started to stabilize. Specifically, early in practice, the lack of familiarity with the task and weak internal model render predictions less stable (i.e., there is more variance in the predictions since the learner may not have a sense of what their performance was). Later in practice, the predictions start to stabilize as the learner becomes more knowledgeable about the task and develops a stronger internal model. Thus, a prediction violation (better-than-expected outcome) at this stage should lead to a larger RewP for a given level of error.

Even though we did not make any directional prediction, we included average radial error in the first model to investigate the effect of participants' average performance on the RewP. From a reinforcement learning perspective, there could be an interaction between current and average accuracy. This is because participants who are more accurate (i.e., showing smaller average RE) might show smaller RewPs at the trial level as they expect rewards more frequently, lowering their reward-prediction error. Moreover, Frömer et al., (2016) found that RewP amplitude was inversely correlated with participants' cumulative accuracy. Thus, adding measures of within- and between-subject performance to our model allowed us to test for the main effect of average RE and the interaction between single-trial RE and average RE. Contrary to Frömer et al. (2016)'s findings, we found no evidence of the effect of participants' average performance on the RewP. This inconsistency might be due to the fact that Frömer et al. only analyzed trials that landed on target, which might have driven the main effect of average accuracy. This is because participants with low hit rates received feedback about outcomes that were unexpectedly good, resulting in larger RewPs. On the other hand, participants with high hit rates received feedback about outcomes that were relatively expected, resulting in smaller RewPs.

Regarding the relationship between RewP and performance adjustments during acquisition we found that, for errors along the x-axis, the effect of previous single-trial RewP on absolute change in CE varied as a function of trial. Specifically, early in practice, after a successful trial, a larger RewP was associated with a larger adjustment in performance. This is inconsistent with the reinforcement learning prediction that large positive reward-prediction errors lead to behavior repetition but is in line with recent research findings (Lohse et al., 2020). For instance, Lohse et al. (2020) found that larger current single-trial RewP amplitude was associated with a greater probability of changing response in a perceptual category learning task the next time a stimulus from the same category was presented, which is consistent with the notion that the RewP reflected an unexpected reward, likely because the correct association between stimulus and response had not been learned yet. Along the same lines, the positive relationship between RewP and performance adjustments we observed early in practice may be due to larger RewPs reflecting surprisingly good outcomes but a lack of knowledge about how to repeat the precipitating action due to a weak internal model. Interestingly, toward the end of practice, a larger RewP after a successful trial was associated with a smaller adjustment in performance, as predicted by reinforcement learning theory. This finding might be explained by a stabilization in performance predictions (see previous paragraph) and/or an improvement in participants' capability to reproduce the previously rewarded behavior due to a strong internal model.

For errors along the x- and y-axis, we found that larger errors on the previous trial were associated with larger adjustments in performance, and this relationship became stronger later in practice exclusively for the y-axis. This suggests that toward the end of practice, for a given level of error, participants made larger performance adjustments, likely because they became better at calibrating their own movement, which is expected with skill acquisition and the strengthening of an internal model (Schmidt & Lee, 2020). Notably, the task constraints in our experiment afforded more opportunity to adjust performance along the y-axis than the x-axis, specifically by increasing arm movement amplitude or velocity. Even though participants improved their movement calibration, performance adjustments were not predicted by the RewP. We acknowledge that these results are difficult to reconcile with the results from the x-axis, but as described in Luft (2014)'s review, future behavior correction is not always predicted by the RewP. In fact, evidence in favor of the RewP as a predictive signal of behavior adjustment on the next trial is highly mixed.

Finally, counter to our prediction, the analysis of the relationship between aggregate RewP and long-term behavior adaptation showed that aggregate RewP did not predict post-test performance. Similar results were found in the study by Lohse et al. (2020) where aggregate

RewP amplitude did not predict performance on one-week retention and transfer tests. In another study in the motor learning domain, RewP was associated with practice performance but not learning (Grand et al., 2017b). Together, the present and past studies call attention to the importance of examining the RewP-behavior relationship over different timescales, especially in psychophysiological studies focused on uncovering the basis of the learning process, as there may not be a direct correspondence between RewP and behavior adaptation over short-term and long-term scales. Our findings in particular also strengthen the notion that the motor skill acquisition process is complex and multifaceted, and that a more complete explanation of how this process occurs might come from an approach that combines more than one mechanism (e.g., reinforcement learning, model-based learning, use-dependent plasticity; Kantak & Winstein, 2012).

Conclusion

The present study investigated reinforcement learning predictions and their underlying mechanisms in short- and long-term motor behavior adaptation. Our results showed that the RewP behaved as a measure of reward-prediction errors, being more positive for more accurate compared to less accurate trials. Importantly, the effect of graded feedback on the RewP was shown across successful (on-target) and unsuccessful (off-target) feedback. Moreover, we found that single-trial RewP was implicated in performance adjustments along the x-axis but not the y-axis. To our knowledge, this is the first study to investigate how a psychophysiological measure of reward-prediction error, a major driver of reinforcement learning, is associated with performance adjustments in a motor skill learning context. Even though our results suggest that RewP is involved in short-term behavior adjustments that occur over the course of acquisition, we found no evidence of the relationship between aggregate RewP and post-test performance.

More studies are needed to elucidate how reinforcement learning theory and its neural mechanisms can explain motor skill learning.

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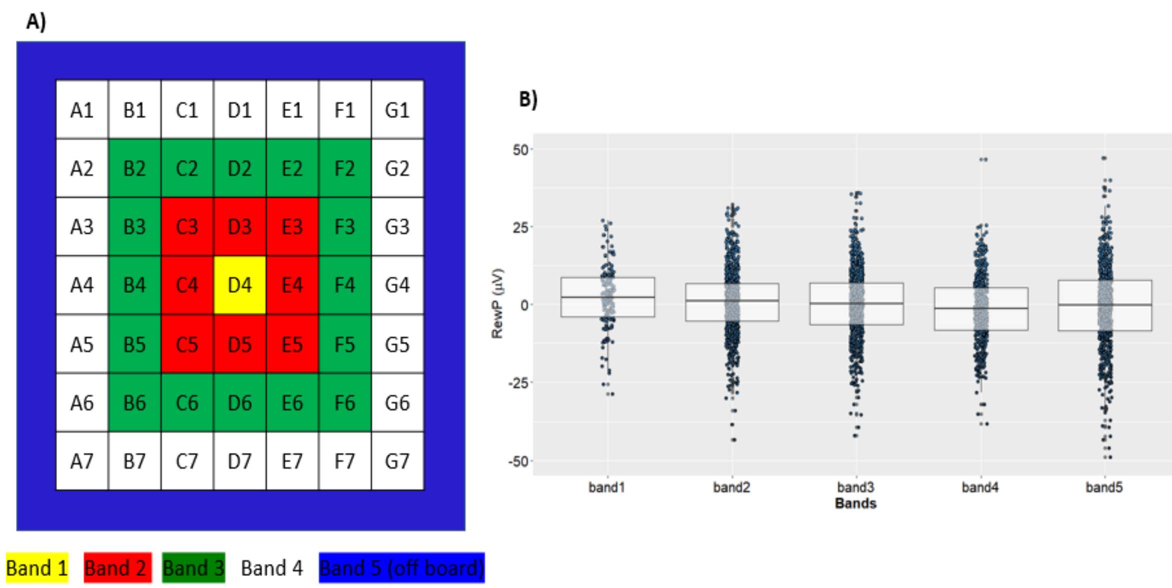
Appendix 1

Relationship Between Single-trial RewP and Feedback Band

Figure A1

Relationship Between Single-trial RewP and Performance Accuracy



Note. A: Feedback bands as a function of where the bean bag landed on the target . B: Graphical representation of the relationship between single-trial RewP and feedback band. $M_{Band\ 1}$ = 2.27, $M_{Band\ 2}$ = 1.10, $M_{Band\ 3}$ = 0.16, $M_{Band\ 4}$ = -1.46, $M_{Band\ 5}$ = -0.80.

Appendix 2

Discrepancies Between the Pre-Registration Form and What Was Presented in Chapter 4

The original version of the pre-registration form is presented below along with any changes that have been made to the document, which are presented after each question.

1) What's the main question being asked or hypothesis being tested in this study?

This study focuses on the neurophysiological mechanisms of reinforcement learning in a motor learning context. Specifically, using EEG data from a study that investigates explanations for the learning benefit of practicing a motor skill with self-control of augmented feedback, our goal is to use mixed-effects models to answer the following questions.

- Question 1: Does the reward positivity (RewP) predict performance during acquisition across participants?
- Question 2: What is the effect of single-trial RewP on performance adjustments during acquisition?
- Question 3: What is the effect of performance on the RewP?
- Question 4: How does the RewP affect long-term learning?

Details of how we will test each of these questions statistically are presented below.

Divergencies

Chapter 4 was focused on questions 2, 3, and 4 as they reflect the novelty of the study. In recognition of its relevance, the analysis of the whether the RewP predicts performance during acquisition across participants (Question 1) is presented below.

Table A2

Fixed Effects for the Analysis of the Effect of Aggregate RewP on Performance During Acquisition

Fixed Effects	*β*	*95% CI*	*t*-value	*p*-value
Intercept	0.00	[-0.16; 0.16]	0.00	1
Pretest	0.45	[0.29; 0.61]	5.57	<.001***
Self-control	0.04	[-0.28; 0.35]	0.25	.806
Error Estimation	-0.09	[-0.41; 0.22]	-0.59	.556
Aggregate RewP	0.01	[-0.15; 0.17]	0.14	.891
Self-control x Error Estimation	0.52	[-0.11; 1.15]	1.63	.105

Note. Self-control was coded as self-control = -0.5; yoked = 0.5. Error estimation was coded as error estimation = -0.5; traditional = 0.5.

2) Describe the key dependent variable(s) specifying how they will be measured.

Performance and learning will be measured by assessing skill accuracy (radial error; Hancock, Butler, & Fischman, 1995) on a non-dominant arm beanbag throwing task. Performance will also be indexed as constant error and will be calculated separately for x- and y-axis for all trials of the acquisition phase.

EEG time-locked to augmented feedback during acquisition will be assessed. Specifically, we will examine the RewP component of the event-related potential (ERP) waveform. The RewP will serve as the dependent variable and predictor variable in statistical models informed by reinforcement learning (RL) theory.

Key dependent variable for each research question:

- Question 1: average radial error (acquisition)
- Question 2: constant error on the current trial
- Question 3: single-trial RewP on the current trial
- Question 4: average radial error (post-test)

Divergencies

For question 2, we used absolute change in CE as the dependent variable as this measure has been used in the past to quantify changes in performance (Lee & Carnahan, 1990) and it facilitates result interpretation.

3) How many and which conditions will participants be assigned to?

Participants will be assigned to one of four conditions: (1) self-control/error estimation; (2) self-control/traditional; (3) yoked/error estimation; or (4) yoked/traditional. Participants will perform the motor skill in a pretest, acquisition phase, and a post-test (retention and transfer test), which occurs 24 h after an acquisition phase. Participants will perform the skill from the same distance in the pretest, acquisition phase, and retention test, and they will perform the skill from 1 m farther in the transfer test.

Divergencies

None.

4) Specify exactly which analyses you will conduct to examine the main question/hypothesis.

Question 1: Does the RewP predict performance during acquisition across participants?

- Model: Linear regression
- Dependent variable: average radial error (acquisition)
- Fixed effects: pretest, condition (self-control, error estimation and their interaction), and aggregate RewP

In R:

```
mod1 <- lm(avg.re ~ pretest + SC.cond*EE.cond + aggregate.rewp, DATA)
```

Question 2: What is the effect of single-trial RewP on performance adjustments during acquisition?

- Model: Mixed-effects model
- Dependent variable: constant error (current trial)

- Fixed effects: condition (self-control, error estimation and their interaction) and trial number, constant error (previous trial), dimension (x/y), single-trial RewP (previous trial), and their interactions
- Random effects: Participant, Participant x Trial

In R:

```
mod2 <- lmer(ce.c ~
            # fixed-effects
            SC.cond*EE.cond + trial*ce.p*dimension*single.trial.rewp.p +
            # random-effects
            (1 + trial | subID), DATA, REML = TRUE)
```

Divergencies

For mod2, absolute change in CE was used as the dependent variable. Also, instead of CE on the previous trial, we used absolute error on the previous trial as one of the fixed effects. During the analysis process, we adopted a more conservative approach and included random slopes of previous absolute error and previous single-trial RewP. However, the model did not converge. Thus, after inspecting the correlation and the variance accounted for by each random effect, we decided to drop the random slopes of absolute error and previous single-trial RewP.

Question 3: What is the effect of performance on the RewP?

- Model: Mixed-effects model
- Dependent variable: single-trial RewP (current trial)
- Fixed effects: condition (self-control, error estimation and their interaction) and trial, single-trial radial error (current trial), average radial error (acquisition), and their interactions

- Random effects: Participant, Participant X Trial

In R:

```
mod3 <- lmer(single.trial.rewp ~
        # fixed-effects
        SC.cond*EE.cond + trial*single.trial.re.c*avg.re +
        # random-effects
        (1 + trial | subID), DATA, REML = TRUE)
```

Divergencies

For mod3, which in chapter 4 is referred to as model 1 (question 1), we decided to adopt a more conservative and include random slopes of single-trial RewP.

Question 4: How does the RewP affect long-term learning?

- Model: Mixed-effects model
- Dependent variable: average radial error (post-test)
- Fixed effects: pretest, condition (self-control, error estimation and their interaction), type of post-test (retention/transfer), condition x type of post-test interaction, aggregate RewP and aggregate RewP x type of post-test interaction
- Random effects: Participant

In R:

```
mod4 <- lmer(posttest ~
        # fixed-effects
        pretest + SC.cond*EE.cond*posttest.type + aggregate.rewp*posttest.type
+
        # random-effects
```

(1 | subID), DATA, REML = TRUE)

Divergencies

For mod4, which in chapter 4 is referred to as model 3 (question 3), we added the interaction between pretest and post-test type to the model.

5) Outliers and Exclusions?

Participants who have <20 usable feedback trials will be excluded. Feedback trials are defined by not being marked as having an artifact (EEG processing stage) and trials that landed on the target. If influential points are identified (e.g., by Cook's Distance >1.0), sensitivity analyses will be carried out to determine the level of influence on the models and ensure the robustness of the results.

Divergencies

As explained in the text, trials that landed off board were included in the analyses.

6) How many observations will be collected or what will determine sample size? No need to justify decision, but be precise about exactly how the number will be determined.

The sample size was determined based on the behavioral hypotheses. Specifically, we powered the study for the upper-end of the effect size estimate for the benefit of self-controlled practice compared to a yoked control group (Cohen's f = .20 [$\eta^2_p \sim .038$]); personal communication from Brad McKay). We chose the upper limit because we are controlling for pretest (thus accounting for variance not explained by self-controlled practice), which is not always the case in self-control over practice condition studies. Using G*Power 3.1.9.2, we entered this effect size along with the other following input parameters for an ANOVA (fixed effects, special, main effects and interactions): α = .05, β = .20, numerator df = 1, groups = 4. This yielded a sample size =

199, which we rounded up to 200. We decided to conduct a sequential analysis with an interim analysis (N = 100) using the Pocock boundary (interim and final α = .0294). Data collection will terminate at the interim analysis under the following conditions: (1) the interaction (Self-Control x Error Estimation) is significant; (2) either/both main effect(s) is/are significant and the upper bound(s) of the CI(s)$_{95\%}$ of the non-significant effect(s) (main effect and/or interaction) is/are less than η^2_p = .038, since power to detect a smaller effect size would require N > 200, which is a greater N than we are willing to collect; or (3) both main effects and the interaction are non-significant and the upper bounds of the CIs$_{95\%}$ of the effects are less than η^2_p = .038.

Divergencies

None.

7) Anything else you would like to pre-register? (e.g., data exclusions, variables collected for exploratory purposes, any unusual analyses planned?)

Bivariate variable error (precision) will be assessed during pretest, acquisition, and post-test and may be used for exploratory purposes.

In an exploratory fashion, we may also test whether training condition moderates the reinforcement leaning-predicted relationships.

Divergencies

None.

8) Have any data been collected for this study already?

Yes. We have collected EEG and performance/learning accuracy data from approximately 110 participants. Behavior data (i.e., performance and learning accuracy) from 100 participants were used in the interim analysis, as described above. Since the pre-determined conditions to terminate data collection were not met, we decided to proceed with data collection.

Divergencies

None.

Appendix 3

Analysis of the Relationship Between Single-trial RewP and Performance Adjustments During Acquisition

Table A3

Random and Fixed Effects for the Analysis of the Effect of RewP on Performance Adjustments

Random Effects			
Effect	*Variance*	*SD*	*Corr*
Intercept	36.44	6.04	
Trial	8.59	2.93	-0.13
	888.07	29.80	

Fixed Effects				
Effects	*β*	*95% CI*	*t*-value	*p*-value
Intercept	26.56	[25.20; 27.91]	38.47	<.001***
Self-control	0.26	[-1.82; 2.54]	0.22	.825
Error Estimation	1.07	[-1.08; 3.46]	0.92	.362
Trial	-1.03	[-2.06; 0.04]	-1.97	.050*
Previous Absolute Error	0.42	[0.39; 0.44]	32.81	<.001***
Dimension	14.73	[13.13; 16.37]	16.54	<.001***
Previous Single-trial RewP	0.40	[-0.48; 1.26]	0.90	.370
Self-control x Error Estimation	-1.27	[-6.00; 3.32]	-0.54	.589
Trial x Previous Absolute Error	0.05	[0.02; 0.07]	3.76	<.001***
Trial x Dimension	-2.00	[-3.74; -0.21]	-2.20	.028*
Previous Absolute Error x Dimension	-0.11	[-0.16; -0.07]	-4.54	<.001***
Trial x Previous Single-trial RewP	0.01	[-0.94; 0.86]	0.01	.990
Previous Absolute Error x Previous Single-trial RewP	-0.01	[-0.03; 0.02]	-0.67	.502
Dimension x Previous Single-trial RewP	1.44	[-0.30; 3.28]	1.60	.109
Trial x Previous Absolute Error x Dimension	0.08	[0.03; 0.13]	3.14	.002**
Trial x Previous Absolute Error x Previous Single-trial RewP	0.00	[-0.02; 0.02]	0.01	.992
Trial x Dimension x Previous Single-trial RewP	2.02	[0.18; 3.79]	2.20	.028*
Previous Absolute Error x Dimension x Previous Single-trial RewP	-0.03	[-0.08; 0.02]	-1.20	.230
Trial x Previous Absolute Error x Dimension x Previous Single-trial RewP	-0.04	[-0.08; 0.01]	-1.47	.141

Note. Number of observations: 12488, groups: Participant, 134. All variables were z-transformed prior to the analysis except for absolute change in CE and previous absolute error. Self-control was coded as self-control = -0.5; yoked = 0.5. Error estimation was coded as error estimation = -0.5; traditional = 0.5. Dimension was coded as x-axis = -0.5; y-axis = 0.5.