Autonomy-supportive practice manipulations and skill acquisition

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Published and true are not synonyms.

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Lay Abstract

Practice environments that provide learners with autonomy have been argued to be more effective for learning new motor skills compared to more controlling environments. Two techniques that can be used to create autonomy-supportive learning environments are giving learners control over a feature of their practice or the language used when giving task instructions. This dissertation addresses knowledge gaps and several methodological limitations of previous literature by measuring key psychological variables, the use of novel experimental groups, large *N* studies, modern statistical techniques, and open science practices. Findings showed that under many conditions perceptions of autonomy and competence can be impacted positively; however, these psychological benefits do not reliably translate into superior motor performance or learning. Collectively, results of this dissertation challenge mainstream perspectives regarding a direct and causal role of motivational influences on motor skill acquisition.

Abstract

There has been growing interest in the role of motivation in motor learning, and specifically how autonomy, competence, and intrinsic motivation may directly benefit the skill acquisition process. Within the autonomy branch of the motivation pillar in OPTIMAL theory, supporting a learner's basic psychological need for autonomy contributes to a virtuous cycle that enhances expectancies for success (i.e., perceptions of competence) and in turn facilitates motor performance and learning. Although many experiments have concluded support for OPTIMAL theory, these studies have often relied on small sample sizes, have not been pre-registered, and have consistently failed to include appropriate measures that assess key predictions in the theory. The purpose of this dissertation was to address these methodological limitations and test core predictions in the OPTIMAL theory regarding the direct and causal role of autonomy-supportive practice conditions—control over practice and instructional language—on motor performance and learning.

Experiments 1 and 2 (Chapter 2) critically tested between the information-processing and motivation-based (i.e., OPTIMAL theory) explanations of the self-controlled learning advantage by providing participants in choice and yoked groups with error or graded feedback (Experiment 1) and binary feedback (Experiment 2). Results showed no self-controlled learning advantage and exercising choice in practice did not increase perceptions of autonomy, competence, or intrinsic motivation, nor did it improve error estimation accuracy. Although these findings are difficult to reconcile with either explanation, they are consistent with a growing body of evidence suggesting self-controlled conditions are not advantageous for motor learning.

Experiment 3 addressed a methodological limitation of past self-controlled learning research by including a novel yoked group that was explicitly told they were being denied choice and that their observation schedule was created by another participant. Results showed no self-controlled learning advantage despite finding higher perceptions of autonomy in the choice group. These findings are consistent with Experiments 1 and 2, and further questions the causal role of autonomy-support on motor learning and the robustness of the so-called self-controlled learning advantage.

Experiment 4 investigated the influence of different instructional language styles on skill acquisition. Throughout practice participants received task instructions that used either autonomy-supportive or controlling language. Results showed no performance differences in acquisition or retention despite finding higher perceptions of autonomy and competence in the autonomy-supportive group. These findings are inconsistent with key predictions in OPTIMAL theory regarding the role of autonomy in motor learning.

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Chapter 1

Introduction

Whether it's a child learning to throw a ball to play catch, an adult re-learning to use a fork after having a stroke, or Simone Biles doing a double layout half twist on floor, motor skill acquisition is a fundamental aspect of life. Motor skill underlies many attributes that allow us to exist and interact with our environments as humans; for example locomotion, communication, or skilled athletic performance, to name a few. Guthrie (1952) defined skill as "the ability to bring about some end result with maximum certainty and minimum outlay of energy, or of time and energy" (p. 136). This definition captures the idea that skilled performance is goal-directed and is to be achieved with minimal costs. Skilled performance allows us to perform *motor skills* (i.e., actions), which here are operationally defined as tasks that must be learned (or re-learned) and require voluntary control of movements to obtain a goal (Magill & Anderson, 2021).

In everyday language, we often use the terms skill, actions, movements, and/or abilities interchangeably. In motor learning research, however, there are subtle yet important differences between these terms that are worth highlighting. *Movements* refer to the specific patterns of motion between joints and body segments (Magill & Anderson, 2021), and are the component parts of a skill which are observable and measurable. That is, motor skills are composed of movements, but a near infinite number of movements can be used to obtain the same goal of a specific motor skill (Latash, 2018; Todorov & Jordan, 2002; Wolpert, Diedrichsen, & Flanagan, 2011). Motor skills are also supported by *motor abilities*, which are relatively stable and enduring traits that serve as a determinant of a person's achievement potential related to the performance of a motor skill (Magill & Anderson, 2021). Examples of motor abilities include reaction time, multi-limb coordination, and dexterity. Let's consider these definitions with an example of a person shooting a basketball. This is a motor skill given it must be learned and the goal is to get the ball into the hoop. Various movements can achieve this task goal, including shooting with your arms above your head or an underhand throw, and both can be completed with various combinations of force production or release points. Motor abilities such as the coordination between the limbs and visual system, and dexterity to manipulate the ball will allow these movements to be undertaken. Finally, skilled performance would allow the person to consistently achieve the task goal and have very few missed shots—all while requiring minimal amounts of neuromotor or cognitive efforts. The way in which this player went about acquiring (or relearning) this motor skill can be captured by a process referred to as motor learning.

1.1 Motor learning

Motor learning is an umbrella term that encompasses various processes which lead to changes in performance. For example, how is a tennis player able to maintain a high level of skill on a serve regardless of the type of court they play on? Or how does a dancer go from doing bounces and wiggles in baby ballet to eloquently performing a *pas de deux* in Swan Lake? There are many ways in which motor learning has been defined, with commonality between definitions including how practice and/or experience affects the completion of a motor skill (Krakauer, Hadjiosif, Xu, Wong, & Haith, 2019; Magill & Anderson, 2021; Schmidt, Lee, Winstein, Wulf, & Zelaznik, 2018). To capture the multifaceted nature of *motor learning*, Krakauer and colleagues (2019) used a two-part categorization that I adopted for my dissertation.

1.1.1 Skill maintenance

Skill maintenance is operationally defined as the process of "maintain[ing] performance levels of existing skills under changing conditions" (Krakauer et al., 2019, p. 615). For example, maintaining a beautiful gymnastics performance at the end of a fatiguing routine, achieving a low score as wind conditions change on a golf course, or walking successfully on a variety of surfaces such as an icy sidewalk or a sandy beach. Common ways to study skill maintenance include imposing visuomotor perturbations while participants perform a well-learned motor skill such as target-directed reaching (e.g., Coltman, Cashaback, & Gribble, 2019; Mazzoni & Krakauer, 2006; McDougle, Ivry, & Taylor, 2016), saccadic eye movements (e.g., Alahyane et al., 2007; McLaughlin, 1967), or treadmill walking (e.g., Malone, Vasudevan, & Bastian, 2011; Morton & Bastian, 2006). In visuomotor rotation experiments, for instance, visual feedback of a cursor representing the participants unseen hand location in space is rotated by some amount (e.g., 30°) in a clockwise or counter clockwise direction (for a review see Shadmehr, Smith, & Krakauer, 2010). Participants must learn to alter their movements to counter this perturbation to maintain their previous performance level of the cursor hitting the target. Studying motor learning from a skill maintenance perspective often involves relatively simple skills and has primarily been studied using shorter timescales (i.e., immediate retention through aftereffects Mazzoni & Krakauer, 2006; although longer-term aftereffects have been examined Fernández-Ruiz & Díaz, 1999). This skill maintenance category of motor learning is not the focus of my dissertation.

1.1.2 Skill acquisition

Skill acquisition can be operationally defined as the "processes by which an individual acquires the [capability] to rapidly identify an appropriate movement goal given a particular task context, select the correct action given a sensory stimulus and/or the current state of the body and the world, and execute that action with accuracy and precision" (Krakauer et al., 2019, p. 615). A common way to study skill acquisition is to have individuals perform a new skill and measure how their performance changes over time and the relative permanence and/or generalizability of these changes. Of interest in this area is examining various practice conditions (e.g., augmented feedback, observational learning, self-controlled learning, instructional language) to identify the practice factors that enhance learning, hinder learning, or have no effect at all (see Adams, 1987; Salmoni, Schmidt, & Walter, 1984; Schmidt & Bjork, 1992; Sigrist, Rauter, Riener, & Wolf, 2013; Wulf, Shea, & Lewthwaite, 2010 for respective reviews). As the skill acquisition category of motor learning was the focus of my dissertation, a discussion of the typical experimental approach and the time-dependent processes that support this type of learning is warranted.

A typical skill acquisition experiment often includes at least three phases (see Figure 1.1). During the pre-test, participants complete a relatively low number of trials to capture baseline performance levels. Following the pre-test, participants complete the acquisition (or training) phase that involves more trials than the pre-test. Here, participants are separated into at least two groups and experience different levels of the practice variable being studied (for example, practicing multiple skills in a blocked/fixed order versus in a random/interleaved order). Following an interval of time with no practice, participants then complete retention and/or transfer tests to assess learning. Although this period of no practice varies from experiment to experiment, a general rule of thumb is that it should be at least 24 hours to allow any transient/temporary effects to dissipate (see Kantak & Winstein, 2012; Schmidt & Bjork, 1992; Soderstrom & Bjork, 2015 for reviews). Retention tests assess the relative permanence or persistence

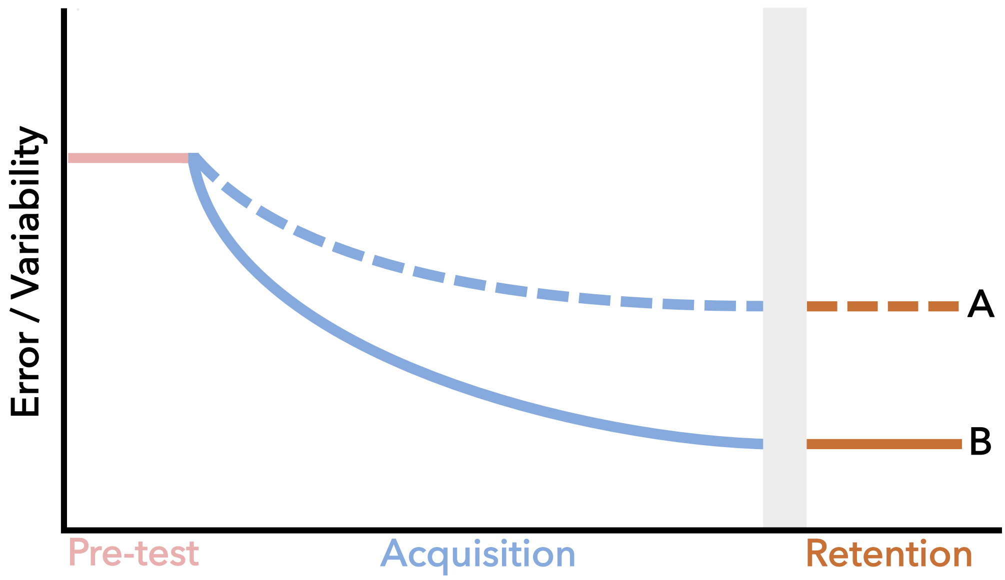

Figure 1.1. Overview of a typical skill acquisition experiment with hypothetical data. The pre-test (pink) consists of a relatively low number of trials to assess baseline performance level. The acquisition phase (blue) consists of many more trials than the pre-test with participants practicing the novel motor skill in one of two groups. (Although more than two groups is also possible.) The groups will differ based on some level of the manipulated practice variable of interest (e.g., skilled model versus novice model versus no model). After an interval without practice (shaded grey band), all participants complete a delayed retention test (orange). This test consists of a relatively low number of trials and the manipulation from acquisition is no longer present. Motor learning is inferred from performance in the delayed retention test based on the difference in performance from pre-test to retention. Learning differences between groups are assessed by comparing performance on the retention test. In this example, it can be seen that both groups improved their performance from pre-test to retention and during the acquistion phase. However, Group B (solid line) has less performance error or variability in retention than Group A (dashed line), suggesting superior motor learning. Adapted from Sternad (2018).

of the skill, whereas transfer tests assess the adaptability or generalizability of the skill. Using observational learning as an example, during the pre-, retention, and transfer tests all participants would not observe a model. In acquisition, however, one group might watch a skilled model and the other group might watch a novice model. This example highlights that the pre-test, retention test, and transfer test are all performed under a common level of the practice variable and that the manipulation is only present during acquisition.

There are a variety of processes that are thought to occur during the different phases of a motor learning experiment, which are captured in the motor behavior-memory framework (Kantak & Winstein, 2012). The first is the *encoding* of a motor memory, which is thought to occur during the acquisition phase. Here, the learner makes associations between the task goal, movements, and movement outcomes by processing information and using feedback to develop an error detection and correction mechanism. In the delay between acquisition and retention (i.e., the retention interval), motor memory *consolidation* occurs where the motor memory formed during encoding is strengthened with the passage of time. Evidence of off-line gains in performance from motor memory consolidation comes from participants having improved performance after a period of no practice, either on a retention test or a second day of practice (e.g., E. M. Robertson & Cohen, 2006). The final stage of this framework, *retrieval*, is related to the retention test (or some other learning test) where the participants must retrieve the motor memory encoded and consolidated during acquisition and the retention interval, respectively. According to Kantak & Winstein (2012), retrieval not only reveals the effectiveness of the encoding and consolidation processes, but is the only possible measure of learning and memory.

1.1.3 Performance-learning distinction

In any motor learning experiment, an important distinction is between performance and learning (Kantak & Winstein, 2012; Magill & Anderson, 2021; Schmidt & Bjork, 1992; Schmidt et al., 2018; Soderstrom & Bjork, 2015). Performance is observable behaviour and may be transiently influenced by many factors. For example, when a coach is teaching a gymnast how to do a cartwheel, they are able to observe the way the gymnast is performing the skill from their takeoff position, the way they place their hands on the floor, and how they move their legs overhead. Performance levels can be transient as they can be impacted by fatigue, motivation, and/or feedback. Learning, on the other hand, is not directly observable, and must therefore be inferred from performance. To capture this distinction, Magill & Anderson (2021) defined *motor learning* as a change in the capability of a person to perform a skill that must be inferred from a relatively permanent improvement in performance as a result of practice or experience. This underscores the importance of retention and/or transfer tests following a period of no practice in motor learning experiments as a technique to separate transient or temporary effects from those that are relatively permanent.

The need for delayed tests to examine the potential paradoxical effects between performance and learning were highlighted in a seminal paper by Salmoni and colleagues (1984). That is, some conditions of practice which have immediate performance benefits during acquisition may not carry over to longer-term retention, or may actually deteriorate retention (Kantak & Winstein, 2012). Some classic examples that are often used to highlight this performance-learning paradox are high frequencies of knowledge of results feedback (e.g., Salmoni et al., 1984), constant versus variable practice (e.g., Schmidt, 1975), and random versus blocked practice (e.g., Lee & Magill, 1983; Shea & Morgan, 1979). In the absence of delayed retention and/or transfer tests, researchers and practitioners may fall victim to this performance-learning paradox and draw inappropriate conclusions about the effectiveness of a practice condition for motor learning. Interestingly, learners are also susceptible to this performance-learning paradox as they will interpret current performance levels as a valid index of learning (e.g., Carter, Smith, & Ste-Marie, 2016; Simon & Bjork, 2001; Simon & Bjork, 2002).

1.2 Motor learning theories

1.2.1 Information-processing perspectives

Humans have been likened to computers such that we receive some input from our body and/or environment, process this information, then produce some motor output based on some decision from this information. For example, during a reaction time task an individual must perceive the presented stimulus, prepare a response, and execute their planned action. Inspired by the development of information theory (Shannon, 1948; Shannon & Weaver, 1964), pioneers of motor behaviour often interpreted their findings with this computer metaphor in mind, including Hick's Law (Hick, 1952; see also Hyman, 1953), stimulus-response compatibility effects (Fitts & Seeger, 1953), Fitts' Law (Fitts, 1954), and response complexity effects in the "memory drum" theory (Henry & Rogers, 1960). This information processing perspective has been the dominant view in the skill acquisition motor learning literature and was central to early theories of motor learning.

Adams (1971) proposed the Closed Loop Theory of Motor Learning where he conceptualized motor learning as a problem-solving process to which knowledge of results[1] feedback was considered an essential solution. Adams' theory consisted of two distinct constructs: the memory trace and the perceptual trace. The memory trace would select and initiate a response from an internally stored repertoire of actions. Once the movement had been initiated, the perceptual trace governed action execution and served as a reference of correctness from past actions. Once knowledge of results had been received, the learner could compare it against the perceptual trace, detect differences (i.e., errors) between the intended and actual movement outcome, and correct those errors on the next attempt. Adams proposed that knowledge of results was essential

[1]Knowledge of results is a category of augmented feedback that provides information about a response outcome relative to the task goal.

on every trial—at least until a skill was well-learned—to strengthen the perceptual and memory traces to support motor learning as a problem-solving process.

Shortly after Adams' (1971) theory, Schmidt (1975) published his Schema Theory of Motor Learning to address two key problems in Closed Loop Theory: the storage and novelty problems. The storage problem was concerned with how the central nervous system could possibly hold a near infinite number of motor programs and feedback states simultaneously. The novelty problem pertained to how someone could possibly perform a task not yet experienced or stored by the central nervous system. Schmidt addressed these issues with three key components in his theory: a generalized motor program, the recall schema, and the recognition schema. Rather than a unique motor program for every possible task and its variations (e.g., Keele, 1968), Schmidt proposed there was a generalized motor program for each *class of movements* that could be altered by parameters to achieve a task goal. The recall schema was responsible for movement initiation and production based on the initial conditions of the task, outcomes of past similar movements, and specific task requirements. Finally, the recognition schema was used to assess the movement against expected outcomes based on sensory consequences and actual movement outcomes as a way to evaluate relative success of the action. Similar to Closed Loop theory, knowledge of results played an essential role as this feedback could be used to update the generalized motor program and schemata for future attempts of the task. Although both Adams' (1971) and Schmidt's (1975) theories were highly influential and motivated decades of research, both overemphasized the importance of receiving knowledge of results feedback after all trials to facilitate learning (for a discussion see Salmoni et al., 1984).

Salmoni et al. (1984) reviewed the knowledge of results and motor learning literature and noted that researchers were drawing conclusions about feedback effectiveness based on practice performance rather than relying on retention and/or transfer designs. This methodological limitation resulted in the incorrect view that knowledge of results after every trial was most effective for learning (e.g., Bilodeau & Bilodeau, 1958; Bilodeau, Bilodeau, & Schumsky, 1959). In this highly influential paper, Salmoni and colleagues (1984) proposed the Guidance hypothesis to account for the temporary or transient effects of knowledge of results feedback, as well as its relatively permanent effects. Here, knowledge of results was important for guiding the learner to the correct response; however, when provided too frequently it had a "crutch-like" effect as learners would ignore their intrinsic feedback in favour of the more precise knowledge of results information.[2] Consequently, performance would suffer when knowledge of results was no longer available. Following the proposal of the Guidance hypothesis, several knowledge of results scheduling techniques were investigated and evaluated using no-feedback retention and/or transfer designs. This research revealed that the most beneficial techniques were schedules that would prevent a dependence on the provision of knowledge of results while encouraging individuals to learn to use their intrinsic feedback. Some examples of knowledge of results scheduling techniques that have been interpreted as support for the Guidance hypothesis include a reduced relative frequency of feedback (e.g., Winstein & Schmidt, 1990), summary feedback (e.g., Schmidt, Lange, & Young, 1990; Schmidt, Young, Swinnen, & Shapiro, 1989), and error estimation (e.g., Guadagnoli & Kohl, 2001).

Information processing accounts of motor learning have been highly successful over the years—not only for classic manipulations such as variability of practice (e.g., Shoenfelt, Snyder, Maue, McDowell, & Woolard, 2002; Wulf & Schmidt, 1997), contextual interference effects (e.g., Lee & Magill, 1983; Shea & Morgan, 1979), and the scheduling of knowledge of results (e.g., Winstein & Schmidt, 1990; Wulf & Schmidt, 1989), but also for more contemporary manipulations such as observational learning (e.g., R. Robertson, St. Germain, & Ste-Marie, 2018), self-controlled practice conditions (e.g., Carter, Carlsen, & Ste-Marie, 2014), and expecting to teach (e.g., Daou, Lohse, & Miller, 2016). Yet for some other contemporary practice conditions such as conceptions of ability (e.g., Wulf & Lewthwaite, 2009), social-comparative feedback (e.g., Lewthwaite & Wulf, 2010b), incidental choices (e.g., Lewthwaite, Chiviacowsky, Drews, & Wulf, 2015), and instructional language (e.g., Hooyman, Wulf, & Lewthwaite, 2014), this information processing perspective has been argued to be unsatisfactory (see Lewthwaite & Wulf, 2010a; Wulf et al., 2010 for discussions).

1.2.2 Motivation perspective

Wulf & Lewthwaite (2016) argued that theories that likened humans to computers were inadequate to capture the breadth of human behaviour (for an alternative discussion of this same issue see Cisek, 1999). Based predominantly on findings from nearly a decade of their skill acquisition research, Wulf & Lewthwaite (2016) published their Optimizing Performance Through Intrinsic Motivation and Attention for Learning theory (hereafter referred to as OPTIMAL theory). OPTIMAL theory was heavily influenced by Self-Determination theory (Ryan & Deci, 2000a) and consists of an attention pillar and a

[2] In addition to this informational or guidance role of knowledge of results, Salmoni et al. (1984) also acknowledged that knowledge of results can serve a motivational role and an associational function.

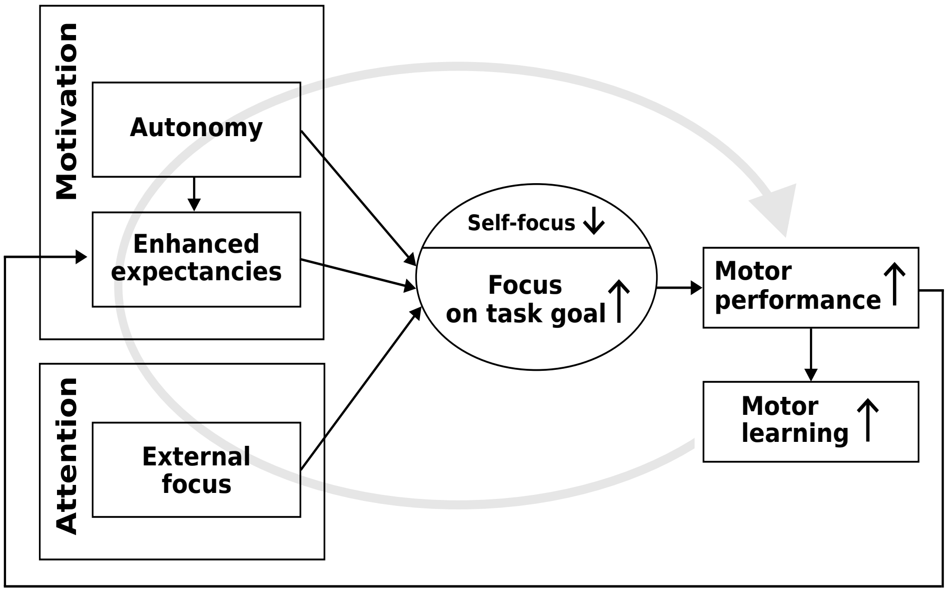

Figure 1.2. Overview of OPTIMAL theory. Conditions of practice which include an external focus of attention, enhanced expectancies for future performance, and autonomy-support promote goal-action coupling to increase focus on the task goal, decrease focus on the self, and improve motor performance. This creates a virtuous cycle (grey arrow) wherein continual improvements in motor performance will further enhance expectancies for future success and facilitate motor learning. Taken from Wulf & Lewthwaite (2016).

motivation pillar (see Figure 1.2). In the attention pillar, a performer's *focus of attention* can be directed either internally (e.g., a body part involved in action) or externally (e.g., a tool involved in the action). The motivation pillar consists of *autonomy*, wherein perceptions of autonomy can be supported or thwarted, and *enhanced expectancies*, wherein perceptions of future success can be increased or decreased. According to Wulf & Lewthwaite (2016), a virtuous cycle is created when one or more of these factors are positively impacted (i.e., using an external focus of attention, enhancing expectancies, and/or supporting autonomy), which improves goal-action coupling to enhance intrinsic motivation and in turn both motor performance and learning. The autonomy branch of the motivation pillar is the focus of my dissertation; however, for completeness a brief overview of attentional focus and enhanced expectancies is provided below.

Attentional focus

Focus of attention refers to the information a performer's attention is mentally directed towards (Wulf, 2007; Wulf, Höß, & Prinz, 1998) and can be either internal or external. When a performer adopts an internal focus of attention they are attending to the movements of their body and when they adopt an external focus of attention they attend to the intended movement effects. It has generally been accepted that an external focus of attention is beneficial for motor performance and learning compared to an internal focus of attention (for a review see Wulf, 2013). For example, instructing learners to focus on the wheels of a ski simulator (Wulf et al., 1998, Experiment 1) or the markers on a balance platform (Wulf et al., 1998, Experiment 2) enhanced motor learning compared to focusing on one's feet. The benefits of an external focus is considered a robust motor learning phenomenon and has also been demonstrated in children (e.g., Chiviacowsky, Wulf, & Ávila, 2013), older adults (e.g., Chiviacowsky, Wulf, & Wally, 2010), and clinical populations (e.g., Landers, Wulf, Wallmann, & Guadagnoli, 2005), with discrete (e.g., An, Wulf, & Kim, 2013; McKay & Wulf, 2012) and continuous (e.g., Wulf et al., 1998) tasks, and has resulted in more efficient movements (e.g., Lohse, Sherwood, & Healy, 2011; Zachry, Wulf, Mercer, & Bezodis, 2005).

Enhanced expectancies

The expectations a learner holds about future success are impacted by previous experiences (Wulf & Lewthwaite, 2016). Within OPTIMAL theory, expectancies for future success can either be lowered or enhanced. Situations that enhance expectancies for future performance (i.e., perceptions of competence) are beneficial for motor performance and learning, for example providing feedback after relatively good versus relatively poor trials (e.g., Chiviacowsky & Wulf, 2007), perceptions of task difficulty (e.g., Palmer, Chiviacowsky, & Wulf, 2016), positive social comparative feedback (e.g., Chiviacowsky et al., 2010), and feedforward self-modeling (e.g., Clark & Ste-Marie, 2007). Similar to an external focus, the benefits of enhanced expectancies are also considered a robust effect as it has been shown to improve motor performance and/or learning in adults (e.g., Palmer et al., 2016) and children (e.g., Bahmani, Wulf, Ghadiri, Karimi, & Lewthwaite, 2017), with discrete (e.g., Ávila, Chiviacowsky, Wulf, & Lewthwaite, 2012) and continuous (e.g., Lewthwaite & Wulf, 2010b) tasks, as well as lab-based (e.g., Chiviacowsky & Drews, 2016) and sports-based (e.g., Harter, Cardozo, & Chiviacowsky, 2019) tasks.

Autonomy

From Self-Determination Theory, autonomy is operationally defined as the sense of ownership and initiative over one's behaviours (Ryan & Deci, 2020). Self-Determination Theory is a comprehensive framework, consisting of six mini-theories,[3] that can be used to study human behaviour with an emphasis on motivation and personality (Ryan & Deci, 2007). Within Self-Determination Theory, and more specifically Basic Psychological Needs Theory (Ryan & Deci, 2000b, 2017), conditions that support an individual's experience of autonomy, competence, and/or relatedness increase intrinsic motivation and result in a range of positive outcomes, including enhanced performance, persistence, and creativity. Building on this, Wulf & Lewthwaite (2016) argued that autonomy-supportive practice conditions will improve motor performance by enhancing expectancies and led to superior motor learning by making dopamine available for memory consolidation and neural pathway development.

Wulf & Lewthwaite (2016) outlined three practice variables that researchers and practitioners can manipulate to support (or thwart) perceptions of autonomy to facilitate (or hinder) motor performance and learning. The three practice variables are: 1) control over practice conditions, 2) incidental choices, and 3) instructional language. Support for the effectiveness of giving learners control over a feature of their practice condition, or task-relevant choices, for motor learning include their observation schedule (e.g., Ste-Marie, Vertes, Law, & Rymal, 2013; Wulf, Raupach, & Pfeiffer, 2005), the use of assistive devices (e.g., Hartman, 2007; Wulf & Toole, 1999), and when to receive augmented feedback (e.g., Carter et al., 2014; Chen, Hendrick, & Lidor, 2002; Janelle, Barba, Frehlich, Tennant, & Cauraugh, 1997; Patterson & Carter, 2010). Evidence for the benefits of incidental, or task-irrelevant choices, for motor learning include choosing the colour of the golf ball to putt (e.g., Lewthwaite et al., 2015, Experiment 1; Wulf, Chiviacowsky, & Cardozo, 2014), the colour of the mat under the target during a lasso task (e.g., Wulf, Iwatsuki, et al., 2018, Experiment 1), which of two pictures to hang in a laboratory (e.g., Lewthwaite et al., 2015, Experiment 2), and the order in which exercises are performed (e.g., Wulf, Freitas, & Tandy, 2014). Compared to control over practice conditions and incidental choices, instructional language has received considerably less attention in the motor learning literature. However, there is some evidence suggesting that motor performance and learning is enhanced when participants receive instructions

[3]The various mini theories were developed to explain phenomena that emerged from laboratory and applied research, with each focused on a component of motivation or personality functioning (https://selfdeterminationtheory.org/theory/). The six mini theories are: 1) Cognitive Evaluation Theory, 2) Organismic Integration Theory, 3) Causality Orientations Theory, 4) Basic Psychological Needs Theory, 5) Goal Contents Theory, and 6) Relationships Motivation Theory.

using autonomy-supportive language compared to controlling (or neutral) language (e.g., Arsham, Sarabandi, & Ghanaatian, 2021; Hooyman et al., 2014).

The focus of my dissertation is the autonomy branch of the motivation pillar in OPTIMAL theory (Wulf & Lewthwaite, 2016), with an emphasis on the practice variables of *control over practice conditions* (i.e., self-controlled learning) and *instructional language*. Incidental choices were not included given the mixed support for their effectiveness to facilitate learning (e.g., Grand, Daou, Lohse, & Miller, 2017; McKay & Ste-Marie, 2020) and more importantly, that task-relevant choices were shown to be more effective than incidental (i.e., task-irrelevant) choices (Carter & Ste-Marie, 2017b; but see Wulf, Iwatsuki, et al., 2018 Experiment 2). An overview of the relevant control over practice conditions and instructional language research that motivated the experiments in my dissertation is provided in the following two sections.

1.3 Control over practice conditions

In many real-world settings, decisions about how long to practice for, which skills should be practiced, and when to provide feedback are often made by a coach or practitioner. For example, a coach might give an athlete a correction after they made an error to improve their performance, and a physiotherapist might praise a client when they successfully complete a skill to provide encouragement. There are many situations, however, when a coach or practitioner may not be immediately available to provide feedback. Consider a large team where the coach cannot watch every athlete at once. Individual athletes may seek out and request feedback on a given attempt of the skill. This control over an aspect of their practice environment has been a popular topic in the motor learning literature over the last few decades. While this manipulation has received various names in the literature (e.g., self- and/or learner-regulated, -directed, -selected, or autonomy-supportive), for the purpose of this dissertation it will be referred to as *self-controlled learning*.

A typical self-controlled learning experiment includes at least two groups. The first is the self-controlled group, where participants are provided control or choice over some aspect of their practice environment. The second group is the yoked group, where participants experience the same practice schedule created by a participant in the self-controlled group, but without the same choice opportunity. Using knowledge of results feedback as an example, if a participant in the self-controlled group requested feedback after trials 1, 3, 6, and 9 in a block of 10 trials, their yoked counterpart will also receive feedback after trials 1, 3, 6, and 9 for that block of 10 trials. This ensures that each self-controlled and yoked pairing not only receive the same total amount of feedback, but also the same relative placement of the feedback trials throughout acquisition. Therefore, any performance and/or learning differences observed between the groups can be attributed to exercising or not exercising choice over a practice variable such as feedback. The typical finding in this research is that participants in the self-controlled group outperform those in the yoked group on delayed retention and/or transfer tests (see Sanli, Patterson, Bray, & Lee, 2013; Ste-Marie, Carter, & Yantha, 2020; Wulf, 2007 for reviews).

A self-controlled learning advantage has been found across a variety of conditions of practice, for example when to receive augmented feedback (e.g., Janelle et al., 1997; Patterson & Carter, 2010), frequency of model demonstration (e.g., Wulf et al., 2005), use of assistive devices (e.g., Wulf & Toole, 1999), and the order in which to practice tasks (e.g., Wu & Magill, 2011). The self-controlled learning advantage is widely viewed as a robust phenomenon because it has been shown in young adults (e.g., Chiviacowsky & Wulf, 2005; Janelle, Kim, & Singer, 1995), older adults (e.g., Lessa & Chiviacowsky, 2015), children (e.g., Chiviacowsky, Wulf, de Medeiros, Kaefer, & Tani, 2008; Ste-Marie et al., 2013), and clinical populations (e.g., Chiviacowsky, Wulf, & Lewthwaite, 2012; Chiviacowsky, Wulf, Machado, & Rydberg, 2012), with lab-based (e.g., Carter et al., 2014; Chiviacowsky & Wulf, 2002) and sports-based (e.g., Lemos, Wulf, Lewthwaite, & Chiviacowsky, 2017; Marques & Corrêa, 2016) tasks, and with continuous (e.g., Wulf & Toole, 1999), discrete (e.g., Carter, Rathwell, & Ste-Marie, 2016), and serial (e.g., Chiviacowsky & Wulf, 2002; Patterson & Carter, 2010) tasks. To account for this self-controlled learning advantage, researchers have typically adopted either an information-processing explanation or a motivation-based explanation.

1.3.1 Explanations for the self-controlled learning advantage

Researchers that adopt the information-processing explanation of the self-controlled learning advantage have argued that having choice opportunities allows the learner to tailor practice to their individual needs (Chiviacowsky & Wulf, 2002, 2005) by engaging in performance-contingent strategies (Carter et al., 2014; Carter, Rathwell, et al., 2016; Laughlin et al., 2015; Pathania, Leiker, Euler, Miller, & Lohse, 2019) to reduce uncertainty about movement outcomes (Barros, Yantha, Carter, Hussien, & Ste-Marie, 2019; Carter et al., 2014; Carter & Ste-Marie, 2017b, 2017a; Grand et al., 2015). In other words, it is thought that participants in self-controlled groups engage in deeper and/or effortful processing activities (e.g., planning, evaluation) compared to those in the yoked group (Barros et al., 2019; Carter & Patterson, 2012; Janelle et al., 1995). Support for this perspective has come from more accurate error estimation scores in self-controlled groups compared to yoked groups (Carter et al., 2014; Carter & Patterson, 2012), the finding that self-controlled feedback schedules are effective if the feedback decision is made after rather than before a trial (Carter et al., 2014; Chiviacowsky & Wulf, 2005), that the typical self-controlled learning advantage can be eliminated when key information-processing intervals are disrupted with a secondary task (Carter & Ste-Marie, 2017a; Couvillion, Bass, & Fairbrother, 2020; Woodard & Fairbrother, 2020), and that task-relevant choices are more effective than task-irrelevant choices (Carter & Ste-Marie, 2017b; c.f. Wulf, Iwatsuki, et al., 2018, Experiment 2).

Proponents of the motivation-based explanation of the self-controlled learning advantage have argued that providing learners choice opportunities during practice satisfies a basic psychological need for autonomy (Chiviacowsky, 2014; Lewthwaite et al., 2015), enhances expectancies for future success (Chiviacowsky, 2014; Janelle et al., 1995), and increases motivation to improve performance and learning (Lewthwaite et al., 2015; Wulf & Lewthwaite, 2016; Wulf et al., 2010). Persuasive evidence for the motivational explanation is that motor learning benefits have been found when participants exercise choice over something that is irrelevant to task success. For instance, performance and/or learning benefits have been found when participants were allowed to choose the colour of golf balls to putt (Lewthwaite et al., 2015, Experiment 1; Wulf, Chiviacowsky, et al., 2014) or mat to place under a target (Wulf, Iwatsuki, et al., 2018, Experiment 1), which of two paintings should be hung in a lab (Lewthwaite et al., 2015, Experiment 2), the order in which exercises are performed (Wulf, Freitas, et al., 2014) or which hand is used in a maximal force production task (Iwatsuki, Abdollahipour, Psotta, Lewthwaite, & Wulf, 2017), and which photos to look at while running (Iwatsuki, Navalta, & Wulf, 2018). It is thought that these incidental choices can isolate the motivational nature of having choice as information about task success is absent from such choices (Wulf & Lewthwaite, 2016). Lastly, the consistent finding that participants self-report a preference for requesting feedback after perceived good trials (Chiviacowsky & Wulf, 2002; Fairbrother, Laughlin, & Nguyen, 2012; Patterson & Carter, 2010; Patterson, Carter, & Sanli, 2011) has been interpreted as additional support for the motivational explanation as this is thought to protect or enhance perceptions of competence (Chiviacowsky & Wulf, 2002; Wulf & Lewthwaite, 2016).

Although self-controlled learning experiments have been a popular research area for more than two decades, there have been some consistent methodological limitations in experiments testing the information-processing and/or motivation-based explanations. That is, many of these experiments do not include measures that capture the underlying mechanisms proposed in the explanations. For instance, measures related to the information-processing explanation such as error estimation are often missing from experiments concluding it is an important skill supporting the self-controlled learning advantage (e.g., Carter & Ste-Marie, 2017a; Chiviacowsky & Wulf, 2005; Couvillion et al., 2020; Woodard & Fairbrother, 2020). A notable limitation of the experiments that have included error estimation as an outcome variable is a failure to include any baseline assessments of error estimation. Thus, it remains unclear whether self-controlled learning improves error estimation abilities or whether the previously reported differences in retention or transfer tests have resulted from a failure of randomization. In terms of the motivational explanation, the notion that being given some kind of choice opportunity is autonomy-supportive has been primarily assumed (e.g., Abdollahipour, Palomo Nieto, Psotta, & Wulf, 2017; Chua, Wulf, & Lewthwaite, 2018; Lewthwaite et al., 2015; Wulf, Freitas, et al., 2014; Wulf, Lewthwaite, Cardozo, & Chiviacowsky, 2018) or psychological variables that are measured (e.g., positive affect in Wulf, Iwatsuki, et al., 2018) do not map onto key tenets of OPTIMAL theory (Wulf & Lewthwaite, 2016).

Although there is no consensus about which of these explanations is correct, it is important to note that these explanations are not necessarily mutually exclusive. Indeed, some researchers have suggested that the motivational benefits of choice precede any information-processing benefits (Chiviacowsky, 2014), while others have suggested that information-processing benefits have a greater relative contribution than any motivational influences (Carter et al., 2014). Given the debate around these explanations and the aforementioned methodological limitations, further investigation is required to critically test predictions from the information-processing and motivation-based (i.e., OPTIMAL theory) explanations to better understand why self-controlled practice conditions are a advantageous for motor learning. This is addressed in the experiments described in Chapters 2 and 3.

1.4 Instructional language

In the Self-Determination Theory (Ryan & Deci, 2000a) literature, autonomy-supportive instructional language has received considerable attention and support for its effectiveness with perceptions of autonomy (e.g., Reeve & Tseng, 2011), positive health outcomes (e.g., Mossman, Slemp, Lewis, Colla, & O'Halloran, 2022), in higher education settings (e.g., Okada, 2021), and classroom climate (e.g., Cheon, Reeve, Marsh, &

Song, 2022) to name a few. In contrast, autonomy-supportive instructional language has received minimal attention in the motor learning literature despite being identified in OPTIMAL theory (Wulf & Lewthwaite, 2016) as one of the three ways to facilitate motor performance and learning in the autonomy branch of the motivation pillar.

Hooyman and colleagues (2014) investigated the impact of different instructional language on motor performance and learning. Participants were tasked with learning a modified cricket bowling action towards a target in one of three experimental groups: 1) autonomy-supportive instructional language, 2) controlling instructional language, and 3) neutral instructional language. Perceived choice, self-efficacy, and positive affect were assessed at two timepoints in the experiment. In the 24-hr delayed retention test, participants in the autonomy-support group had greater accuracy than the controlling group; however, neither of these groups differed from the neutral instructions group. At the end of acquisition, the autonomy-support group reported the highest perceptions of autonomy and higher self-efficacy and positive affect than the controlling language group. There were no differences in psychological constructs between any groups before retention. The authors concluded that autonomy-supportive instructional language enhanced motor learning by increasing self-efficacy, greater movement automaticity, reduced the attentional demands required to control negative consequences of having autonomy thwarted, and enhanced expectancies for future performance to improve memory consolidation.

Although the results of Hooyman et al. (2014) are consistent with the vast autonomy-support Self-Determination Theory literature (see Reeve, 2009; Ryan & Deci, 2020 for reviews; see Mossman et al., 2022; Okada, 2021; and Su & Reeve, 2011 for meta-analyses), there were some methodological limitations in the experiment that could impact the reliability of their conclusions. First, only the autonomy-supportive instructional language group received an analogy in their instructions, which have been shown to benefit motor learning (see Masters, van Duijn, & Uiga, 2020 for a review). Second, the authors relied on a relatively small sample size (n = 16 per group), which has been identified as an issue in motor learning research (Lohse, Buchanan, & Miller, 2016). Third, the psychological constructs of perceived choice and positive affect do not respectively map onto a holistic definition of autonomy based on Self-Determination Theory or key tenets of OPTIMAL theory. Lastly, the authors claimed an interaction effect for their psychological data despite analyzing each timepoint separately (see Nieuwenhuis, Forstmann, & Wagenmakers, 2011 for a discussion of this issue). Given the relative dearth of skill acquisition experiments investigating autonomy-supportive instructional language and the methodological limitations of Hooyman et al. (2014), further investigation is required to assess the effectiveness of autonomy-supportive instructional language for motor performance and learning. This is addressed in the experiment described in Chapter 4.

1.5 Approach to dissertation

We often hear that science is a self-correcting process (e.g., Merton, 1973). A crucial element of this self-correcting process—and thus how science is supposed to work—is *replication*; we gain confidence in the accuracy of our findings only when they can be corroborated by other scientists (Ritchie, 2020). As I began my doctoral studies, I became increasingly aware of the growing recognition that the scientific literature was filled with unreliable and unreplicable findings (Baker, 2016; Chambers, 2017; Ioannidis, 2005; Open Science Collaboration, 2015); commonly referred to as the replication crisis. For instance, teams of scientists attempted to replicate 100 psychology experiments and failed 67% of the time (Open Science Collaboration, 2015). Similar concerns around replicability of findings have been noted for neuroscience (Boekel et al., 2015; Turner, Paul, Miller, & Barbey, 2018), medicine (NCI-NHGRI Working Group on Replication in Association Studies, 2007; Nosek & Errington, 2017), and economics (Christensen & Miguel, 2018) to name a few. Around this time, discussions surrounding the replicability of research in kinesiology and sport and exercise science were emerging (Aschwanden, 2019; Borg et al., 2020; Caldwell et al., 2020; Twomey et al., 2021) and the The Society for Transparency, Openness, and Replication in Kinesiology (https://storkinesiology.org/) was also created. Additionally, an important paper specific to motor learning research by Lohse et al. (2016) was instrumental to my scientific and statistical thinking, and how I wanted to approach research during my doctoral studies.

Lohse and colleagues (2016) surveyed articles from seven common journals where motor learning scientists published their work between January 2012 and August 2014. The authors noted evidence of positivity bias, improbable effect sizes, low statistical power, a high number of statistical tests in studies, and inconsistent analyses for retention and transfer tests. To address these issues, Lohse and colleagues (2016) provided five recommendations (see Table 1.1) related to replicability and reproducibility that motor learning researchers could adopt in their own research: 1) outcomes should be separated into primary and secondary outcomes, 2) *a priori* power calculations should be performed and reported, 3) hypotheses should be stated as formal statistical

Table 1.1. Recommendations forwarded by Lohse and colleagues (2016) to address problems in the motor learning literature and examples of how they were adopted in this dissertation.

Recommendation	Explanation	Example from dissertation
Outcomes should be separated into primary and secondary outcomes	The primary outcome variable should be what an experiment is powered to detect. Committing to a primary outcome variable should reduce generating hypotheses after results are known	In Chapter 4, stacking time is specified as the primary outcome variable
A-priori power calculations should be performed and reported	Authors should decide their sample size based on formal power calculations for the primary outcome variable	A-priori power calculations were done for experiments in every chapter
Hypotheses should be stated as formal statistical effects	Power analyses need to be conducted based on the formal hypothesis being tested (e.g., main effect, interaction)	The power analysis from Chapter 2 was done based on the main effect of Choice
Fully disclose participant recruitment, data filtering, and all analyses for transparency	Include all information about how participants were recruited and randomized, how data was processed, and if/how any data points or participants were excluded from analysis	All information about data collection, data, and code for all experiments are included open-access on the lab's GitHub page
Pre-register your protocol and analysis plan	Register the protocol, analysis plan, and predictions in a trial registry	Experiments from Chapters 3 and 4 were pre-registered using AsPredicted and the Open Science Framework

effects, 4) fully disclose participant recruitment, data filtering, and all analyses for transparency, and 5) pre-register your protocol and analysis plan. Although Lohse and colleagues (2016) acknowledged that adopting these recommendations might lead to considerable changes in how we conduct research (e.g., publish fewer but larger N studies), failing to make these changes would run the risk of having findings in our field viewed with great skepticism (p. 54). As such, I made a conscious effort to incorporate the recommendations forwarded by Lohse and colleagues (2016) into the planning and dissemination stages of my dissertation experiments.

1.6 Dissertation overview

The overarching goal of this dissertation was to critically test predictions related to the autonomy branch (see Table 1.2) of the motivation pillar from Wulf and Lewthwaite's (2016) OPTIMAL theory of motor learning. For most experiments, I used the *control over practice conditions* as the practice variable of interest. Control over knowledge of results feedback was used in Experiments 1 and 2, and control over video demonstrations

Table 1.2. Predictions made in OPTIMAL theory pertaining to autonomy-support and motor learning and which chapters of this dissertation will test them.

Number	Prediction	Chapters
2	Enhanced expectancies and autonomy support contribute to efficient goal-action coupling by readying the motor system for task execution	2, 3, 4
3	Autonomy-support facilitates performance by enhancing expectancies	2, 3, 4
7	Enhanced expectancies and autonomy support facilitate motor learning by making dopamine available for memory consolidation and neural pathway development	N/A

was used in Experiment 3. In Experiment 4, the practice variable I manipulated was *instructional language*, which to date has received minimal attention in the skill acquisition literature. The third autonomy related practice variable, incidental choices, was not explored given the mixed nature of the findings (e.g., Grand et al., 2017; Lewthwaite et al., 2015), and that control over a task-relevant practice feature was shown to be more effective than a task-irrelevant feature (Carter & Ste-Marie, 2017a; but see Wulf, Lewthwaite, et al., 2018 Experiment 2).[4] By manipulating these two practice variables in ways thought to either support or thwart autonomy, I assessed their utility for skill acquisition as predicted by Wulf and Lewthwaite in their OPTIMAL theory of motor learning.

The experiments described in the following chapters used motor tasks requiring upper limb movements that were either lab-based (Experiments 1 and 2) or applied (Experiments 3 and 4) in nature. Physical performance was measured using various behavioural outcome variables such as error scores (Experiments 1 and 2) and response time (Experiments 3 and 4). In all experiments I included measures related to perceptions of autonomy, competence, and intrinsic motivation—key psychological constructs identified in OPTIMAL theory (Wulf & Lewthwaite, 2016). All experiments included a pre-test, an acquisition phase, and delayed (~24 hours) retention and/or transfer tests, with performance on these delayed learning tests as the primary interest. Collectively, these experiments provide insights into the role of autonomy in skill acquisition and evaluate Wulf and Lewthwaite's (2016) claim that motivational factors have a *direct* influence on motor performance and learning. The four experiments are

[4]More recent experimental (McKay & Ste-Marie, 2020, 2022) and meta-analytic (McKay, Yantha, Hussien, Carter, & Ste-Marie, 2022) work that was published during collection of my dissertation work strongly supports my decision to have not used incidental choices as a practice variable of interest.

outlined briefly below, with the specific hypotheses and predictions described in the corresponding chapters.

1. In Experiment 1, I critically tested between the information-processing and motivational accounts of the self-controlled learning advantage. Autonomy and informational value of feedback were manipulated in a 2 Choice x 2 Feedback characteristics factorial design.

2. Experiment 2 was an extension of Experiment 1 to further probe explanations for the learning benefits of self-controlled practice conditions by providing feedback with minimal informational value to participants in self-controlled and yoked groups.

3. In Experiment 3, I examined the autonomy-supportive nature of choice and addressed a potential methodological limitation in the self-controlled literature. Here, a novel yoked group was included and participants in this group were made explicitly aware they were being denied choice opportunities provided to other participants in the experiment.

4. In Experiment 4, I examined the influence of instructional language on motor performance and learning by providing either autonomy-supportive or controlling instructions to participants during acquisition.

Chapter 2

Exercising choice over feedback schedules during practice is not advantageous for motor learning

A version of this chapter has been published:

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2.1 Introduction

The underlying source of errors in skilled actions are often ambiguous and difficult to assign as the learner must rely on noisy and delayed sensory information. Feedback from an external source, such as a coach or computer display, can facilitate or augment this process (Sigrist et al., 2013). Knowledge of results feedback (Salmoni et al., 1984) can provide varying amounts of information to learners depending on its characteristics. Error feedback provides precise information about the magnitude and direction of the error (e.g., -42 cm), graded feedback provides coarse information about either the magnitude or direction of the error (e.g., "too far"), and binary feedback indicates only success or failure information (e.g., "miss") (Luft, 2014).[1] When to provide this feedback is often decided by an external agent; however, this feedback decision can also be made by the learner, a form of self-controlled learning. These self-controlled feedback schedules have typically enhanced motor skill learning compared to yoked feedback schedules, wherein learners experience the feedback schedule created by a self-controlled counterpart, but without any choice (see Sanli et al., 2013; Ste-Marie, Carter, et al., 2020 for reviews).

Why self-controlled learning advantages emerge has garnered considerable attention in the motor skill learning literature. Within their OPTIMAL (Optimizing performance through intrinsic motivation and attentional learning) theory of motor learning, Wulf & Lewthwaite (2016) have argued that providing participants the opportunity to exercise choice, as in a self-controlled group, creates a virtuous cycle. Specifically, choice leads to increased (perceived) autonomy, leading to enhanced expectancies (e.g., perceived competence) and increased (intrinsic) motivation. These motivational influences lead to improved motor performance, creating a positive feedback loop that ultimately enhances motor learning compared to those not given the same choice opportunities. Support for this view has been drawn from experimental work where participants exercise choice over task-irrelevant or incidental choices. Exercising choice over the color of golf balls to putt (Lewthwaite et al., 2015 Experiment 1) or the mat underneath a target (Wulf, Iwatsuki, et al., 2018 Experiment 1), which picture to hang in a lab (Lewthwaite et al., 2015 Experiment 2), hand order in a maximal force production task (Iwatsuki et al., 2017), which photos to look at while running (Iwatsuki et al., 2018), and the order of exercises to perform (Wulf, Freitas, et al., 2014) have been suggested to improve motor performance or learning. Other research, however, have failed to replicate this

[1] Others have referred to error feedback as quantitative feedback and graded feedback as qualitative feedback (e.g., Magill & Wood, 1986). We use the terminology error, graded, and binary feedback because graded and binary feedback are different forms of qualitative feedback.

benefit of task-irrelevant or incidental choices on motor performance or learning (Carter & Ste-Marie, 2017b; Grand et al., 2017; McKay & Ste-Marie, 2020, 2022).

Rather than a motivational account, others have forwarded an information-processing explanation. From this perspective, exercising choice allows learners to tailor practice to their individual needs (Chiviacowsky & Wulf, 2002, 2005) by engaging in performance-contingent strategies (Carter et al., 2014; Carter, Rathwell, et al., 2016; Laughlin et al., 2015; Pathania et al., 2019) to reduce uncertainty about movement outcomes (Barros et al., 2019; Carter et al., 2014; Carter & Ste-Marie, 2017a, 2017b; Grand et al., 2015). Evidence for this view has come from experiments that showed the timing of the feedback decision relative to task performance matters (Carter et al., 2014; Chiviacowsky & Wulf, 2005), that task-relevant choices are more effective than task-irrelevant choices (Carter & Ste-Marie, 2017b; cf. Wulf, Iwatsuki, et al., 2018 Experiment 2), that interfering with information-processing activities during (Couvillion et al., 2020; Woodard & Fairbrother, 2020) or after (Carter & Ste-Marie, 2017a; Woodard & Fairbrother, 2020) task performance eliminates self-controlled learning benefits, and that the ability to accurately estimate one's performance is enhanced in choice compared to yoked groups (Carter et al., 2014; Carter & Patterson, 2012). Thus, further investigation is required to test predictions from these two explanations to better understand why exercising choice during practice confers an advantage for motor skill learning.

To dissociate between the motivational and information-processing accounts of the self-controlled learning advantage, we manipulated the amount of information participants in choice and yoked (i.e., no-choice) groups experienced with their feedback schedule during acquisition of a novel motor task. In Experiment 1, participants received error or graded feedback to assess how high and moderate levels of informational value impact the self-controlled learning advantage. Given both error and graded feedback provide salient information about how to correct one's behavior relative to the task goal (i.e., both generate an error signal), in Experiment 2 we provided participants with binary feedback. As binary feedback is devoid of information about the necessary change to improve one's behavior (i.e., does not generate an error signal), we could better isolate the motivational nature of choice to test between the two explanations for the self-controlled learning advantage. Motor learning was assessed using delayed (~24 hours) retention and transfer tests. If the OPTIMAL theory is correct, we hypothesized that the characteristics of one's feedback schedule *would not matter* for the self-controlled learning advantage as this advantage arises from the opportunity for choice–a common feature of all choice groups. Thus, we predicted all choice groups would demonstrate superior performance and learning compared to the yoked groups. Alternatively, if the information-processing account is correct, we hypothesized that the characteristics of one's feedback schedule *would matter* for the self-controlled learning advantage as feedback with greater informational value would be more effective for reducing uncertainties about movement outcomes. Thus, we predicted that choice over an error feedback schedule would be the most effective pairing for performance and learning. We also included self-report measures of perceptions of autonomy, competence, and intrinsic motivation, and assessments of error estimation abilities to respectively test auxiliary assumptions of the OPTIMAL theory and information-processing explanations.

2.2 Methods

We report how we determined our sample size, all data exclusions (if any), all manipulations, and all measures in the study (Simmons, Nelson, & Simonsohn, 2012). All data and `R` scripts can be accessed here: https://github.com/cartermaclab/expt_sc-feedback-characteristics.

2.2.1 Participants

Experiment 1

One hundred and fifty-two right-handed (Oldfield, 1971), healthy adults participated in Experiment 1 (M_{age} = 20.64 years, SD_{age} = 2.45, 88 females). Sample size was determined from an *a-priori* power analysis using the *ANOVA: fixed effects, main effects and interactions* option in G*Power (Faul, Erdfelder, Buchner, & Lang, 2009) with the following parameters: $\alpha = 0.05$, $\beta = .20$, $f = 0.23$, numerator = 1, and groups = 4. This revealed a required sample of 151 participants. Our chosen effect size was based on a meta-analytic estimate ($f = .32$) by McKay, Carter, & Ste-Marie (2014); however, we used a more conservative estimate given the uncertainty of how choice would interact with our feedback characteristic manipulation. Participants were compensated $15 CAD or with course-credit for their time. All participants gave written informed consent and the experiment was approved by McMaster University's Research Ethics Board.

Experiment 2

A new sample of 76 right-handed (Oldfield, 1971), healthy adults participated in Experiment 2 (M_{age} = 20.18 years, SD_{age} = 3.18, 47 females). Sample size was selected so group size matched that used in Experiment 1. Participants were compensated $15 CAD or with course-credit for their time. All participants gave written informed consent and the experiment was approved by McMaster University's Research Ethics Board.

2.2.2 Task

In Experiments 1 and 2, participants sat in a chair facing a monitor (1920x1080 resolution) with their left arm in a custom manipulandum that restricted movement to the horizontal plane. Their elbow was bent at approximately 90° and they grasped a vertical handle with their left hand. Handle position was adjusted as needed to ensure the central axis of rotation was about the elbow. The task required a rapid "out-and-back" movement such that the reversal happened at 40° (in pre-test, acquisition, and retention) or 60° (in transfer). The starting point for all trials was 0°. Participants were instructed to make a smooth movement to the reversal and back without hesitating when reversing their movement. The movement time goal to the reversal was always 225 ms. The task and instructions were similar to those used by Sherwood (1996; 2009). Vision of the manipulandum and limb were occluded during all phases of the experiment. Angular displacement for the elbow was collected via a potentiometer attached to the axis of rotation of the custom manipulandum. Potentiometer data were digitally sampled at 1000 Hz (National Instruments PCIe-6321) using a custom LabVIEW program and stored for offline analysis.

2.2.3 Procedure

Experiment 1

The first 76 participants were randomly assigned to either the Choice+Error-Feedback group (n = 38; M_{age} = 20.24 years, SD_{age} = 2.37, 22 females) or the Choice+Graded-Feedback group (n = 38; M_{age} = 20.76 years, SD_{age} = 3.02, 26 females). This is typical in the self-controlled learning literature as the self-controlled participants' self-selected feedback schedules are required for providing feedback to the participants in the yoked (i.e., control) groups. The remaining 76 participants were randomly assigned to either the Yoked+Error-Feedback group (n = 38; M_{age} = 20.53 years, SD_{age} = 2.13, 23 females) or the Yoked+Graded-Feedback group (n = 38; M_{age} = 21.03 years, SD_{age} = 2.32, 22 females).

Data collection consisted of two sessions separated by approximately 24 hours.[2]

[2]Six participants (three Choice+Error-Feedback and three Choice+Graded-Feedback) had their second session completed approximately 48 hours later because a snowstorm closed the University.

Session one included a pre-test (12 trials) and an acquisition phase (72 trials). Session two included the delayed retention (12 trials) and transfer (12 trials) tests. No feedback about motor performance was provided in pre-test, retention, or transfer. Prior to the pre-test, all participants received instructions about the task and its associated spatial and timing goals. Additionally, half of the participants in each group were randomly selected to verbally estimate their performance on the spatial and timing goals after each trial in the pre-test. Only a subset of participants were asked to estimate their performance in pre-test to mitigate the potential that doing so would prompt participants to adopt this strategy during the experiment as error estimation has been suggested (e.g., Chiviacowsky & Wulf, 2005) to be adopted spontaneously by participants controlling their feedback schedule. However, asking participants to estimate their performance during pre-test is necessary to be able to assess how this skill develops as a function of one's practice condition.

Participants were reminded of the instructions about the task and its associated goals at the start of the acquisition phase. Group specific instructions regarding feedback were also provided. Participants in the Choice+Error-Feedback group and the Choice+Graded-Feedback group were told they could choose their feedback schedule, with the restriction that they must select feedback on 24 of the 72 acquisition trials. They were informed that if the number of remaining feedback requests equaled the number of remaining acquisition trials, these trials would default to feedback trials. This feedback restriction was implemented to ensure the relative frequency of feedback was equated across all groups. Similar restrictions have been used in past research involving multiple choice groups (e.g., Chiviacowsky & Wulf, 2005). Participants in the Yoked+Error-Feedback group and the Yoked+Graded-Feedback group were told they may or may not receive feedback following a trial based on a predetermined schedule. Thus, participants in these groups were not aware that their feedback schedule was actually created by a participant in a corresponding choice group. While this yoking procedure ensures that the total number of feedback trials and their relative placement during acquisition are identical, the content of the feedback reflected each participant's own performance. Error feedback for the spatial and timing goals was provided as the difference between the participant's actual performance and the task goal (i.e., constant error). Graded feedback for the spatial goal was provided as "too short" if performance was $<$ 40 degrees (or 60 degrees in transfer), "hit" if exactly 40 degrees, and "too far" if $>$ 40 degrees. For the timing goal, graded feedback was provided as "too fast" when performance was $<$ 225 ms, "hit" if exactly 225 ms, and "too slow" if $>$ 225 ms. All participants were shown a sample feedback display that corresponded to their

Table 2.1. Cronbach's alpha for each questionnaire at each timepoint.

	After pre-test	After block 1	After block 6	Before retention
Experiment 1				
Perceived autonomy	0.68	0.81	0.80	0.83
Perceived competence	0.86	0.93	0.95	0.94
Intrinsic motivation	0.86	0.90	0.92	0.93
Experiment 2				
Perceived autonomy	0.39	0.73	0.79	0.85
Perceived competence	0.92	0.88	0.91	0.91
Intrinsic motivation	0.91	0.91	0.93	0.94

Note. Block 1 and 6 are from the acquisition phase.

experimental group and were asked to interpret it aloud for the researcher to verify understanding.

A typical acquisition trial (see Figure 2.1) began with the current trial number displayed (500 ms), followed by a visual "Get Ready!" and a visual go-signal (800 ms apart). Participants were free to begin their movement when ready following the visual go-signal (i.e., green circle) as this was not a reaction time task. The computer screen was blank while participants made their movement. When participants returned to the starting position, a red circle was displayed on the monitor. Following a 2000 ms feedback delay interval, the feedback decision prompt was presented for the self-controlled groups. The number of remaining feedback trials was also displayed during this feedback delay interval. If feedback was not selected, a blank screen was displayed for 3000 ms. If feedback was selected via verbal response (or imposed on the yoked groups), it was also displayed for 3000 ms.

Before the retention and transfer tests, participants were reminded about the task and its associated goals. All participants were asked to verbally estimate their performance after each trial in retention and transfer. After the pre-test, trials 12 and 72 in acquisition, and before the delayed retention test, participants verbally answered a series of questions pertaining to perceived competence, task interest and enjoyment, and perceived autonomy.[3] The perceived competence and task interest and enjoyment questions were from the Intrinsic Motivation Inventory (McAuley, Duncan, & Tammen, 1989; Ryan, 1982) and the perceived autonomy questions were used in earlier work (Barros et al., 2019; Carter & Ste-Marie, 2017b; St. Germain et al., 2022). Cronbach's alpha values for each questionnaire at each time point are reported in Table 2.1.

[3]The questionnaires can be found in the publicly available project repository in the materials directory.

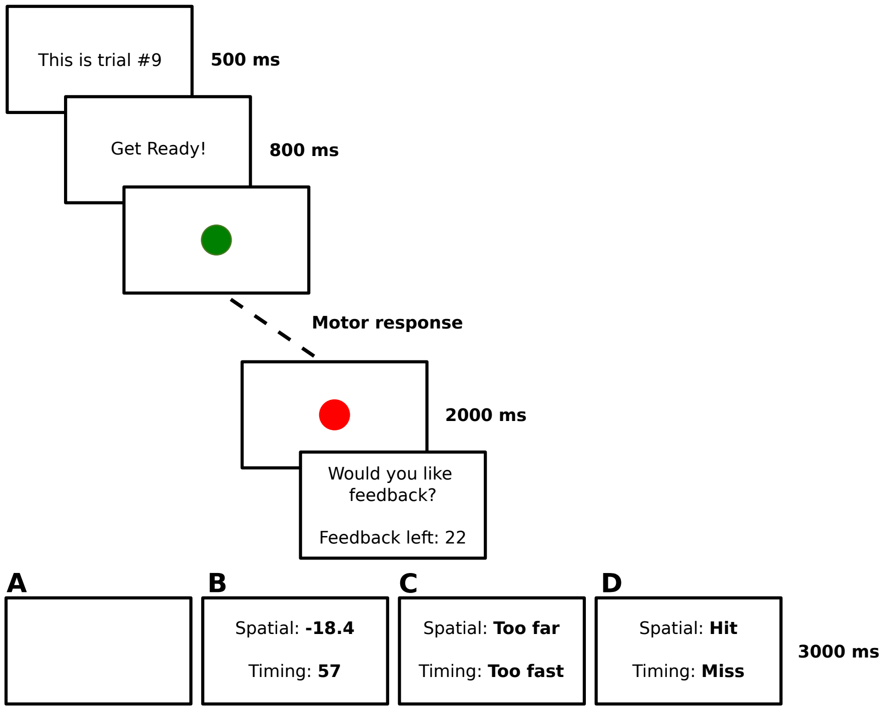

Figure 2.1. Overview of a typical acquisition trial for the choice groups. The sequence of events a participant in the choice groups experienced during the acquisition phase. Trials began by informing participants the trial number (500 ms) they were on in acquisition. Shortly after, the text "Get Ready!" appeared on the screen and 800 ms later a visual go-signal was presented in the form of a green circle in the center of the screen. Participants began their movement when ready after seeing the visual go-signal as we were not interested in reaction time. While participants completed their rapid out-and-back movement, the computer screen was blank. Upon returning to the starting position, a red circle appeared in the center of the screen. A 2000 ms feedback delay interval was used and this interval was followed by the feedback prompt. The feedback prompt also displayed an updated counter representing the number of feedback trials they had left. If the number of remaining feedback trials matched the number of acquisition trials left, these trials automatically defaulted to feedback trials. On trials where feedback was not requested, a blank screen **(A)** was shown for 3000 ms. When feedback was selected via verbal response, feedback was provided for both the spatial and timing goals according to their experimental group. The error feedback group **(B)** saw their constant error, the graded feedback group **(C)** saw either "too far" or "too short" for the spatial goal and "too fast" or "too slow" for the timing goal, and the binary feedback group **(D)** saw either "hit" or "miss" for the task goals. The sequence of events was the same for the yoked groups with the exception they did not see a feedback prompt. The sequence of events was similar in pre-test, retention, and transfer except all trials were no-feedback trials.

Experiment 2

Similar to Experiment 1, the first half of participants were assigned to the Choice+Binary-Feedback group (n = 38; M_{age} = 22.37 years, SD_{age} = 3.13, 19 females) and the remaining participants were assigned to the Yoked+Binary-Feedback group (n = 38; M_{age} = 18.00 years, SD_{age} = 0.93, 28 females). Binary feedback for the spatial goal was provided as "hit" if performance was exactly 40 degrees (or 60 degrees in transfer) and as "miss" for everything else. For the timing goal, binary feedback was provided as "hit" when performance was exactly 225 ms and as "miss" for everything else. Data collection was identical to that of Experiment 1, except in the acquisition instructions participants in both groups were shown a sample binary feedback display and were asked to interpret it aloud for the researcher to verify understanding.

2.2.4 Data Analysis

Movement trajectories for all trials were visually inspected by a researcher and trials with errors (e.g., technical issues, moving before the "go" signal) were removed. A total of 4.03% (662/16146) and 3.73% (306/8208) of trials for Experiments 1 and 2 were removed, respectively. Trials were aggregated into blocks of 12 trials, resulting in one block of trials for pre-test, retention, and transfer, and six blocks of trials for acquisition. Our primary performance outcome variable was total error (E) (Henry, 1974, 1975) and was computed using the equation:

$$E = \sqrt{\sum (x_i - T)^2/n} \tag{2.1}$$

where x_i is the score on the ith trial, T is the target goal, and n is the number of trials in a block.

To test for performance differences in pre-test, retention, and transfer, total error for the spatial and timing goals were analyzed in separate mixed ANOVAs (*Experiment 1:* 2 Choice x 2 Feedback x 3 Test; *Experiment 2:* 2 Choice x 3 Test). To test for performance differences during acquisition, total error for the spatial and timing goals during acquisition were analyzed in separate mixed ANOVAs (*Experiment 1:* 2 Choice x 2 Feedback x 6 Block; *Experiment 2:* 2 Choice x 6 Block). Model diagnostics of total error for the spatial and timing goals revealed skewed distributions. We therefore conducted sensitivity analyses using the shift function, which is a robust statistical method well-suited for skewed distributions (Rousselet & Wilcox, 2020; Rand R. Wilcox, 2021). The results of these analyses (see **Supplementary 2A**) were consistent with those of the mixed ANOVAs, which we report below. Our primary psychological outcome variables were intrinsic motivation (i.e., interest/enjoyment), perceived competence, and perceived autonomy. The mean score of the responses for these constructs at each time point was calculated for each participant and analyzed in separate mixed ANOVAs (*Experiment 1:* 2 Choice x 2 Feedback x 4 Time; *Experiment 2:* 2 Choice x 4 Time). Of secondary interest, error estimation abilities were assessed as total error between a participant's estimation and actual performance in pre-test (50% of the participants in each group in Experiments 1 and 2), retention, and transfer (see **Supplementary 2B**).

Alpha was set to .05 for all statistical analyses. Corrected degrees of freedom using the Greenhouse-Geisser technique are always reported for repeated measures with more than two levels. Generalized eta squared (η^2_G) is provided as an effect size statistic (Bakeman, 2005; Daniël Lakens, 2013; Olejnik & Algina, 2003) for all omnibus tests. Post hoc comparisons were Holm-Bonferroni corrected to control for multiple comparisons.

2.3 Results

2.3.1 Pre-test, retention, and transfer

Experiment 1

Spatial (Fig. 2.2A) and timing (Fig. 2.2B) error decreased from the pre-test to the retention and transfer tests. There was a main effect of Test for spatial error, $F(1.33, 196.52) = 40.20$, $p < .001$, $\eta^2_G = .138$, where performance was less errorful in retention and transfer than pre-test (p's $< .001$) and performance in retention was better than transfer ($p < .001$). A main effect of Test was also found for timing error, $F(1.08, 160.23) = 81.21$, $p < .001$, $\eta^2_G = .245$, with pre-test performance more errorful than both retention and transfer (p's $< .001$), and retention was less errorful than transfer ($p < .001$). The main effect of Choice was not significant for both spatial, $F(1, 148) = .52$, $p = .471$, $\eta^2_G = .001$, and timing, $F(1, 148) = .32$, $p = .547$, $\eta^2_G < .001$, error.

Experiment 2

Spatial (Fig. 2.3A) and timing (Fig. 2.3B) error did not change considerably from the pre-test to the retention and transfer tests. The main effect of Choice was not significant for both spatial, $F(1, 74) = .23$, $p = .631$, $\eta^2_G = .002$, and timing, $F(1, 74) = .11$, $p = .738$, $\eta^2_G = .001$, error. All other main effects and interactions were also not significant.

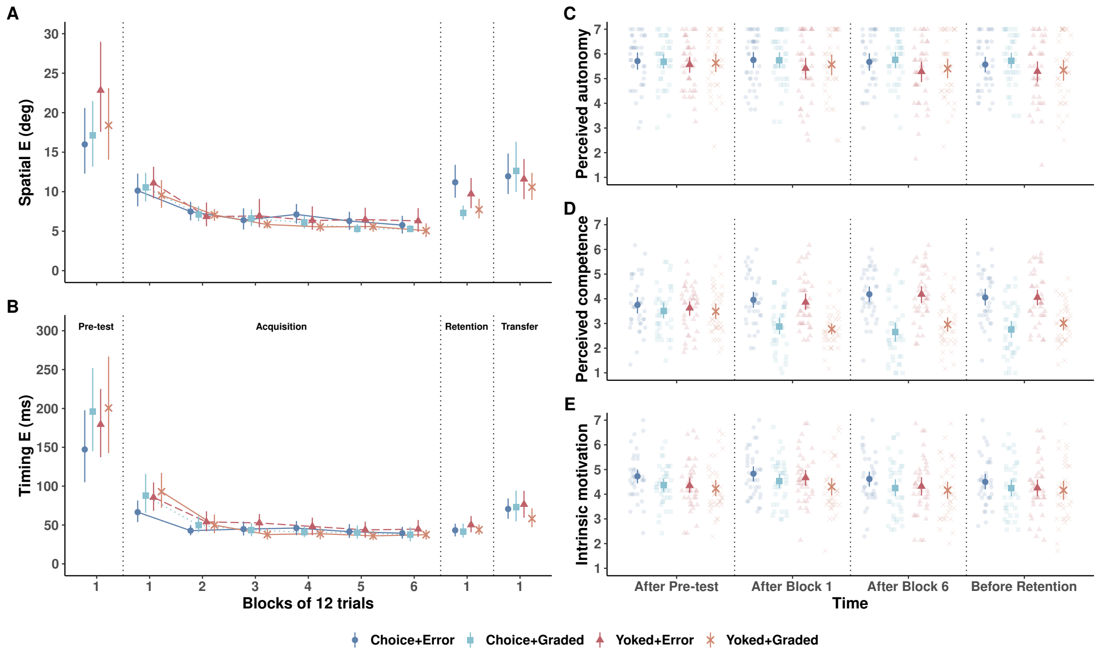

Figure 2.2. Experiment 1 data. The Choice with error feedback (Choice+Error) group is shown in dark blue circles, the Choice with graded feedback (Choice+Graded) group is shown in light blue squares, the Yoked with error feedback (Yoked+Error) group is shown in red triangles, and the Yoked with graded feedback (Yoked+Graded) group is shown in yellow crosses. Error bars denote 95% bootstrapped confidence intervals. **(A)** Spatial total error (degrees) and **(B)** timing total error (ms) averaged across blocks and participants within each group. Dotted vertical lines denote the different experimental phases. Pre-test and acquisition occurred on Day 1 and retention and transfer occurred approximately 24-hours later on Day 2. Self-reported scores for perceived autonomy **(C)**, perceived competence **(D)**, and intrinsic motivation **(E)** after the pre-test and after blocks 1 and 6 of acquisition on Day 1, and before the retention test on Day 2. Scores could range on a Likert scale from 1 (Strongly disagree) to 7 (Strongly agree). Dots represent individual data points.

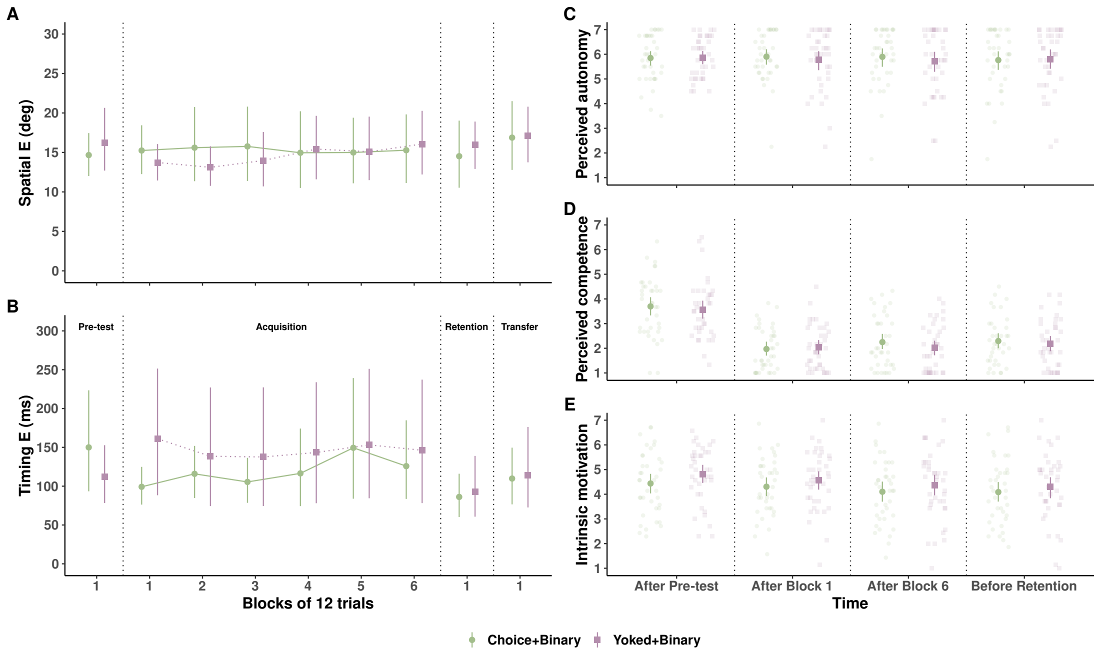

Figure 2.3. Experiment 2 data. The Choice with binary feedback (Choice+Binary) group is shown in green circles and the Yoked with binary feedback (Yoked+Binary) group is shown in purple squares. Error bars denote 95% bootstrapped confidence intervals. **(A)** Spatial total error (degrees) and **(B)** timing total error (ms) averaged across blocks and participants within each group. Dotted vertical lines denote the different experimental phases. Pre-test and acquisition occurred on Day 1 and retention and transfer occurred approximately 24-hours later on Day 2. Self-reported scores for perceived autonomy **(C)**, perceived competence **(D)**, and intrinsic motivation **(E)** after the pre-test and after blocks 1 and 6 of acquisition on Day 1, and before the retention test on Day 2. Scores could range on a Likert scale from 1 (Strongly disagree) to 7 (Strongly agree). Dots represent individual data points.

2.3.2 Acquisition

Experiment 1

All groups of participants improved their performance of the spatial goal during the acquisition phase (Fig. 2.2A). This was supported by a significant main effect of Block, $F(2.41, 357.13) = 60.18$, $p < .001$, $\eta^2_G = .130$, where block 1 was less accurate than all other blocks (p's $< .001$), block 2 was less accurate than all subsequent blocks (p's $\leq .021$), and blocks 3 and 4 were more errorful than block 6 (p's $\leq .015$). The main effect of Choice was not significant, $F(1, 148) = .06$, $p = .813$ $\eta^2_G < .001$. Timing error also decreased during the acquisition period (Fig. 2.2B). The significant main effect of Block, $F(1.75, 259.59) = 55.44$, $p < .001$, $\eta^2_G = .138$, was superseded by a significant Feedback x Block interaction, $F(1.75, 259.59) = 3.56$, $p = .035$, $\eta^2_G = .010$. Post hoc comparisons showed that timing error for those receiving error feedback was reduced from block 1 in all subsequent blocks (p's $< .001$), but performance plateaued from block 2 onward in acquisition (p's $\geq .257$). Timing error for the participants that received graded feedback was also reduced from block 1 in all subsequent blocks (p's $< .001$); however, these participants continued to improve across acquisition blocks as block 2 was more errorful than blocks 3 to 6 (p's $\leq .028$). The main effect of Choice was not significant, $F(1, 148) = .54$, $p = .465$ $\eta^2_G = .002$. Descriptives for the number of "hit" trials for each group are provided in Table 2.2.

Experiment 2

Spatial (Fig. 2.3A) and timing (Fig. 2.3B) error remained relatively flat from block 1 to block 6 in the acquisition period. The main effect of Choice for both the spatial, $F(1, 74) = .08$, $p = .776$, $\eta^2_G < .001$, and the timing, $F(1, 74) = .37$, $p = .542$, $\eta^2_G = .004$, goals were not significant. All other main effects and interactions for both task goals were not significant. Descriptives for the number of "hit" trials for each group are provided in Table 2.2.

2.3.3 Psychological variables

Experiment 1

Perceptions of autonomy (Fig. 2.2C) showed a slight decrease across time points, supported by a main effect of Time, $F(2.25, 332.95) = 3.69$, $p = .022$, $\eta^2_G = .003$. Perceived autonomy was higher after block 1 of acquisition compared to self-reported ratings prior to completing the retention test ($p = .031$). The main effect of Choice

Table 2.2. Total number of "hits" for the spatial and timing goals during acquisition for each group, and the minimum and maximum "hits" at the participant level within each group.

	Spatial goal		Timing goal	
Group	Total	Min–Max	Total	Min–Max
Experiment 1				
Choice+Error-Feedback	4	0–2	30	0–4
Choice+Graded-Feedback	2	0–1	26	0–4
Yoked+Error-Feedback	1	0–1	28	0–3
Yoked+Graded-Feedback	3	0–1	33	0–3
Experiment 2				
Choice+Binary-Feedback	2	0–1	14	0–3
Yoked+Binary-Feedback	4	0–1	10	0–2

was not significant, $F(1,148) = 2.38$, $p = .125$, $\eta^2_G = .014$. Self-ratings for perceived competence (Fig. 2.2D) were similar across groups after the pre-test, but then began to diverge after block 1 based on feedback characteristic. Main effects of Time, $F(1.92, 283.47) = 3.43$, $p = .036$, $\eta^2_G = .006$, and Feedback, $F(1,148) = 47.36$, $p < .001$, $\eta^2_G = .188$, were superseded by a Feedback x Time interaction, $F(1.92, 283.47) = 28.04$, $p < .001$, $\eta^2_G = .050$. Perceived competence scores were not significantly different after the pre-test ($p = .232$); however, perceptions of competence were significantly lower in those participants receiving graded feedback compared to error feedback at all other time points (p's $< .001$). The main effect of Choice was not significant, $F(1,148) = 0.03$, $p = .862$, $\eta^2_G < .001$. Self-reported scores for intrinsic motivation (Fig. 2.2E) generally decreased after block 1, which was supported by a main effect of Time, $F(2.40, 355.90) = 14.69$, $p < .001$, $\eta^2_G = .012$. Intrinsic motivation scores initially increased following the pre-test to after block 1 ($p = .003$); however, scores after block 1 of acquisition were greater than those reported at the end of acquisition (i.e., block 6) and before retention (p's $< .001$). Self-reported ratings were also lower before retention compared to after the pre-test ($p = .043$). The main effect of Choice was not significant, $F(1,148) = 1.69$, $p = .195$, $\eta^2_G = .010$.

Experiment 2

Self-reported scores for perceived autonomy (Fig. 2.3C) were similar across all time points. The main effect of Choice was not significant, $F(1,74) = 0.07$, $p = .792$, $\eta^2_G < .001$. All other main effects and interactions were also not significant. Perceptions of competence (Fig. 2.3D) showed a considerable decrease after the pre-test, $F(1.85, 136.91) = 106.10$, $p < .001$, $\eta^2_g = .298$, where scores were significantly greater after the pre-test compared to all other time points (p's $< .001$), and were higher after block 1 of acquisition than before retention ($p = .004$). The main effect of Choice was not significant, $F(1, 74) = 0.25$, $p = .620$, $\eta^2_G = .002$. Self-ratings for intrinsic motivation generally decreased across time points (Fig. 2.3E), which was supported by a main effect of Time, $F(2.37, 175.55) = 15.31$, $p < .001$, $\eta^2_G = .018$. Intrinsic motivation was higher after the pre-test than after block 6 of acquisition and before retention (p's $< .001$), and higher after block 1 than after block 6 and before retention (p's $< .026$). The main effect of Choice was not significant, $F(1, 74) = 1.04$, $p = .312$, $\eta^2_G < .013$.

2.3.4 Equivalence analysis

Our main comparison of interest was between choice and yoked (i.e., no-choice) groups. To evaluate the self-controlled learning effect, Hedges' g for the spatial and timing goals were aggregated within each experiment while accounting for within-subject dependencies (see **Supplementary 2C** for the psychological data). Next, random effects meta-analyses were conducted on the retention test data[4] to generate a summary point estimate and 90% confidence intervals with Experiments 1 and 2 combined and also separate. The overall estimated effect when combining both experiments was $g = .05$ (favoring self-controlled) and 90% confidence interval [-.12, .23]. The overall estimated effect for Experiment 1 was $g = .03$ (favoring self-controlled) and 90% confidence interval [-.19, .25]. For Experiment 2, it was $g = .09$ (favoring self-controlled) and 90% confidence interval [-.19, .37].

Equivalence tests can be conducted to evaluate whether the observed differences are significantly smaller than a pre-determined smallest effect size of interest (see Harms & Lakens, 2018 for a discussion). Typically, a two one-sided tests procedure is used to compare the observed effect to upper and lower equivalence bounds, and if the effect is significantly smaller than both bounds then the hypothesis that the effect is large enough to be of interest is rejected (Daniel Lakens, 2017; Schuirmann, 1987). However, we did not pre-specify a smallest effect of interest, so instead we report the 90% confidence intervals (see above). All effect sizes outside this interval would be rejected by the two-one sided tests procedure while all values inside the interval would not. Based on the combined overall estimate the present experiments can be considered inconsistent with all effects larger than $g = \pm.23$.

[4]We report an estimate for retention tests to facilitate comparison to a recent meta-analysis (McKay, Yantha, et al., 2022) that produced estimated effects of self-controlled learning at retention specifically.

2.4 Discussion

The purpose of the present experiments was to test between motivational and information-processing accounts of the putative self-controlled learning advantage (see Ste-Marie, Carter, et al., 2020 for a review). According to the OPTIMAL theory (Wulf & Lewthwaite, 2016), self-controlled practice or choice conditions are advantageous because the provision of choice increases perceptions of autonomy and competence, which increase intrinsic motivation and ultimately both motor performance and learning. Conversely, others have argued that self-controlled feedback is effective because it provides the opportunity to request feedback in a performance dependent way that reduces uncertainty about movement outcomes relative to task goals (Carter et al., 2014; Carter & Ste-Marie, 2017a; Grand et al., 2015) to enhance error detection and correction abilities (Barros et al., 2019; Carter et al., 2014; Chiviacowsky & Wulf, 2005). In contrast to these predictions, we did not find evidence that providing learners with choice over their feedback schedule was beneficial for motor learning, despite collecting a much larger sample ($N = 228$ across both Experiments) than those commonly used in self-controlled learning experiments (median sample size $N = 36$ in a meta-analysis by McKay, Yantha, et al., 2022) and motor learning experiments in general (median n/group = 11 in a review by Lohse et al., 2016). Further, exercising choice in practice did not enhance perceptions of autonomy, competence, or intrinsic motivation, and also did not result in more accurate performance estimations in delayed tests of motor learning. Overall, we found no support for the OPTIMAL theory or information-processing perspective. Our results challenge the prevailing view that the self-controlled learning benefit is a robust effect.

The failed replication of a self-controlled learning advantage was surprising given the dominant view for the past 25 years has been that it is a robust effect and one that should be recommended to coaches and practitioners (Sanli et al., 2013; Ste-Marie, Carter, et al., 2020; Wulf & Lewthwaite, 2016). Our findings are, however, consistent with a growing list of relatively large–often pre-registered–experiments that have not found self-controlled learning benefits (Bacelar, Parma, Cabral, et al., 2022; Grand et al., 2017; Leiker, Pathania, Miller, & Lohse, 2019; McKay & Ste-Marie, 2020, 2022; St. Germain et al., 2022; Yantha, McKay, & Ste-Marie, 2022). One possible explanation for this discrepancy between earlier and more recent experiments may be that the self-controlled learning advantage was the result of underpowered designs, which has been highlighted as a problem in motor learning research (see Lohse et al., 2016 for a discussion). When underpowered designs find significant results, they are prone to be false positives with inflated estimates of effects (Button et al., 2013; Daniel Lakens, 2014), which can be further exaggerated with questionable research practices such as p-hacking and selective reporting (e.g., Munafò et al., 2017; Simmons, Nelson, & Simonsohn, 2011). Thus, a self-controlled learning advantage may not actually exist. Alternatively, if one does exist then it seems likely it is a much smaller effect than originally estimated and requires considerably larger samples to reliability detect than those commonly used in motor learning research. Consistent with these ideas, a recent meta-analysis provided compelling evidence that the self-controlled learning advantage is not robust and its prominence in the motor learning literature is due to selective publication of statistically significant results (McKay, Yantha, et al., 2022). We estimated the overall effect of self-controlled practice in retention collapsed across experiments to be significantly smaller than any effect larger than g = .23. This is consistent with the estimates from McKay, Yantha, et al. (2022) after accounting for publication bias (g = -.11 to .26), which suggested either no effect or a small effect in an unknown direction. Taken together, we argue that it may be time for the self-controlled learning advantage to be considered a non-replicable effect in motor learning.

Given our current replication failure with those in recent years (Bacelar, Parma, Cabral, et al., 2022; Grand et al., 2017; Leiker et al., 2019; McKay & Ste-Marie, 2020, 2022; St. Germain et al., 2022; Yantha et al., 2022) and the conclusions from McKay, Yantha, et al. (2022), motivational (i.e., OPTIMAL theory) versus information-processing explanations seem moot. Nevertheless, the present results are incompatible with both perspectives.[5] Specifically, having choice opportunities during practice did not enhance perceptions of autonomy, competence, or intrinsic motivation in either experiment, inconsistent with OPTIMAL theory. Similarly, self-controlled feedback schedules did not enhance error estimation skills compared to yoked schedules (see **Supplementary 2B**) and choice did not interact with feedback characteristics, inconsistent with the information-processing perspective. Instead, the results from Experiments 1 and 2 suggest that feedback characteristics were a more important determinant of motor performance during acquisition and delayed tests of learning than the opportunity to choose. When feedback provided information about the direction of an error or when it contained both direction and magnitude of an error, participants were able to improve at the task throughout acquisition and retain these improvements in skill relative to pre-test. However, when feedback was binary and direction and magnitude

[5]Although the lack of performance improvements in Experiment 2 are compatible with the information-processing perspective, we do not interpret this as support for this view over the motivational one given the conclusions from McKay and colleagues' (in-press) recent meta-analysis.

of an error was absent, there was no improvement in skill from baseline levels. This is in contrast with past research that has shown people can learn motor tasks with binary feedback (Cashaback et al., 2019; e.g., Cashaback, McGregor, Mohatarem, & Gribble, 2017; Izawa & Shadmehr, 2011). One possible explanation for this discrepancy may be the amount of practice trials (Magill & Wood, 1986). Practicing with binary feedback may inherently require a longer training period for learning to occur compared to graded and error feedback, which both have greater precision. Additionally, we used a strict criteria with binary feedback where any outcome other than zero error was considered a miss. Thus, binary feedback may be more effective when paired with a tolerance zone such as that used in the bandwidth technique (see Anderson, Magill, Mayo, & Steel, 2020 for a review; Cauraugh, Chen, & Radio, 1993; Lee & Carnahan, 1990).

Although unexpected, the influence of feedback characteristics on perceptions of competence may hint to a dissociation between informational and motivational impacts of knowledge-of-results feedback. In Experiment 1, participants who received graded feedback reported significantly lower perceptions of competence than participants who received error feedback. Yet, despite these lower expectations for success, the graded feedback groups did not demonstrate degraded performance or learning compared to the error feedback group. Participants in Experiment 2 who received binary feedback reported the lowest perceptions of competence and were also the only participants who did not show improvements in task performance from pre-test. The number of "hits" for the spatial and timing goals were quite low for all groups. Although this may have impacted perceptions of competence, the relatively low "hit" rate did not seem to differentially impact intrinsic motivation as self-reported levels were quite similar for all groups. Future research is necessary to better understand this dissociation of informational and motivational influences of feedback characteristics and how it interacts with the task, individual, and environment.

In two experiments we failed to observe the predicted benefits of self-controlled feedback on motor learning. Similarly, we failed to find the predicted motivational and informational consequences of choice in either experiment, challenging both the OPTIMAL theory and information-processing explanation of the so-called self-controlled learning advantage. Although the present experiments were not pre-registered, the analysis plan was determined prior to viewing the data. In addition, a suite of sensitivity analyses were conducted to determine the extent to which the present results depended on the chosen analysis methods (see **Supplementary 2A**). The sensitivity analyses supported the conclusions of the primary analyses and are consistent with research that has followed pre-registered analysis plans (Bacelar, Parma, Cabral, et al., 2022; Grand et al., 2017; Leiker et al., 2019; McKay & Ste-Marie, 2020, 2022; St. Germain et al., 2022; Yantha et al., 2022). Lastly, our results and conclusions are in line with a recent meta-analysis (McKay, Yantha, et al., 2022) that suggests the apparent benefits of self-controlled practice are due to selection bias rather than true effects.

2.5 Supplementary 2A

Model diagnostics of total error (E) for the spatial and timing goals revealed skewed distributions. As a result, we carried out sensitivity analyses using shift functions, which is a robust technique that is well-suited for skewed distributions (Rousselet & Wilcox, 2020; Rand R. Wilcox, 2021). For all shift functions, we collapsed across retention and transfer data to compute a single post-test score for both the spatial and timing goals. Given our primary interest was related to the role of choice during practice for motor learning and shift functions only compare two groups, we only ran these analyses on the choice factor for Experiment 1 (collapsed across feedback) and Experiment 2. Conducting a shift function analysis is a multi-step process that first involved calculating the 20% trimmed means for each participant and time point. Next, deciles for the factor of choice (choice versus yoked) in Experiments 1 and 2 were calculated using the Harrell-Davis estimator (Harrell & Davis, 1982; Rousselet, Pernet, & Wilcox, 2017). Lastly, differences at each decile were evaluated based on the 95% confidence intervals corrected for multiple comparisons using Hochberg's method (Hochberg, 1988). Spatial and timing measures were analyzed separately for Experiments 1 and 2.

Choice versus yoked

For Experiment 1, the 95% confidence intervals overlapped with zero at each decile when comparing the choice and yoked groups for both the spatial (Fig. 2.4A) and timing (Fig. 2.5A) goals. Similarly, in Experiment 2 there were no significant differences between the choice and yoked groups in any decile for the spatial (Fig. 2.4B) and timing (Fig. 2.5B) goals. The results from these shift function analyses are consistent with the analyses reported in the main manuscript and a recent meta-analysis (McKay, Yantha, et al., 2022), suggesting that giving learners choice during practice is not advantageous for motor learning.

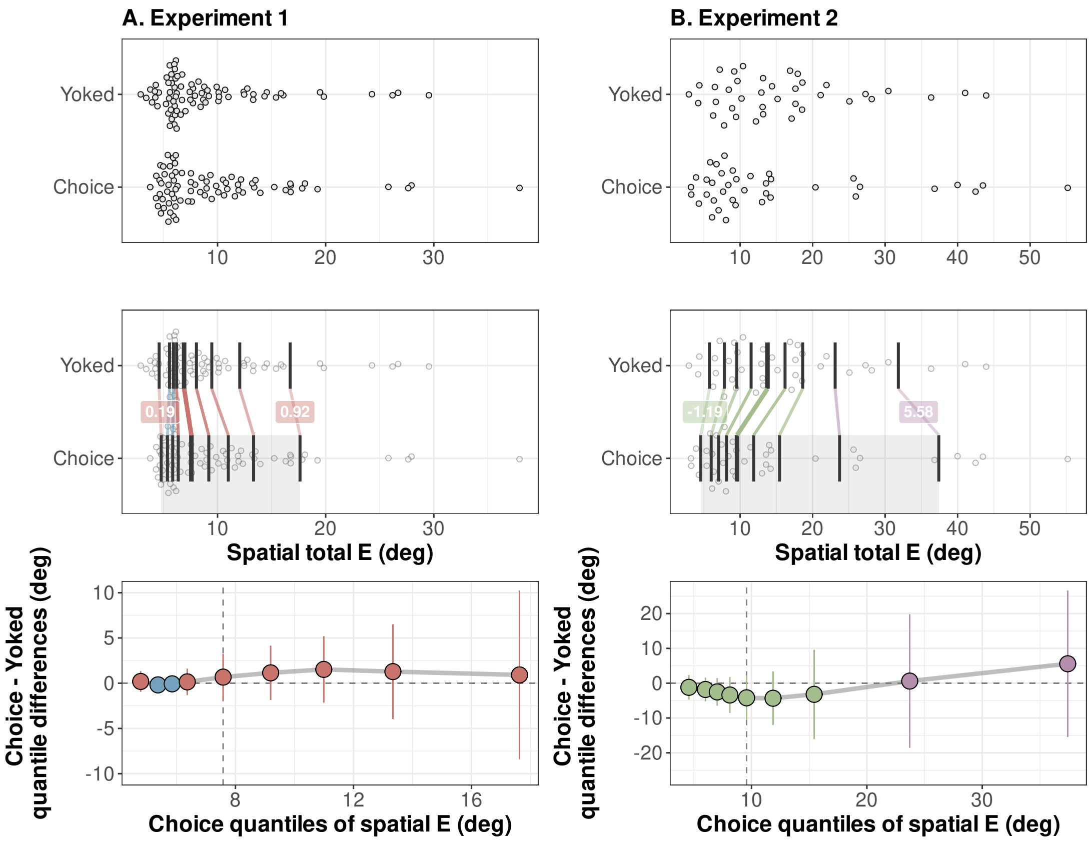

Figure 2.4. Post-test (collapsed across retention and transfer) spatial E (deg) shift function. The top row illustrates scatter plots of individual mean spatial E for each group in Experiment 1 **(A)** and Experiment 2 **(B)**. The middle row illustrates the same scatter plots as the top row, but with the deciles of each distribution represented by the black lines. The thick black line represents the the median of each distribution. The deciles from each group are joined by colored lines, with blue (Experiment 1) and green (Experiment 2) indicating lower error for the choice group deciles, and red (Experiment 1) and green (Experiment 2) indicating lower error for the yoked group deciles. The bottom row illustrates the shift function, which focuses on the grey shaded region of the x-axis in the middle row. The deciles for the choice group are plotted on the x-axis and the difference in deciles between the choice and yoked group are plotted on the y-axis. The vertical dash line represents the median of the choice distribution. The same color coding for differences in deciles from the middle row is used in the bottom row. Error bars represent 95% confidence intervals, corrected for multiple comparisons. The horizontal dashed line represents zero differences between the deciles of the two group. If a 95% confidence interval overlaps with zero, there is no significant difference between the two groups on that decile of the distribution. There were no significant differences for any decile in either experiment.

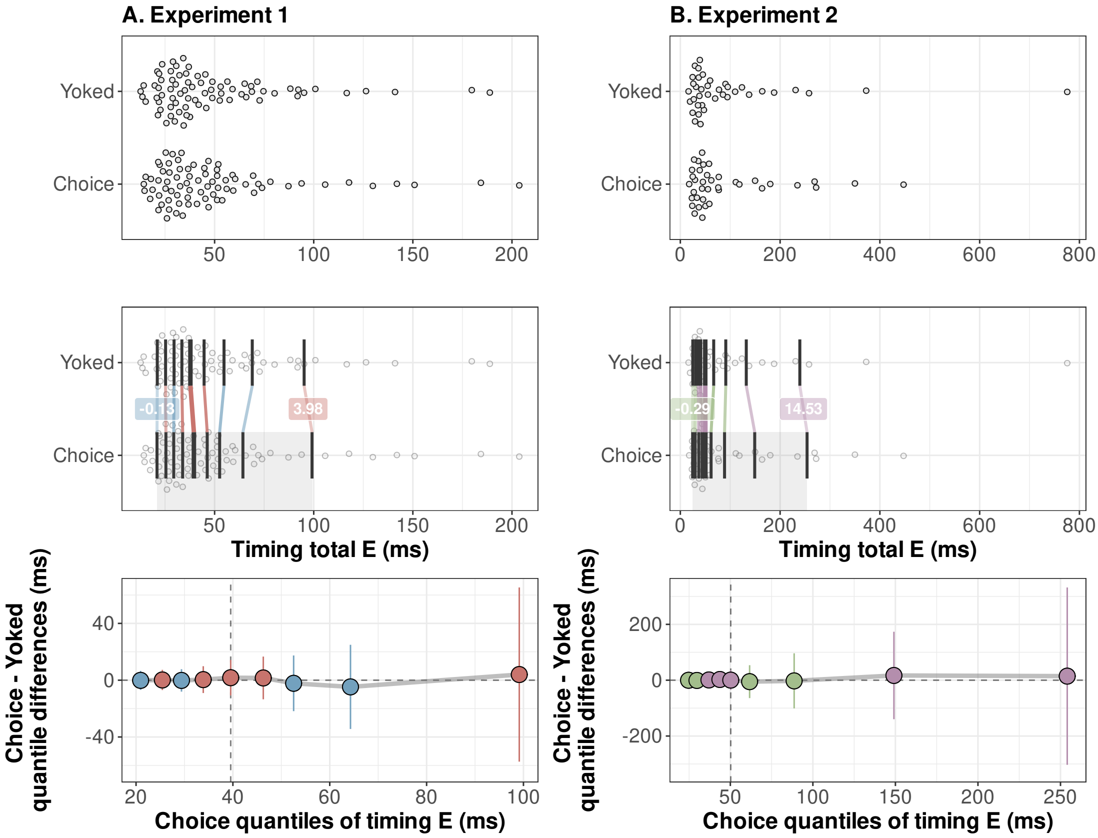

Figure 2.5. Post-test (collapsed across retention and transfer) timing E (ms) shift function. The top row illustrates scatter plots of individual mean timing E for each group in Experiment 1 **(A)** and Experiment 2 **(B)**. The middle row illustrates the same scatter plots as the top row, but with the deciles of each distribution represented by the black lines. The thick black line represents the the median of each distribution. The deciles from each group are joined by colored lines, with blue (Experiment 1) and green (Experiment 2) indicating lower error for the choice group deciles, and red (Experiment 1) and green (Experiment 2) indicating lower error for the yoked group deciles. The bottom row illustrates the shift function, which focuses on the grey shaded region of the x-axis in the middle row. The deciles for the choice group are plotted on the x-axis and the difference in deciles between the choice and yoked group are plotted on the y-axis. The vertical dash line represents the median of the choice distribution. The same color coding for differences in deciles from the middle row is used in the bottom row. Error bars represent 95% confidence intervals, corrected for multiple comparisons. The horizontal dashed line represents zero differences between the deciles of the two group. If a 95% confidence interval overlaps with zero, there is no significant difference between the two groups on that decile of the distribution. There were no significant differences for any decile in either experiment.

2.6 Supplementary 2B

We asked all participants to estimate their performance on the spatial (Fig. 2.6) and timing (Fig. 2.7) components of the motor task after each trial in retention and transfer, similar to past research (e.g., Barros et al., 2019; Carter et al., 2014; Carter & Patterson, 2012). Additionally, half of the participants in each group were randomly selected to also estimate their performance after each trial in the pre-test (not shown). To assess error estimation for the spatial and timing components, we first calculated the difference between a participant's estimated performance and their actual performance on each trial. Next, we computed total estimation error (EE) using the equation:

$$EE = \sqrt{CE^2 + VE^2} \tag{2.2}$$

where CE was the average estimation bias and VE was the standard deviation of these errors. This approach is consistent with that used by Bruechert, Lai, & Shea (2003).

Error estimation in retention and transfer

Total estimation error was generally lower in retention compared to transfer for both the spatial (Fig. 2.6) and timing (Fig. 2.7) domains in Experiments 1 and 2. For the spatial estimation error, we found a significant main effect of Test in Experiment 1, $F(1, 148) = 32.40$, $p < .001$, $\eta^2_G = .060$, and in Experiment 2, $F(1, 74) = 18.79$, $p < .001$, $\eta^2_G = .026$, with more accurate estimations in retention than transfer. Although there was a significant Choice x Feedback x Test interaction in Experiment 1 for timing estimation error, $F(1, 148) = 7.12$, $p = .008$, $\eta^2_G < .001$, we did not decompose the interaction as the effect size estimate was less than .001. There were no significant main effects or interactions for timing estimation error in Experiment 2. A potential explanation for the difference in estimation accuracy between retention and transfer for the spatial component and not the the timing component is that only the spatial goal changed. Specifically, it was 40 deg in retention and 60 deg in transfer whereas the timing goal remained 225 ms in both tests.

Development of error estimation skills

In Experiment 1, the subset of participants in each group who estimated their error after each pre-test trial improved the accuracy of their estimation skills throughout the experiment. We found significant main effects of Test for both the spatial, $F(1.45, 104.67) = 20.72$, $p < .001$, $\eta^2_G = .144$, and timing, $F(1.10, 79.40) = 46.88$, $p <$

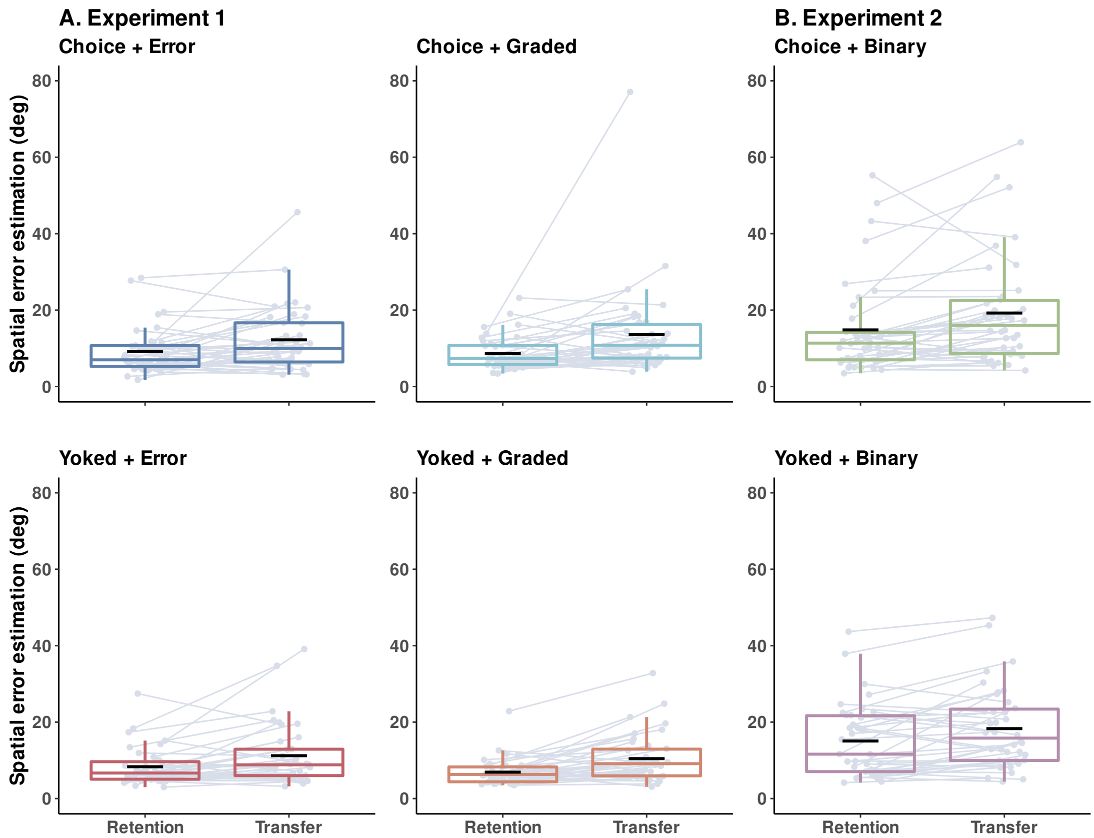

Figure 2.6. Total spatial error estimation data. Boxplots of retention and transfer data from all participants for Experiment 1 **(A)** and Experiment 2 **(B)**. The Choice with error feedback (Choice+Error) group is shown in dark blue, the Choice with graded feedback (Choice+Graded) group is shown in light blue, the Choice with binary feedback (Choice+Binary) group is shown in green, the Yoked with error feedback (Yoked+Error) group is shown in red, the Yoked with graded feedback (Yoked+Graded) group is shown in yellow, and the Yoked with binary feedback (Yoked+Binary) group is shown in purple. Boxplots represent 25th, 50th, and 75th percentiles, and the solid black line denotes the group mean. Grey connected dots represent individual data for participants in each group.

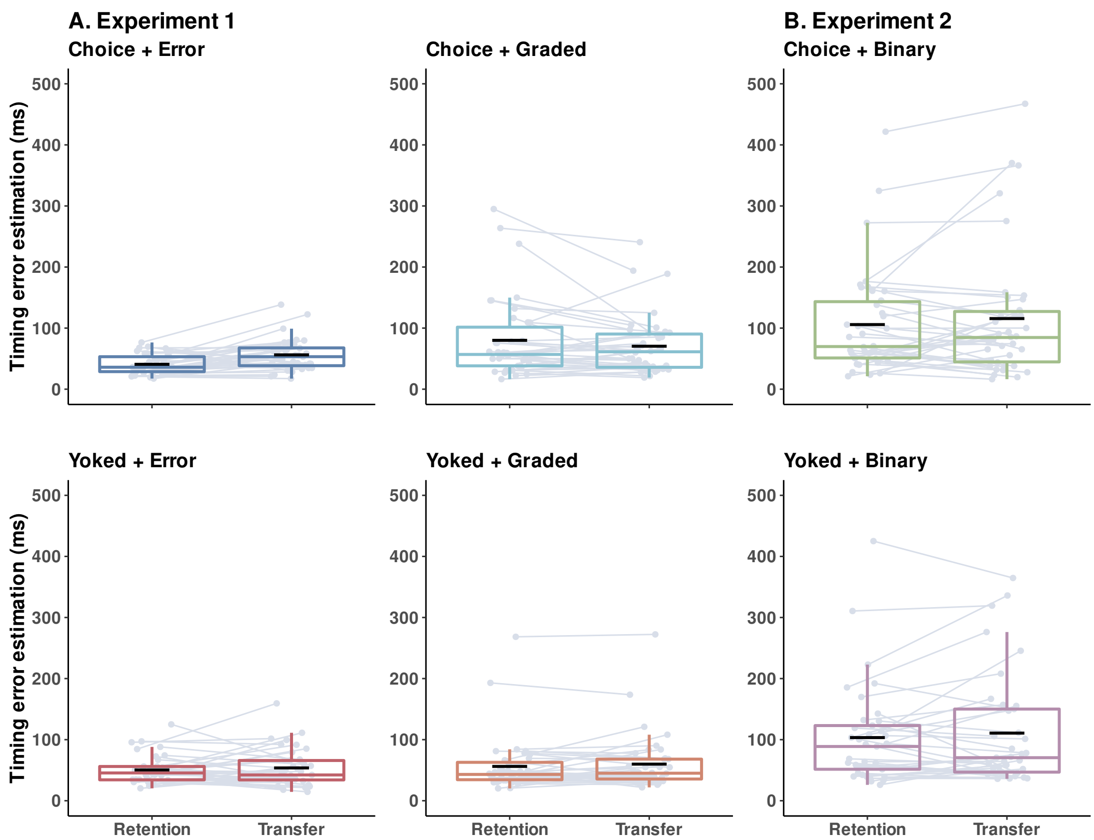

Figure 2.7. Total timing error estimation data. Boxplots of retention and transfer data from all participants for Experiment 1 **(A)** and Experiment 2 **(B)**. The Choice with error feedback (Choice+Error) group is shown in dark blue, the Choice with graded feedback (Choice+Graded) group is shown in light blue, the Choice with binary feedback (Choice+Binary) group is shown in green, the Yoked with error feedback (Yoked+Error) group is shown in red, the Yoked with graded feedback (Yoked+Graded) group is shown in yellow, and the Yoked with binary feedback (Yoked+Binary) group is shown in purple. Boxplots represent 25th, 50th, and 75th percentiles, and the solid black line denotes the group mean. Grey connected dots represent individual data for participants in each group. Individual data of 1 participant in each of the Choice+Error, Yoked+Error, and Yoked+Binary groups is not shown as their error exceeded 500 ms.

.001, $\eta^2_G = .060$, error estimations. For the spatial error estimations, Holm-Bonferonni post-hoc tests revealed that pre-test had higher error than retention and transfer (p's < .001), and transfer had higher error than retention ($p < .001$). Similarly, timing error estimations were more accurate in retention and transfer compared to the pre-test (p's < .001). In Experiment 2, none of the main effects or interactions were significant.

Performance accuracy of estimators versus non-estimators

We assessed whether performance accuracy differed between the subset of participants who were randomly assigned to estimate their error in pre-test compared to those who were not. Total error for the spatial and timing goals were analyzed in separate mixed ANOVAs (*Acquisition*: 2 Estimation x 6 Block; *Learning*: 2 Estimation x 2 Test) for each experiment. None of the main effect or interactions were significant in Experiment 1 or Experiment 2. A possible explanation for this finding is that estimating one's error is most effective when knowledge of results feedback is provided following the estimation (Guadagnoli & Kohl, 2001). Another possible explanation is that self-controlled feedback schedules promote spontaneous error estimation (e.g., Chiviacowsky & Wulf, 2005). Given recent support for this idea (Bacelar, Parma, Cabral, et al., 2022), it seems plausible that all participants in a self-controlled group in the present experiments engaged is some form of error estimation activities throughout acquisition when they deliberated about using (or not using) one of their limited feedback requests.

2.7 Supplementary 2C

Contrary to the motivational perspective (i.e., OPTIMAL theory), we did not find that the opportunity to exercise choice over feedback during practice enhanced perceptions of competence, autonomy, or intrinsic motivation relative to not having this same choice opportunity. Equivalence tests can be used to null findings more informative (Harms & Lakens, 2018; Daniel Lakens, 2017; Schuirmann, 1987); however, a typical two one-sided test procedure may not be appropriate for this analysis for a couple reasons. First, we did not specify an *a priori* smallest effect size of interest for any of the psychological constructs. Second, the questionnaires were administered at various time points—after pre-test, after acquisition blocks 1 and 6, and before retention—during the experimental protocol. Given the choice and feedback manipulations were present at after acquisitions blocks 1 and 6 and not after pre-test or before retention, it would be inappropriate to aggregate across time points. We instead report mean differences and 90% confidence intervals between choice and yoked groups at each questionnaire time point for both experiments. The present experiments can be considered inconsistent with all effects larger than $g = \pm$ the largest absolute confidence interval bound presented in Table 2.3.

Table 2.3. Effect sizes for each questionnaire at each timepoint.

	After pre-test		After block 1		After block 6		Before retention	
Questionnaire	*g*	90% CI	*g*	90% CI	*g*	90% CI	*g*	90% CI
Experiment 1								
Perceived autonomy	.10	[-.17, .36]	.22	[-.05, .49]	.32	[.05, .59]	.28	[.01, .55]
Perceived competence	.07	[-.19, .34]	.10	[-.17, .36]	-.12	[-.38, .15]	-.11	[-.37, .16]
Intrinsic motivation	.27	[0, .54]	.20	[-.07, .46]	.17	[-.09, .44]	.15	[-.11, .42]
Experiment 2								
Perceived autonomy	-.02	[-.29, .24]	.12	[-.15, .39]	.15	[-.11, .42]	-.03	[-.30, .23]
Perceived competence	.12	[-.15, .38]	-.08	[-.35, .18]	.22	[-.05, .49]	.10	[-.16, .37]
Intrinsic motivation	-.31	[-.58, -.04]	-.22	[-.48, .05]	-.20	[-.46, .07]	-.17	[-.43, .10]

Note. Block 1 and 6 are from the acquisition phase. Negative values favor yoked group.

2.8 Bridging summary to Chapter 3

The results of the current experiments wherein a self-controlled learning advantage was not found was surprising given this has been touted a robust effect (for reviews see Sanli et al., 2013; Ste-Marie, Carter, et al., 2020; Wulf, 2007). This lack of replication may have been due to a potential methodological limitation in this chapter, as well as the motor learning literature as a whole. Participants in yoked groups were told they received feedback based on a predetermined schedule and thus were unaware that they were being denied a choice opportunity. Therefore, to further control or thwart perceptions of autonomy, a novel yoked group who was made explicitly aware they are being denied choice opportunities was included in Chapter 3. This allows for a stronger and more thorough (Patall, Cooper, & Robinson, 2008) test of the autonomy pillar of OPTIMAL theory by examining it across three, instead of two as in Chapter 2, different levels: supported (i.e., self-controlled), controlled (i.e., yoked), and thwarted (i.e., explicit yoked). Further, while lab-based aiming tasks can be informative for investigating underlying mechanisms of human behaviour, they have been criticized for being too reductionistic and lacking applicability in real-world learning (Ingram & Wolpert, 2011; Wolpert et al., 2011). To address this potential limitation, the task in Chapter 3 will be an applied task in a lab-based setting to balance ecological validity and experimental control (Haar, Van Assel, & Faisal, 2020; Ranganathan, Lee, & Krishnan, 2022).

Chapter 3

Increased perceptions of autonomy through choice fail to enhance motor skill retention

A version of this chapter won the NASPSPA outstanding student paper award in 2020 and has also been published:

St. Germain, L., Williams, A., Balbaa, N., Poskus, A., Leshchyshen, O., Lohse, K.R., & Carter, M.J. (2022). Increased perceptions of autonomy through choice fail to enhance motor skill retention. *Journal of Experimental Psychology: Human Perception and Performance*, 48(4), 370–379. https://doi.org/10.1037/xhp0000992

3.1 Introduction

A popular recommendation in recent years for creating an effective environment for motor skill learning has been to allow the learner to take control over an element of their practice that is traditionally controlled by a coach, therapist, or teacher (Sanli et al., 2013; Ste-Marie, Carter, et al., 2020). This recommendation is based on the consistent finding that participants in a self-controlled (i.e., choice) group perform with higher proficiency compared to participants in a yoked (i.e., control) group on delayed retention and/or transfer tests. Participants in the yoked group do not experience the same choice opportunity provided to those in the self-controlled group. Instead, they are linked to a self-controlled participant and experience this participant's self-selected practice schedule. This so-called self-controlled learning advantage has been shown when participants are given the opportunity to schedule task difficulty (e.g., Andrieux, Danna, & Thon, 2012; Leiker et al., 2016), the order that multiple tasks are practiced (e.g., Wu & Magill, 2011), the frequency of watching a modeled demonstration (e.g. Wulf et al., 2005), and when to receive augmented feedback (e.g., Janelle et al., 1997; Patterson & Carter, 2010).

Over the years, this manipulation has been described using a variety of names (e.g., self- and/or learner-controlled; -regulated; -directed; -selected), but more recently some researchers have adopted the term autonomy-support. Within their OPTIMAL theory of motor learning, Wulf & Lewthwaite (2016) argued that providing learners with opportunities for choice creates an autonomy-supportive practice environment, which facilitates motor performance and learning. Specifically, the authors predict that autonomy-support facilitates performance by enhancing expectancies (Prediction 3, p. 1404), that enhanced expectancies and autonomy support contribute to efficient goal-action coupling by readying the motor system for task execution (Prediction 2, p. 1404), and that enhanced expectancies and autonomy support facilitate motor learning by making dopamine available for memory consolidation and neural pathway development (Prediction 7, p. 1404). In other words, these psychological benefits of increased perceptions of autonomy and competence, and the resulting increases in performance and learning are a by-product of having choice itself. Overall, Wulf and Lewthwaite's (2016) OPTIMAL theory of motor learning provides a motivational explanation for the learning advantages of self-controlled practice conditions over yoked practice conditions.[1]

[1]It should be noted that other researchers have instead presented an information-processing explanation for the self-controlled learning advantages (see Ste-Marie, Carter, et al., 2020 for a recent discussion of the motivational and information-processing explanations). We acknowledge this view here;

Despite its prominent role as a robust and generalizable learning variable in the OPTIMAL theory (p. 1393), there is considerable ambiguity surrounding whether the provision of choice is in fact an autonomy-supportive manipulation. First, the notion that practicing in a self-controlled group is actually more autonomy-supportive than a yoked group has primarily been assumed (e.g., Abdollahipour et al., 2017; Chua et al., 2018; Lewthwaite et al., 2015; Wulf, Iwatsuki, et al., 2018) rather than supported empirically. Second, when researchers have included measures related to perceptions of autonomy the data is mixed. For example, Ste-Marie et al. (2013) did not find the expected effect of higher perceptions of autonomy during practice in a self-controlled group as compared to a yoked group. Similar outcomes have been reported by others (e.g., Barros et al., 2019; Carter & Ste-Marie, 2017a; McKay & Ste-Marie, 2022). In contrast, McKay & Ste-Marie (2020) recently found that practicing in a self-controlled group was perceived as more autonomy-supportive than practicing the same task in a yoked group. However, despite their higher perceived autonomy scores the self-controlled group did not have significantly better motor performance and learning as predicted in the OPTIMAL theory. Although the majority of experiments that included a measure related to perceived autonomy reported no group differences, the *absence of evidence is not evidence of absence* (Altman & Bland, 1995). Thus, self-controlled practice conditions could be an autonomy-supportive manipulation but such an effect may actually be quite small and require much larger sample sizes to detect than those used in previous experiments (e.g., Barros et al., 2019; Ste-Marie et al., 2013) and in motor learning experiments in general (see Lohse et al., 2016 for a discussion). This argument of underpowered experimental designs is supported by the results of McKay & Ste-Marie (2020) as these authors had one of the largest sample sizes to date in the self-controlled literature.

There are at least two other methodological issues that warrant consideration. In self-controlled motor learning experiments participants in the self-controlled group are usually given choice over a single component (e.g., feedback or when to watch a modeled demonstration) of their practice and participants in the yoked group are not given choice over this component. However, within the context of practice itself there are other opportunities for choice that participants may explore, independent of their assigned group. Past self-controlled learning experiments have used a variety of motor tasks, including but not limited to basketball free throws (e.g., Aiken,

however, unlike previous experiments (e.g., Barros et al., 2019; Carter et al., 2014; Carter & Ste-Marie, 2017a, 2017b; Couvillion et al., 2020; Woodard & Fairbrother, 2020) the current experiment was not designed to test between explanations. We focused on the motivational explanation as this view has garnered more attention due the OPTIMAL theory of motor learning.

Fairbrother, & Post, 2012) and bean-bag tossing (e.g., Grand et al., 2015). Although participants in a yoked group may not have choice over their feedback schedule (or some other practice variable) while learning such tasks, this does not preclude them from opportunities for choice—and thus autonomy-support—when choosing to try different throwing techniques, speeds, and/or release points. Thus, labeling yoked groups as being devoid of choice opportunities may be a misnomer. The other issue relates to the instructions that are provided to participants in a yoked group. In the context of feedback[2], these participants are typically informed that during practice they may or may not receive feedback after a given trial (e.g., Chiviacowsky & Wulf, 2002; Patterson & Carter, 2010). This means that participants in yoked groups are not even aware that they have been denied an opportunity for choice, or that their feedback schedule was created by another participant who had choice over when feedback was or was not provided. These two methodological issues, either in isolation or simultaneously, may have contributed to the consistent finding in past research with self-controlled and yoked participants self-reporting numerically similar perceptions of autonomy when asked about opportunities for choice with respect to the motor task (e.g., Ste-Marie et al., 2013) or about their practice environment in general (e.g., Barros et al., 2019; Carter & Ste-Marie, 2017a; McKay & Ste-Marie, 2022).

Here, we addressed the methodological limitation of past self-controlled research where participants in the yoked group are unaware that a feature of their practice environment was created by another participant in the experiment. To this end, participants learned a speed cup-stacking task in either a self-controlled group, a traditional yoked group, or a novel explicitly aware yoked group. The self-controlled group has choice over the frequency of watching a video demonstration and the video playback speed (real-time or slow motion). Participants in the traditional and explicit yoked groups were matched to a participant in the self-controlled group and experienced the observation schedule selected by this participant. The key difference being that participants in the explicit yoked group were told that the observation schedule they would experience during practice was selected by another participant, whereas participants in the traditional yoked group were not aware of this. Motor learning was assessed using a delayed retention test. We predicted that the self-controlled group would have significantly faster stacking times in retention than the traditional yoked group. In other words, we expected to replicate the typical self-controlled learning advantage. If the self-controlled learning advantage results from choice being autonomy-supportive as

[2]While feedback is used in this example, this issue surrounding instructions is also relevant to other practice variables commonly used in self-controlled learning experiments.

argued in the OPTIMAL theory (Wulf & Lewthwaite, 2016), then participants in the self-controlled group should also self-report significantly higher scores for perceptions of autonomy. Also based on the view that autonomy-support is the mechanism underlying the typical self-controlled learning advantage (Wulf & Lewthwaite, 2016), we predicted that the explicit yoked group should have significantly slower stacking times in retention. Although we found significantly higher perceptions of autonomy in the self-controlled group, there were no significant group differences in stacking times during retention. Overall, these results do not support our predictions and are inconsistent with predictions 2, 3, and 7 in the OPTIMAL theory of motor learning (Wulf & Lewthwaite, 2016).

3.2 Methods

We report how we determined our sample size, all data exclusions (if any), all manipulations, and all measures in the study (Simmons et al., 2012). The experimental design and analyses were preregistered using AsPredicted.org and can be viewed here: https://aspredicted.org/ze8cj.pdf.

3.2.1 Participants

One-hundred and fifty university students participated in the experiment. Sample size was determined by an a priori power calculation based on our smallest comparison of interest using GLIMMPSE (https://glimmpse.samplesizeshop.org/). An early estimate for the effect of self-controlled over yoked practice was a Hedges' $g = 0.63$ (McKay et al., 2014) and while planning this experiment this effect was estimated to be $g = 0.52$ (Z. Yantha, personal communication, October 2019). Based on this effect, a positive correlation of $r = 0.6$ between retention and pre-test as the covariate, an alpha of 0.05, and 80% power to detect a difference between the self-controlled and traditional yoked groups, the required number of participants was 31 per group. Considering the novelty of our Explicit Yoked group, we assumed a smaller effect, $g = 0.4$, between it and the Traditional Yoked group. Using this effect and the same parameters as above, this resulted in our final sample of 50 participants per group.

Participants completed the experiment in either the Self-Controlled group (M_{age} = 18.0, SD = 0.34; 32 females), the Traditional Yoked group (M_{age} = 19.5, SD = 1.89; 28 females), or the Explicit Yoked group (M_{age} = 19.2, SD = 1.55; 30 females). We collected the Self-Controlled group first as their self-selected observation schedule was required for the yoking procedure for the two other groups. Once the Self-Controlled group had been collected, participants were randomly assigned to one of the two Yoked groups. All participants provided written informed consent approved by and conducted in accordance with the University's Research Ethics Board. Participants received either $15 or a course bonus for their participation.

3.2.2 Material

Participants were tasked with learning the 3-6-3 speed cup stacking sequence based on the rules of the World Sport Stacking Association (https://www.thewssa.com/). The sequence consisted of an upstack phase and a downstack phase using official Speed Stacks cups (https://www.speedstacks.com/). Participants performed the task using both their hands and had to complete an upstacking and a downstacking phase. The upstacking phase began by completing the first 3 cup pyramid, followed by the 6 cup pyramid, and then the other 3 cup pyramid. The downstacking phase began by returning to and collapsing the 3 cup pyramid that was upstacked first, then the 6 cup pyramid, and finally the last 3 cup pyramid.

3.2.3 Procedure

Participants completed two data collection sessions separated by approximately 24 hours. Session 1 consisted of a pre-test and an acquisition phase. Session 2 consisted of a delayed retention test. At the start of each phase of the experiment, all participants received phase-specific instructions \hyperref[sec:sharing]{(see Data, materials, and code availability section). Group specific instructions were provided prior to the acquisition phase (see Table 3.1). The instructions appeared on a 22-inch computer monitor (1920x1080 resolution) positioned to the right of the participant. Participants followed along as the instructions were read aloud by the researcher. Each trial began with participants standing at a standard height table with their hands on marked positions on the table in front of them. The 12 cups were located in upside down stacks of 3-6-3 in front of the participant. Following a "Get Ready!" prompt displayed for 1 s on the monitor and a constant foreperiod of 1 s, an audiovisual "Go-signal" (green square and a beep tone) was presented. Participants were instructed to start stacking as quickly as possible following the "Go-signal" as its presentation initiated the timer. Once the upstack and downstack phases were completed, participants were instructed to press the spacebar on a keyboard located in front of them to stop the timer. If an error occurred (e.g., only completed the upstack phase then stopped the timer, forgot to hit the timer, etc), the experimenter recorded the trial number for later removal.

Table 3.1. Group specific components from the instructions detailing the observation schedule that would be experienced during Acquisition.

	Acquisition instructions
Self-Controlled	Before each trial, you will be asked whether you wish to watch a modeled demonstration of the task. If you choose YES, you will then be asked whether you want to watch the video in real-time or in slow-motion.
Traditional Yoked	Before each trial, you may or may not watch a modeled demonstration of the task based on a pre-determined schedule. If you observe a model, it might be presented in real-time or in slow-motion based on the pre-determined schedule.
Explicit Yoked	Before each trial, you may or may not watch a modeled demonstration of the task based on the schedule another participant selected. If you observe a model, it might be presented in real-time or in slow-motion based on what that participant selected.

The pre-test and delayed retention test both consisted of five no-feedback trials. The acquisition phase had 25 trials and was the only phase where the video demonstration could be watched based on group assignment. Participants in the Self-Controlled group could decide at the start of each trial if they wanted to watch the video demonstration. If they chose to watch the video, they were then asked whether they wanted to watch it in real-time or slow motion (35% of real-time). The real-time video was 6 s in duration and the slow motion version was 18 s. To ensure a constant viewing period of 18 s, a blank screen was shown for 12 s following the end of the real-time video. If they chose not to watch the video, a blank screen was shown for 18 s. Participants in the Traditional Yoked and Explicit Yoked groups received the demonstration schedule created by a participant in the Self-Controlled group with the exception that participants in the Explicit Yoked group were made aware that this schedule was created by another participant. Feedback about stacking time (s) was displayed for 2 s after every acquisition trial.

To test predictions based on the OPTIMAL theory regarding the role of motivation, enhanced expectancies, and autonomy-support, participants completed the interest/enjoyment and perceived competence subscales from the Intrinsic Motivation Inventory (McAuley et al., 1989) and a custom scale regarding choice used in previous self-controlled motor learning experiments (Barros et al., 2019; Carter & Ste-Marie, 2017a). The order of questions from each scale were randomized and each question was rated using a 7-point Likert scale. Participants answered these questions after the pre-test, after trials five and 25 of acquisition, and before the delayed retention test. The values for Cronbach's alpha for each questionnaire at each time point are reported in Table 3.2.

Table 3.2. Cronbach's alpha for each questionnaire at each time point.

	After pre-test	After trial 5	After trial 25	Before retention
Perceived autonomy	0.62	0.79	0.79	0.84
Perceived competence	0.90	0.92	0.94	0.92
Intrinsic motivation	0.86	0.90	0.92	0.92

A custom LabVIEW (National Instruments Inc.) program was created that controlled the presentation of all instructions, the video demonstrations, the timing of the experimental protocol, and recorded and stored the data for later analysis.

3.2.4 Data analysis

Our primary outcome measure was stacking time (i.e., response time) in seconds. Trials recorded as errors ($76/5250 = 1.45\%$) during data collection were manually removed prior to data analysis. For each participant, pre-test and delayed retention trials were aggregated into one block of five trials and acquisition was aggregated into five blocks of five trials. Significance level was set to 0.05 for all statistical tests. Effect sizes for omnibus tests are reported using generalized eta squared (η_G^2) or eta squared (η^2). Post hoc comparisons were conducted using Holm's correction. A Cook's distance of ≥ 1 was used to identify any influential cases and none were identified. Statistical tests are described below.

3.3 Results

3.3.1 Pre-registered analysis

To test whether delayed retention (Figure 3.1C) was differentially impacted by the experimental group experienced during acquisition, we performed a one-way ANCOVA controlling for pre-test. As can be seen, the Self-Controlled group ($M = 9.99$, $95\%CI = [9.68, 10.31]$), the Traditional Yoked group ($M = 10.18$, $95\%CI = [9.86, 10.50]$), and the Explicit Yoked group ($M = 10.12$, $95\%CI = [9.81, 10.44]$) all had similar stacking times in retention (means are shown as the adjusted means controlling for pre-test). The effect of Group was not significant, $F(2, 146) = .335$, $p = .716$, $\eta^2 = .002$.

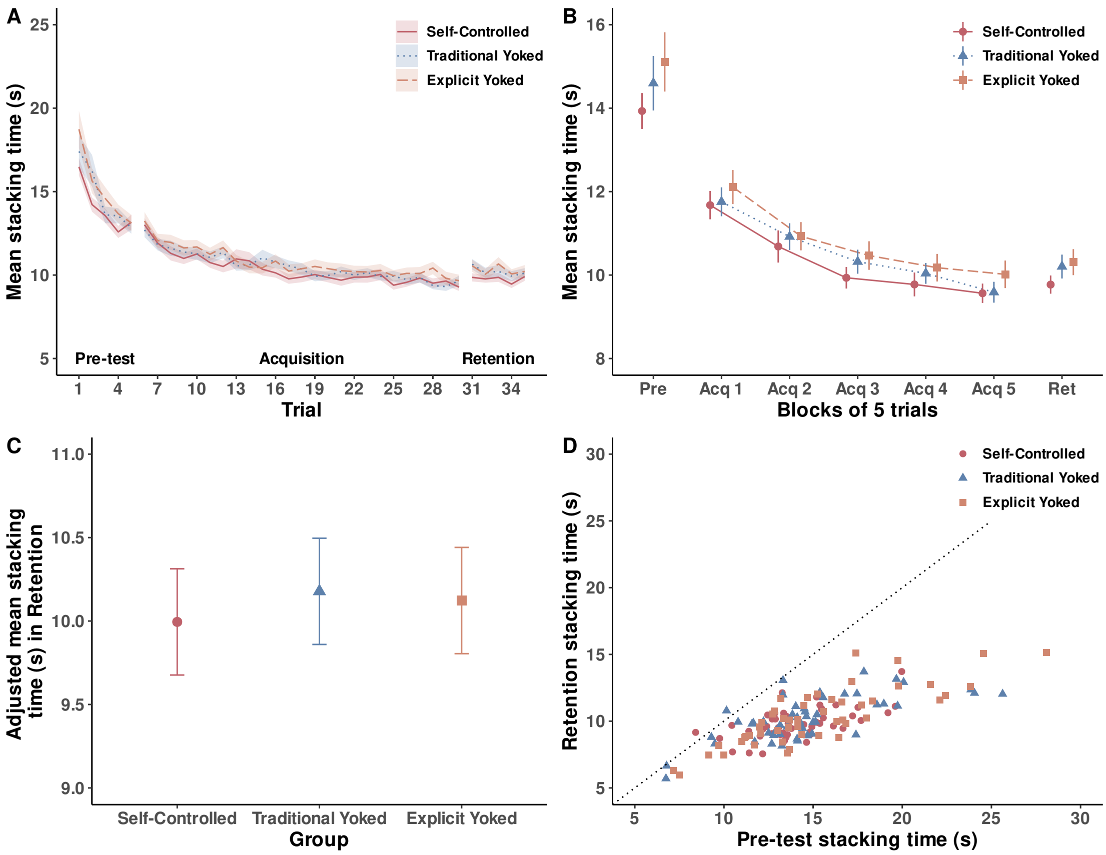

Figure 3.1. Physical performance data of the experiment. The Self-Controlled group is shown in red with solid lines and/or circles, the Traditional Yoked group is shown in blue with dotted and/or triangles, and the Explicit Yoked group is shown in orange with dashed and/or squares. **(A)** Trial-by-trial stacking time (s) data averaged across participants within each group (shaded area represents standard error). Splits between line segments denote the end of one phase and the start of the next. Pre-test (Trials 1 to 5) and Acquisition (Trials 6 to 30) occurred on Day 1 and Retention (Trials 31 to 35) occurred on Day 2, approximately 24-hours later. **(B)** Mean stacking time (s) for each group was computed by averaging the data from **(A)** into 7 blocks of trials. Error bars represent 95% confidence intervals. Splits between line segments denote the end of one phase and the start of the next. **(C)** Mean stacking time (s) adjusted for pre-test for each group. Error bars represent 95% confidence intervals. **(D)** Scatterplot showing the relationship between each participant's mean stacking time (s) in pre-test and retention.

Table 3.3. The mean frequencies for each Acquisition block of requesting to watch the video demonstration and of those requests, the mean frequency of choosing the regular viewing speed.

	Block 1	Block 2	Block 3	Block 4	Block 5
Model viewing frequency	32.6%	16.9%	12.0%%	12.9%	9.3%
Regular speed frequency	33.8%	38.1%	50.0%	34.4%	30.4%

3.3.2 Non pre-registered analyses

Traditional self-controlled learning advantage

To investigate whether a traditional self-controlled learning advantage existed—that is a comparison between the Self-Controlled and Traditional Yoked groups—we analyzed the non-adjusted retention scores using a Welch's t-test. The analysis revealed that the Self-Controlled group ($M = 9.77$, $95\%CI = [9.55, 9.99]$) and the Traditional Yoked group ($M = 10.20$, $95\%CI = [9.92, 10.49]$) were not statistically different, $t(88.10) = 1.45$, $p = .15$, $g = .29$.

Acquisition phase

Model frequency and the video speed request data are reported in Table 3.3. The mean frequency of viewing the video demonstration during acquisition was 16.7%. When a video demonstration was selected, the mean frequency that the regular speed was selected was 37.3%. Stacking time for each Group decreased across acquisition trials (Figure 3.1A) and blocks (Figure 3.1B). A 3 Group x 5 Block mixed ANOVA with repeated measures on Block revealed a significant Block effect, $F(3.63, 533.79) = 116.73$, $p < .001$, $\eta^2_G = .133$. Post hoc comparisons revealed all acquisition blocks were significantly different from each other. Both the Group effect, $F(2, 147) = .738$, $p = .48$, $\eta^2_G = .008$, and Group x Block interaction, $F(7.26, 533.79) = .543$, $p = .802$, $\eta^2_G = .001$, were not statistically significant.

Perceived autonomy

Self-reported perceived autonomy scores are displayed in Figure 3.2A where it can be seen that the Self-Controlled group reported slightly higher scores compared to the two Yoked groups at all time points following the pre-test. A 3 Group x 3 Time ANCOVA controlling for pre-test revealed a significant Group effect, $F(2, 146) = 8.04$, $p < .001$, $\eta^2_G = .083$. Post hoc comparisons on the adjusted means revealed that the Self-Controlled group ($M = 5.61$, $95\%CI = [5.42, 5.80]$) had significantly higher perceptions of autonomy compared to both the Traditional Yoked group ($M = 5.13$, $95\%CI = [4.94, 5.32]$) and the Explicit Yoked group ($M = 5.13$, $95\%CI = [4.94, 5.32]$), which did not differ from each other (and were, in fact, identical to two decimal places). Both the Time effect, $F(1.61, 234.98) = .867$, $p = .400$, $\eta^2_G = .001$, and Group x Time interaction, $F(3.22, 234.98) = 2.10$, $p = .096$, $\eta^2_G = .005$, were not significant.

Intrinsic motivation

Self-reported intrinsic motivation scores can be found in Figure 3.2B. At each time point, all groups reported similar scores on the interest/enjoyment subscale. A 3 Group x 3 Time ANCOVA controlling for pre-test revealed non-significant effects for Group, $F(2, 146) = 2.08$, $p = .129$, $\eta^2_G = .024$, Time, $F(1.68, 245.73) = .563$, $p = .541$, $\eta^2_G = .001$, and the Group x Time interaction, $F(3.37, 245.73) = .333$, $p = .824$, $\eta^2_G = .001$.

Perceived competence

The scores from the perceived competence subscale at each time point are displayed in Figure 3.2C. As can be seen, perceived competence scores showed a modest increase over time followed by a slight decrease before the delayed retention test. A 3 Group x 3 Time ANCOVA controlling for pre-test showed an effect of Time, $F(1.75, 255.22) = 10.60$, $p < .001$, $\eta^2_G = .015$. Post hoc tests on adjusted means revealed that scores were rated as significantly higher at the end of acquisition ($M = 3.98$, $95\%CI = [3.67, 3.89]$) and before retention ($M = 3.78$, $95\%CI = [3.67, 3.89]$) compared to after trial 5 ($M = 3.60$, $95\%CI = [3.49, 3.71]$). However, perceived competence was significantly lower before retention compared to the end of acquisition. No significant effect of Group, $F(2, 146) = 2.65$, $p = .074$, $\eta^2_G = .028$, or Group x Time interaction, $F(3.50, 255.22) = .549$, $p = .677$, $\eta^2_G = .002$, was found.

Equivalence tests

Given our pre-registered analysis resulted in a non-significant finding, we performed two equivalence tests (Self-Controlled versus Traditional Yoked and Traditional Yoked versus Explicit Yoked) on pre-test adjusted retention stacking times using the two one-sided test procedure (Daniel Lakens, Scheel, & Isager, 2018; Schuirmann, 1987). Equivalence tests are a statistical tool that researchers can use to support the absence of a meaningful effect and avoid incorrectly concluded no effect exists based on a non-significant finding (Harms & Lakens, 2018). To establish our smallest effect size of interest (Daniel Lakens, Pahlke, & Wassmer, 2021) we calculated the effect size our design had 33% power to detect

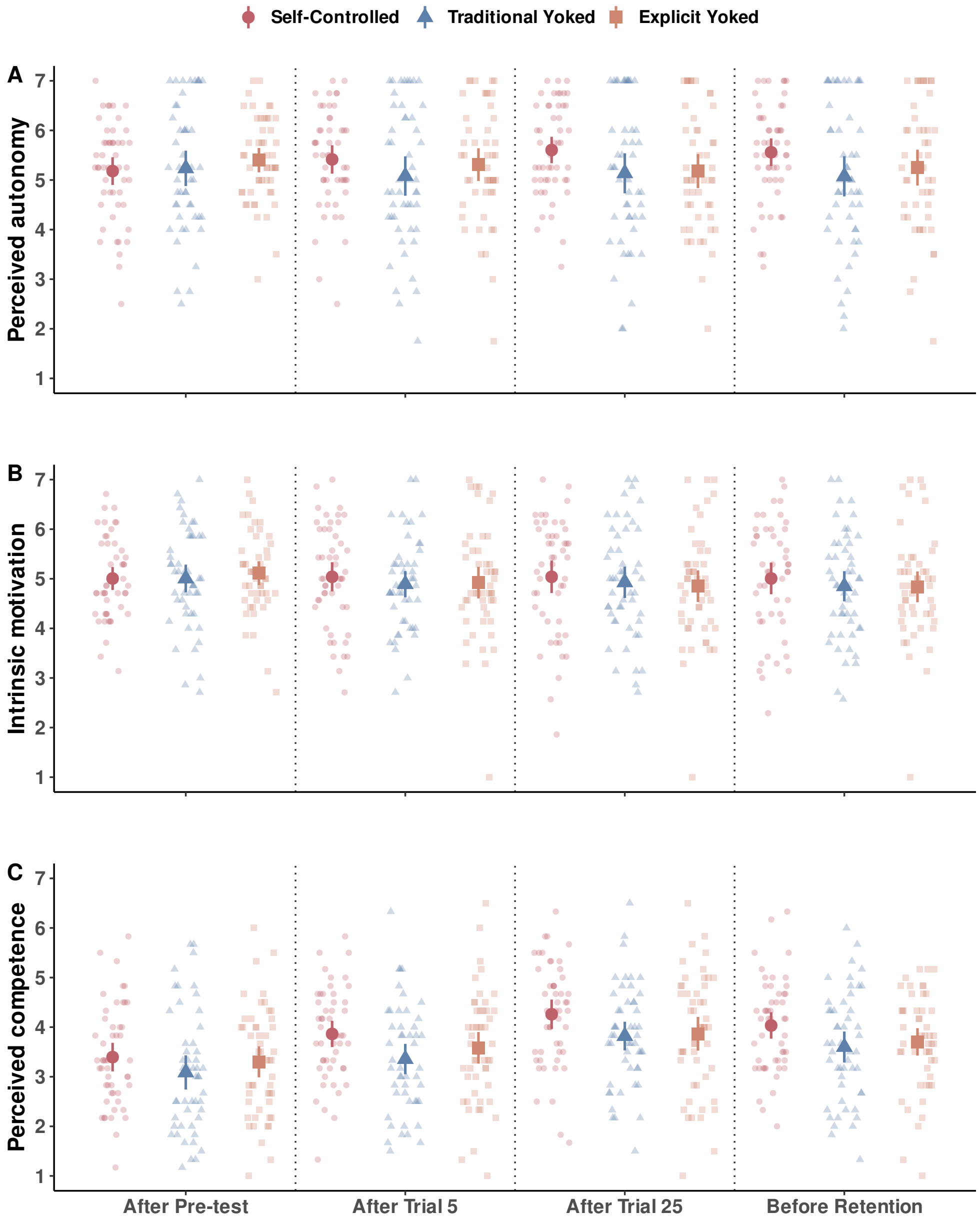

Figure 3.2. Questionnaire data. Groups means for **(A)** perceived autonomy, **(B)** intrinsic motivation, and **(C)** perceived competence after pre-test, trial 5, and trial 25 on Day 1, and before retention on Day 2. The Self-Controlled group is shown in red circles, the Traditional Yoked group is shown in blue triangles, and the Explicit Yoked group is shown in orange squares. Dots represent individual participants in each group. Scores could range from 1 (Strongly disagree) to 7 (Strongly agree). Error bars represent 95% confidence intervals.

(Simonsohn, 2015), which was $d_s = .31$. The Self-Controlled and Traditional Yoked groups were not statistically different, $t(97.99) = -0.830$, $p = 0.409$, and not statistically equivalent, $t(97.99) = 0.720$, $p = 0.237$, given equivalence bounds of $-.355$ and .355 (on a raw scale). Similarly, the Traditional Yoked and Explicit Yoked groups were not statistically different, $t(97.99) = 0.262$, $p = 0.794$, and not statistically equivalent, $t(97.99) = -1.288$, $p = 0.100$, given equivalence bounds of $-.355$ and .355 (on a raw scale).

3.4 Discussion

It has been argued that self-controlled practice conditions are effective for motor learning because they are autonomy-supportive in nature (Wulf & Lewthwaite, 2016). Yet, this claim has received, at best, modest support in the motor learning literature (e.g., McKay & Ste-Marie, 2020; but see Barros et al., 2019; Carter & Ste-Marie, 2017a; Ste-Marie et al., 2013 for non-support). Here, we addressed a possible methodological limitation for this lack of support. That is, in previous self-controlled motor learning research, participants in the yoked group are not made aware that they have been denied opportunities to exercise choice over a practice variable such as feedback or watching a modeled demonstration. In the present experiment we introduced a novel yoked group that was explicitly told that the observation schedule—frequency and speed of video—they would receive during practice was actually one created by another participant in the experiment. Contrary to our prediction, the Explicit Yoked group did not report significantly lower perceived autonomy scores than the Traditional Yoked group, which was only informed their observation schedule was predetermined. In line with the OPTIMAL theory (Wulf & Lewthwaite, 2016), the Self-Controlled group reported significantly higher perceived autonomy-support than both the Traditional and Explicit Yoked groups. However, this boost in perceived autonomy did not lead to enhanced motor performance or learning. These findings from a preregistered experiment are difficult to reconcile with a core pillar of the OPTIMAL theory of motor learning.

3.4.1 Self-controlled practice conditions as autonomy-supportive

A common problem with many of the self-controlled or choice motor learning literature claiming autonomy-support as an underlying mechanism for self-controlled learning advantages is a failure to include measures that actually test this claim (e.g., Chiviacowsky, 2014; Chiviacowsky, Wulf, & Lewthwaite, 2012; Lewthwaite et al., 2015). Instead, this link to autonomy-support is merely assumed based on the provision of choice to one group of participants versus not giving the same choice to another group of participants. As noted earlier, an issue with this assumption is that within the practice environment itself, there are myriad opportunities for participants, independent of group, to exercise choice. In throwing tasks common to motor learning, participants can explore the task workspace by attempting different throwing techniques. In more lab-based tasks that consist of multiple spatial and timing goals (e.g., waveform matching), participants can choose a single goal to focus on and master before shifting their attention to another goal. Commensurate with this idea, Ste-Marie et al. (2013) found no differences in perceived choice regarding the motor task being learned between their self-controlled and yoked groups, despite the self-controlled group showing enhanced retention. This may explain why research has consistently reported perceived autonomy scores that do not differ significantly between self-controlled and yoked groups (Barros et al., 2019; Carter & Ste-Marie, 2017a; McKay & Ste-Marie, 2022).

Another possible explanation for the finding that being in a self-controlled group has not been perceived as more autonomy-supportive than being in a yoked group is that choice is typically provided over a single element of the practice variable, such as the frequency of receiving feedback (e.g., Aiken et al., 2012; Carter & Ste-Marie, 2017b) or the level of task difficulty (Andrieux et al., 2012; Leiker et al., 2016). Control or choice over a single dimension may not be strong enough to elicit a large enough boost in perceived autonomy above and beyond that of being able to explore one's task workspace as mentioned above. Thus, to increase the saliency of the self-controlled manipulation in the present experiment, we gave the self-controlled participants control over two elements of their observation schedule: viewing frequency and video playback speed. Having control of these two dimensions resulted in higher perceptions of autonomy in our Self-Controlled group compared to the Traditional and Explicit Yoked groups. This may explain the inconsistency with past research failing to find this effect (Barros et al., 2019; Carter & Ste-Marie, 2017a; McKay & Ste-Marie, 2022; Ste-Marie et al., 2013). However, McKay & Ste-Marie (2020) recently found that choice over a single element increased perceived autonomy, but similar to our data they also did not find this led to increased learning. Thus, it is unclear whether the higher autonomy scores in the present experiment can in fact be attributed to having control over multiple elements of one's observation schedule.

In a similar vein, past research has also suggested that self-controlled learning benefits are not dependent on the amount of choice opportunities over a single element (Patterson et al., 2011). Given these findings, we argue that the most likely explanation for the inconsistency surrounding the effect of self-controlled practice being autonomy-supportive is that this effect is quite small, and the designs of previous experiments have lacked the statistical power to reliability detect this effect. These experiments have had sample sizes of 20 participants or less per group, whereas there were 50 participants in each group in the present experiment and 64 per group (when collapsed across constant and variable practice schedules) in McKay & Ste-Marie (2020). Collapsing across our two yoked groups, we estimate an effect of autonomy-support of $g = .197$ whereas the estimate from McKay & Ste-Marie (2020) was $g = .57$. Regardless of the estimated size of the effect for autonomy-support through a self-controlled practice condition, a larger issue and challenge to the OPTIMAL theory is that significantly higher perceptions of autonomy did not result in superior performance in either acquisition or delayed retention (Wulf & Lewthwaite, 2016).

3.4.2 No learning advantage from a self-controlled observation schedule

The delayed retention results in the present experiment not only are inconsistent with numerous past experiments reporting a self-controlled motor learning benefit (for a review see Ste-Marie, Carter, et al., 2020), but also the general consensus that the self-controlled learning advantage is a robust effect (Wulf & Lewthwaite, 2016). Of late, however, there has been an increase in the number of papers reporting a failure to detect the so-called self-controlled learning advantage (e.g., Barros et al., 2019; Grand et al., 2017; McKay & Ste-Marie, 2020, 2022). This recent lack of support may arise from the self-controlled learning advantage being a much smaller effect than originally estimated (g = 0.63 by McKay et al. 2014). A more recent estimate of this effect from a meta-analysis using a weight-function model was $g = .11$, $95\% CI = [.047, .18]$ (McKay, Yantha, et al., 2022). This estimate was further reduced to $g = .054$ after controlling for publication bias using the precision-effect estimate with standard error (PEESE) method. Thus, both models seem to suggest a trivial effect for self-controlled learning. Additional simulations included in the meta-analysis provided plausible effect size estimates ranging from $g = -.11$ to .26 (i.e., it could even favour being in the yoked group). In our experiment, the estimated effect size of self-controlled versus yoked (collapsed across our Traditional and Explicit Yoked groups) was $g = 0.22$. While this estimate is larger than those from the weight-function model and PEESE method, it does fall within the range of plausible effect sizes (McKay, Yantha, et al., 2022). When the lack of a self-controlled advantage in previous work and our current experiment are contextualized within the findings of this recent meta-analysis, it is not as surprising that the so-called self-controlled learning benefit was not replicated.

Despite the large sample size of the present experiment (n/group = 50) versus the typical self-controlled motor learning experiment (median n/group = 18 based on the experiments included in the meta-analysis by McKay et al. 2021), the results of the primary analysis and the equivalence tests remain inconclusive. This, along with the effect size estimates in the recent meta-analysis, suggest enormous sample sizes are required to reliably detect an effect of a self-controlled learning advantage. Using the upper bound, $g = .26$, of the range of plausible effects, *253 participants* are required to have 80% power to detect this effect in retention using an independent-samples t-test. The number of participants jumps to *1300 per group*, if for instance the estimate from the weight-function model, $g = .11$, is accurate (McKay, Yantha, et al., 2022). Given the field of motor learning suffers from a lack of adequately powered designs (Lohse et al., 2016) and that underpowered designs are more likely to produce false positives and overestimated effect sizes (Button et al., 2013), we are skeptical of the replicability of the previous overwhelming support for the self-controlled learning advantage.

3.4.3 Lack of support for key predictions of the OPTIMAL theory of motor learning

Within the OPTIMAL theory (Wulf & Lewthwaite, 2016), it is predicted that practice conditions that promote autonomy-support and enhance expectancies contribute to a virtuous cycle that leads to superior motor performance and learning. In other words, significant group differences between self-controlled and yoked groups would be expected on measures related to these psychological constructs. As mentioned earlier, we found higher perceptions of autonomy-support in our Self-Controlled group relative to the Traditional and Explicit Yoked groups; however, this did not enhance motor learning as predicted within the virtuous cycle. Contrary to the OPTIMAL theory, we did not find an effect of group for perceived competence (i.e., enhanced expectancies) or intrinsic motivation. Perceived competence scores did increase across acquisition blocks, which mirrors the improved motor performance seen throughout acquisition. These results are in line with results of a path analysis that suggested self-efficacy (i.e., enhanced expectancies) and intrinsic motivation were insufficient to explain self-controlled learning benefits (Ste-Marie, Carter, Law, Vertes, & Smith, 2016). Overall, our results are inconsistent with key predictions of the OPTIMAL theory regarding autonomy-support and enhanced expectancies, and further question the causal role such manipulations play in motor learning.

In sum, we found that being able to control two aspects of one's observation schedule during practice was perceived as autonomy-supportive relative to not having this same control opportunity. However, making participants in an Explicit Yoked group aware they would be denied a control opportunity during practice did not decrease perceptions of autonomy relative to participants in the Traditional Yoked group. Furthermore, we did not find evidence than an autonomy-supportive manipulation had any direct causal effect on learning and we also failed to replicate the typical self-controlled learning advantage. It is worth noting that in some situations autonomy-support in and of itself might be a desired affective outcome (Ste-Marie, Lelievre, & St. Germain, 2020). In such circumstances, providing learners with choice opportunities may be a useful motivationally-protective outcome. However, such a tool should be used judiciously as it is typically motor outcomes in the form of relatively permanent performance changes that is the true goal of any motor learning intervention (Ste-Marie, Lelievre, et al., 2020). Ultimately, our results add to a growing body of evidence that questions whether autonomy-supportive manipulations directly affect motor learning. One explanation for this lack of replicability is that the *"true"* effect of such manipulations are much smaller than previously estimated (McKay, Yantha, et al., 2022). Given the resources required (in terms of sample size) to reliably detect these tiny effects, we encourage motor learning scientists to invest their limited resources carefully—either by adopting the use of sequential analyses (Daniel Lakens, 2014; Daniel Lakens et al., 2021; Wald, 1945) and/or multi-lab collaborations (Boland, Karczewski, & Tatonetti, 2017) or studying practice conditions that likely have much bigger effects on motor learning.

3.5 Bridging summary to Chapter 4

The results of the current experiment demonstrated that choice was perceived as autonomy-supportive. These higher perceptions of autonomy in the self-controlled group, however, did not translate into improved performance or learning compared to either yoked group. Given the failure to replicate a self-controlled learning advantage across three consecutive experiments in this dissertation, other experiments with large sample sizes also failing to find this effect (Bacelar, Parma, Cabral, et al., 2022; Leiker et al., 2019; McKay, Hussien, et al., 2022; McKay & Ste-Marie, 2020; Yantha et al., 2022), and a very small meta-analytic estimate of the size of the self-controlled learning advantage (McKay, Yantha, et al., 2022), further investigation the choice over practice conditions leg of the autonomy branch of OPTIMAL theory will be abandoned. Instead, autonomy will be investigated through instructional language in Chapter 4 as it has received minimal attention in the motor learning literature and is the final leg standing of the autonomy pillar of OPTIMAL theory.

Chapter 4

Autonomy-supportive instructional language does not enhance skill acquisition compared to controlling instructional language

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A version of this chapter has been pre-printed and is also currently under review:

St. Germain, L., McKay, B., Tandon, C., Seedu, J., Barbera, L., Carrillo, C., Brown, D.M.Y., & Carter, M.J. (2023). Autonomy-supportive instructional language does not enhance skill acquisition compared to controlling instructional language. *Sportχiv*. https://doi.org/10.51224/SRXIV.298

4.1 Introduction

Autonomy support refers to a teaching style or approach that fosters self-determination and intrinsic motivation in learners by providing them with choices, respect, and opportunities to make decisions. In Self-Determination Theory (Deci & Ryan, 2012; Ryan & Deci, 2020), autonomy is broadly defined as the sense of ownership and initiative over one's behaviors. Within the Basic Psychological Needs Theory (Ryan & Deci, 2000a, 2017) of Self-Determination Theory, humans have inherent psychological needs for autonomy, competence, and relatedness. When these needs are satisfied, individuals experience a range of positive outcomes such as enhanced performance, increased intrinsic motivation, and are more likely to engage in activities with a greater sense of interest, enjoyment, and well-being. Indeed, autonomy support has been shown to be efficacious in a variety of contexts, including educational psychology (see Reeve, 2009; Ryan & Deci, 2020 for reviews; and see Su & Reeve, 2011 for a meta-analysis), coaching (see Mossman et al., 2022 for a meta-analysis), and health (see Okada, 2021 for a meta-analysis). During the last decade, motor learning scientists have become increasingly interested in the use of autonomy-supportive practice conditions for skill acquisition (see Sanli et al., 2013; Ste-Marie, Carter, et al., 2020; Wulf & Lewthwaite, 2016 for reviews).

The prevailing autonomy-supportive manipulation in the motor learning literature is providing learners with opportunities for choice either before or during practice (for a review see Ste-Marie, Carter, et al., 2020). In the OPTIMAL theory of motor learning, Wulf & Lewthwaite (2016) highlighted two ways to support autonomy through choice: *control over practice conditions* (i.e., task-relevant choices) and *incidental choices* (i.e., task-irrelevant choices). The dominant view for nearly 25 years has been that both choice manipulations are effective for skill acquisition (e.g., Carter et al., 2014; Carter & Ste-Marie, 2017b; Chiviacowsky & Wulf, 2002, 2005; Lewthwaite et al., 2015; Wulf, Iwatsuki, et al., 2018; Wulf, Freitas, et al., 2014); commonly referred to as the self-controlled learning advantage. Recently, however, this so-called self-controlled learning advantage has failed to be replicated in several large N—and often pre-registered—experiments (Bacelar, Parma, Cabral, et al., 2022; Leiker et al., 2019; McKay, Hussien, et al., 2022; McKay & Ste-Marie, 2020; St. Germain et al., 2022, 2023; Yantha et al., 2022). A recent meta-analysis found that estimates of the self-controlled learning effect could range from g = -0.11 to 0.26 after correcting for publication bias (McKay, Yantha, et al., 2022). McKay, Corson, et al. (2023) re-analyzed this meta-analysis using a robust Bayesian approach (Bartoš, Maier, Wagenmakers, Doucouliagos, & Stanley, 2023; Maier, 2023) and found the overall model ensemble estimated the effect as $d = .034$ (95% credible interval [.0, .248]). Taken together, these studies suggest that the true effects of these choice manipulations for motor learning are uncertain, small, and potentially null. Wulf & Lewthwaite (2016) also highlighted instructional language as a third practice variable that can be manipulated to enhance learning through autonomy-support. Yet the wording of such task instructions has received minimal attention in the motor learning literature to date.

The language used in task instructions exists on a continuum ranging from highly controlling to autonomy-supportive (Reeve, 2009). Factors that contribute to autonomy-supportive instructions are prioritization of the learner's perspective and goals, openness to learner initiative, and support for learner self-direction (Reeve, 2009; Reeve & Tseng, 2011). Reeve & Tseng (2011) provided participants with either autonomy-supportive, neutral, or controlling instructions about how to solve near-unsolvable puzzles. Despite performance on the puzzles being the same between groups (i.e., the puzzles were not solved), the controlling language group had the lowest perceptions of autonomy while the autonomy-supportive group had the highest perceptions of competence. Thus, autonomy-supportive instructions can exert affective benefits even when performance gains are not possible. Hooyman et al. (2014) extended this work to the motor learning literature by providing participants with either autonomy-supportive, controlling, or neutral instructions about how to perform a modified cricket bowl to a target. Compared to the controlling language group, the autonomy-supportive group performed with less error in practice and in a delayed retention test, and also had higher ratings for perceived choice, self-efficacy, and positive affect at the end of practice.

Although the results of Hooyman et al. (2014) suggested a motor performance and learning benefit of autonomy-supportive language, there are some methodological limitations that warrant consideration. First, the autonomy-supportive instructions were confounded with an analogy; previously shown to also facilitate motor performance and learning (e.g., Liao & Masters, 2001; see Masters et al., 2020 for a review). As the analogy was not part of the controlling or neutral language instructions, it is impossible to disentangle whether the benefits in the autonomy-supportive group resulted from the instructional language, the analogy, or some combination of the two. Second, the authors' measure of perceived choice to capture autonomy-support does not comprehensively map onto the basic needs of Self-Determination Theory (McDonough & Crocker, 2007; Ng, Lonsdale, & Hodge, 2011; Ryan & Deci, 2020) and has been shown to be a poor indicator of self-determination and intrinsic motivation (Reeve, Nix, & Hamm, 2003). Lastly, the experimental design was underpowered for all but the largest plausible effect sizes. With 16 participants per group, the main effect of Group at retention would only be able to detect f of 0.4 (equivalent to d of .8 for a t-test) with 80% power. Such an effect is considerably higher than an estimate of the median effect size in motor learning studies ($d = 0.63$, Lohse et al., 2016). Further, when underpowered designs find significant results, they are prone to be false positives with inflated effect size estimates (Button et al., 2013; Simmons et al., 2011).

In the present experiment, we investigated the effects of autonomy-supportive language on motor performance and learning while addressing the above mentioned methodological limitations of Hooyman et al. (2014). Participants practiced a speed cup stacking task and received instructions with either autonomy-supportive or controlling language. We also asked participants to self-report their perceptions of autonomy, competence, and intrinsic motivation at multiple time points in the experiment. Based on OPTIMAL theory (Wulf & Lewthwaite, 2016), we predicted that participants in the autonomy-supportive language group would demonstrate faster stacking times in practice and retention, and report higher perceptions of autonomy, competence, and intrinsic motivation compared to the participants in the controlling language group.

4.2 Methods

We report how we determined our sample size, all data exclusions (if any), all manipulations, and all measures in the study (Simmons et al., 2012). All data and `R` scripts can be accessed here: https://github.com/cartermaclab/expt_instructional-language and the pre-registration can be accessed here: https://doi.org/10.17605/osf.io/9n46p.

4.2.1 Sample size calculation

To test our primary prediction that autonomy-supportive instructional language would enhance motor skill retention compared to controlling instructional language, we performed a two-stage *a priori* power analysis using the smallest effect size of interest approach (see Daniël Lakens, 2022 for a discussion). We specified our smallest effect size of interest as $d = 0.4$. This is a conservative estimate compared to an estimate of the median effect size in the motor learning literature ($d = 0.63$ in Lohse et al., 2016), a meta-analytic estimate of the effect size of autonomy-supportive instructional language

($d = 0.63$ in Su & Reeve, 2011), and has been suggested as a reasonable smallest effect size of interest for psychological research (Brysbaert, 2019).

In the first stage, we used a one-sided Welch's t-test with the following parameters: $\alpha = 0.05$, $\beta = 0.20$, and $d = 0.4$, resulting in 78 participants per group for a total of 156 participants. In the second stage, we used a shift function, which is a family of robust statistical techniques for comparing entire distributions (Rousselet et al., 2017; Rousselet & Wilcox, 2020; Rand R. Wilcox, 2021; Rand R. Wilcox & Rousselet, 2023). It is therefore a useful alternative to comparisons based on means as effects can, and do, occur in the tails of distributions as well. In other words, the shift function is a powerful tool to determine how, and by how much, two distributions differ (Rousselet et al., 2017; Rand R. Wilcox, 2021). For this power analysis, we simulated right-skewed distributions with $n = 78$ per group and a mean difference of 0.4. Right-skewed distributions were used because time based quantities are typically asymmetric (see Rousselet & Wilcox, 2020 for a discussion) and our primary outcome variable was stacking time.

We performed 10,000 simulated experiments using both a one-sided Welch's t-test and a shift function to determine which statistical analysis should be used to test our primary prediction. The t-test had 80% power (consistent with that from the first stage) whereas the shift function had 88% power. As such, the shift function was selected as our primary analysis. We include the t-test as a secondary analysis for the interested reader.

4.2.2 Participants

A convenience sample of undergraduate and graduate students at a university in southwestern Ontario were recruited from undergraduate courses and through word of mouth during the Fall 2022 semester. Participants were randomly assigned to either the autonomy-supportive instructional language group ($M_{\text{age}} = 18.7$ years, $SD = 1.85$, range = 18 to 27, $n = 78$, 54 females) or the controlling language group ($M_{\text{age}} = 18.9$ years, $SD = 2.39$, range = 18 to 29, $n = 78$, 52 females). Participants were compensated $15 CAD or with course-credit for their time. All participants gave written informed consent and the experiment was approved by McMaster University's Research Ethics Board.

4.2.3 Task and apparatus

Participants were tasked with learning the 3-6-3 speed cup stacking sequence in accordance with the rules of the World Sport Stacking Association (https://www.th ewssa.com/). Official Speed Stack cups (https://www.speedstacks.com/) were used and participants performed the task using both of their hands. To successfully complete the 3-6-3 sequence, participants performed an upstack phase and a downstack phase. The cups began in upside down piles consisting of three, six, and three cups from left to right. The upstack phase required participants to create a 3-cup pyramid, followed by a 6-cup pyramid, then another 3-cup pyramid. The down stack phase consisted of collapsing the first 3-cup pyramid from the upstack phase, then the 6-cup pyramid, and then the remaining 3-cup pyramid so the cups were in the same configuration as the start of the task. The goal of the task was to perform the upstack and downstack phases as fast as possible.

4.2.4 Procedure

Data collection involved two sessions that occurred on consecutive days. Session 1 consisted of obtaining informed consent, a demographics questionnaire, the pre-test (5 no-feedback trials), an acquisition phase (30 trials with feedback), and questionnaires related to three psychological constructs. Session 2 consisted of the same three questionnaires and the delayed (~24 hours) retention test (5 no-feedback trials). Participants completed both sessions of the experiment individually. Participants received phase-specific instructions using neutral language at the start of each experimental phase. Group-specific instructions were also provided at the start of acquisition (see below for details). Instructions were displayed on a 22-inch computer monitor (1920 x 1080 resolution) positioned to the left of the participant.

At the start of each trial, participants stood at a standard height table with their hands on marked locations and the 12 upside down cups arranged in the 3-6-3 configuration on the table in front of them. Participants were shown a “Get Ready!” prompt on the computer monitor for 1 s. After a 1 s constant foreperiod, an audiovisual go-signal (green square and a beep tone) was presented. Participants were instructed to begin the upstack phase as quickly as possible following the go-signal as its presentation initiated the timer. Once the upstack and downstack phases were completed, participants hit the spacebar on a keyboard located in front of them to stop the timer. If an error occurred (e.g., forgot to hit the spacebar to stop timer, only completed the upstack phase then stopped the timer, etc.), the researcher recorded the trial number for later removal. Prior to the pre-test, participants received neutral language instructions that described the cup stacking task and the pre-test protocol. Included in these instructions were two videos from the Speed Stacks website. The first was a demonstration of how to perform the 3-6-3 sequence and the second described what to do if any cups were knocked over

Table 4.1. Cronbach's alpha for each questionnaire at each timepoint.

Questionnaire	After pre-test	After acquisition	Before retention
Perceived autonomy	0.79	0.83	0.85
Perceived competence	0.79	0.86	0.88
Intrinsic motivation	0.88	0.91	0.92

during the upstack and/or downstack phases. The pre-test consisted of five trials with no feedback regarding their stacking time. After the pre-test, participants completed three questionnaires related to key psychological variables in OPTIMAL theory (Wulf & Lewthwaite, 2016). Perceived autonomy and perceived competence were assessed using the Basic Needs Satisfaction in Sport Scale, which has been shown to have good reliability and construct validity (Ng et al., 2011). The perceived competence subscale has 5 items, for example "I feel I am good at this task". The perceived autonomy subscale has 10 items to capture choice (4 items, e.g., "In this study, I get opportunities to make choices"), an internal perceived locus of causality (3 items, e.g., "In this study, I feel I am pursuing goals that are my own"), and volition (3 items, e.g., "I choose to participate in this study according to my own free will"). Intrinsic motivation was assessed using the Task Interest and Enjoyment subscale (7 items, e.g., "This cup stacking task was fun to do") of the Intrinsic Motivation Inventory (McAuley et al., 1989). For all questions, participants read a statement on a handout and then verbally reported their answer using a Likert scale ranging from 1 (not true at all) to 7 (very true). Answers were recorded by a trained research assistant and stored for later analysis. Cronbach's alpha values for each questionnaire at each time point are reported in Table 4.1.

Before the acquisition phase, participants received neutral language instructions that described the cup stacking task and the acquisition protocol. The acquisition phase consisted of 30 trials and feedback about stacking time was displayed for 2 s after every trial. Prior to acquisition trials 1, 11, and 21, participants received group-specific instructions based on whether they were randomly assigned to the autonomy-supportive language group or the controlling language group (see Table 4.2). The group-specific instructions were pre-recorded and played as an audio clip to participants. This was done to ensure that instruction delivery was consistent across participants, and to eliminate potential confounds such as differences in tone, pace, number of spoken words, amount of eye contact, etc. Our group-specific instructions were reviewed and revised based on feedback from an expert in autonomy-supportive instructional language

Table 4.2. Group specific instructions received during acquisition before trials 1, 11, and 21.

Group	Acquisition instructions
Autonomy-support	Are you ready to learn how to cup stack? Does this sound like an activity you might want to try? It will probably be helpful if you think of the task as a challenge and consider a goal to complete it as quickly as possible. To help, I'll offer some hints here at the beginning. You have probably already noticed that I've put the cups in three stacks. It might be helpful to arrange them in order from left to right, with three cups on the left, six in the middle, and three on the right. You might be thinking that the best way to complete the task is to upstack the cups from left to right, then return to the beginning and also downstack from left to right. If this is what you're thinking, you are right! I understand that you might feel a little hesitant and unsure. Most people feel this way, at least at first. You are free to begin when you wish.
Controlling	Your job is to learn cup stacking - perform it well and do it as quickly as possible. To do so, do what I tell you to do. Don't begin yet, listen carefully to me. Make sure the stacks are in their proper order. I want the stacks in order from left to right, with three cups on the left, six in the middle, and three on the right. Make sure the stacks are in their proper order. If so, good. If not, fix it. When completing the task, I want you to upstack the cups from left to right, then return to the beginning to also downstack from left to right. If you're thinking of doing it differently - don't, that is not what I told you to do. Begin.

(Dr. Johnmarshall Reeve, personal communication, April 20 2022).[1] After all acquisition trials were finished, participants completed the three questionnaires a second time.

Participants returned to the lab for Session 2 approximately 24 hours after finishing Session 1. Upon arrival, participants completed the three questionnaires for a third and final time. Before performing the delayed retention test, participants received neutral language instructions that described the cup stacking task and the retention protocol. The retention test consisted of five trials with no feedback regarding their stacking time.

A custom LabVIEW (National Instruments Inc.) program was created that controlled the presentation of instructions, the timing of experimental protocol, and recorded and stored the data for offline analysis.

[1]The reviewed instructions referred to a floor curling task rather than the cup stacking task that was ultimately used in the experiment.

4.2.5 Data analysis

Outcome variables

Our primary outcome variable was stacking time, which was the interval between the go-signal and the participant hitting the spacebar. Trials recorded as an error (122/6240, 1.96%) during data collection were manually removed before data analysis. Stacking time was calculated as the mean for blocks of five trials, resulting in one pre-test block, six acquisition blocks, and one delayed retention block. Perceived autonomy and perceived competence scores were respectively calculated as the mean of the 10 autonomy items and six competence items from the Basic Needs Satisfaction in Sport Scale. Intrinsic motivation was calculated as the mean of the seven items of the Task Interest and Enjoyment subscale from the Intrinsic Motivation Inventory.

Primary analysis

We performed a shift function on mean stacking time in retention, adjusted for pre-test scores, to test our primary prediction that autonomy-supportive instructional language would enhance learning compared to controlling instructional language. For the shift function we first calculated the 20% trimmed means for each participant. We then regressed retention stacking time onto pre-test stacking time (i.e, using pre-test as a covariate). Next, we computed deciles using the Harrell-Davis estimator (Harrell & Davis, 1982) and 95% confidence intervals around each decile were calculated using percentile bootstraps. Corrected p-values using Hochberg's method (Hochberg, 1988) were calculated for each decile. The shift function was considered significant if any of the corrected p-values was $\leq .05$.

Secondary analyses

We analyzed mean stacking time during the acquisition period using a 2 Group (Autonomy-support, Controlling) x 5 Block mixed design ANOVA with repeated measures on Block. We assessed the impact of our instructional language manipulation on perceived autonomy, perceived competence, and intrinsic motivation using separate 2 Group (Autonomy-support, Controlling) x 2 Time (After acquisition, Before retention) mixed ANCOVAs with pre-test scores as the covariate and repeated measures on Time.

Alpha was set to .05 for all statistical analyses. Corrected degrees of freedom using the Greenhouse–Geisser method are always reported when appropriate. Generalized eta squared (η_G^2) is reported as an effect size for all omnibus tests. Post hoc comparisons were adjusted for multiple comparisons using the Holm–Bonferroni correction.

4.3 Results

4.3.1 Primary analysis

Pre-test adjusted stacking time data for each participant in the autonomy-supportive language and controlling language groups are shown in Figure 4.1A. The shift function (Fig. 4.1C) comparing the groups at each decile (Fig. 4.1B) is relatively flat in the middle (deciles 3 through 6), but has a negative slope; indicating that the two distributions differ in their spread (Rousselet et al., 2017). The largest differences between the groups were in the first and last deciles. The controlling language group was faster than the autonomy-supportive group in the first decile whereas the autonomy-supportive language group was faster in the last decile. None of the decile comparisons were significant after p-values were adjusted for multiple comparisons.

4.3.2 Secondary analyses

Acquisition phase

Participants in the autonomy-supportive language and controlling language groups decreased their stacking time across acquisition blocks (Fig. 4.2). This was supported by a significant main effect of Block, $F(4.40, 677.96) = 111.91$, $p < .001$, $\eta_G^2 = .137$. Stacking time in Block 1 was slower than all other blocks ($p's < .001$), Block 2 was slower than Blocks 3 to 6 ($p's \leq .004$), and Blocks 3 and 4 were both slower than Blocks 5 and 6 (p's $< .001$). The main effect of Group, $F(1, 154) = 2.20$, $p = .140$, $\eta_G^2 = .011$, and the Group x Block interaction, $F(4.40, 677.96) = 0.59$, $p = .681$, $\eta_G^2 < .001$, were not significant.

Retention test

Performance in the delayed retention test was also analyzed using a more common approach in motor learning research. A one-tailed Welch's t-test on pre-test adjusted mean stacking times for the Autonomy-supportive language group ($M = 9.86$ s, $SD = 0.80$) and the Controlling language group ($M = 10.04$ s, $SD = 1.37$) was not significant, $t(124.51) = 1.00$, $p = .159$, $d = .16$ $[-.156, .477]$.

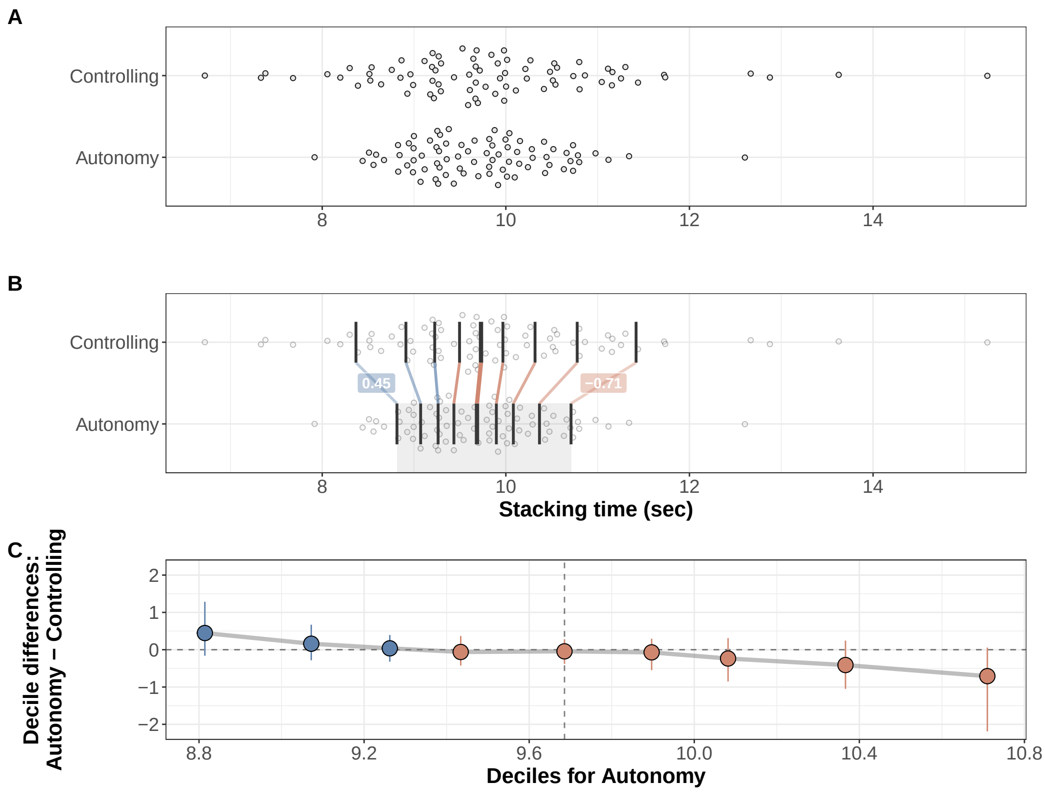

Figure 4.1. Shift function on retention stacking times adjusted for pre-test times. Scatterplot of stacking time as a function of experimental group **(A)** with each data point representing a 20% trimmed mean for an individual participant. The same scatterplot from the top row with the deciles if each distribution represented by the black lines **(B)**. The thick black line represents the median of each distribution. The difference between groups at each decile are represented by the colored lines. A blue line indicates that the Controlling language group was faster in a decile and an orange line indicates that the Autonomy-supportive language group was faster in a decile. The bottom row illustrates the shift function **(C)**, which focuses on the grey shaded region of the x-axis in the middle row. The deciles for the Autonomy-supportive language group are plotted on the x-axis and the difference in deciles between the two groups are plotted on the y-axis. The vertical dashed line represents the median of the Autonomy-supportive language group. The circles represent the decile differences using the same color coding described above. Error bars represent 95% percentile bootstrapped confidence intervals. All decile comparisons were not significant after p-values were adjusted for multiple comparisons using Hochberg's method.

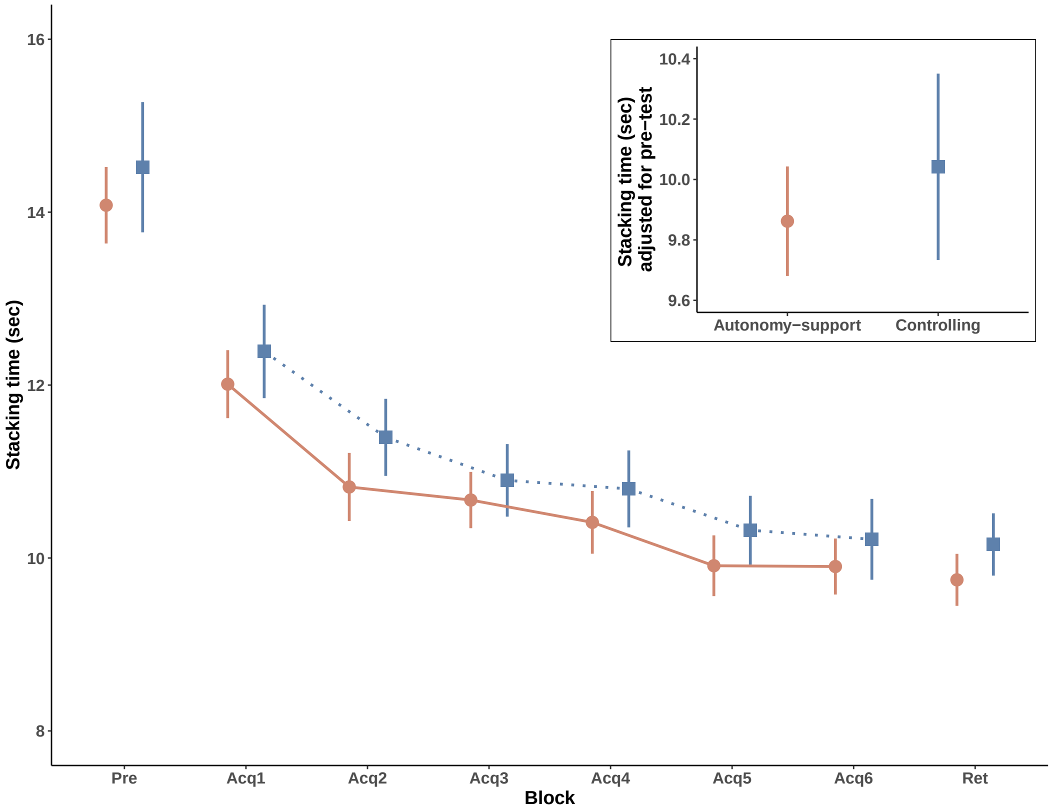

Figure 4.2. Motor performance data for all experimental phases. Mean stacking time (s) for the Autonomy-supportive language (orange circles, solid line) and Controlling language (blue squares, dotted line) groups were computed by averaging the data into blocks of five trials. This resulted in one block for pre-test (Pre), six block for acquisition (Acq), and one block for the ~24-hr delayed retention test (Ret). The pre-test and acquisition blocks were completed in Session 1 and the retention block was completed in Session 2. Feedback about stacking time (s) was only available during the acquisition blocks and was provided after each trial. Group-specific instructions as a function of experimental group were played as pre-recorded audio clips before trials 1 (start of Block 1), 11 (start of Block 3), and 21 (start of Block 5) in acquisition. The inset figure shows pre-test adjusted retention stacking time (s) for both groups. Error bars in both figures represent 95% confidence intervals.

4.3.3 Psychological variables

Perceptions of autonomy (adjusted for pre-test) remained consistent within both groups (Fig. 4.3A). Pre-test, the covariate, was a significant predictor of later time points, $F(1, 153) = 467.970$, $p < .001$. Participants in the autonomy-supportive language group self-reported higher scores than the participants in the controlling language group after acquisition and before retention. This was supported by a significant main effect of Group, $F(1, 153) = 4.40$, $p = .037$, $\eta^2_G = .020$, revealing that our instructional language manipulation worked as intended. The main effect of Time, $F(1, 153) = 0.48$, $p = .490$, $\eta^2_G < .001$, and the Group x Time interaction, $F(1, 153) < 0.01$, $p = .982$, $\eta^2_G < .001$, were not significant.

Perceptions of competence (adjusted for pre-test) were relatively consistent in both groups (Fig. 4.3B). Pre-test was a significant predictor of later time points, $F(1, 153) = 399.13$, $p < .001$. The autonomy-supportive language group had higher perceptions of competence after acquisition compared to the controlling language group. Interestingly, from after acquisition to before retention the autonomy-supportive language group showed a slight decrease in perceived competence whereas the controlling language group had a slight increase. The significant main effects for Group, $F(1, 153) = 5.89$, $p = .016$, $\eta^2_G = .029$, and Time, $F(1, 153) = 8.19$, $p = .004$, $\eta^2_G = .011$, were superseded by a significant Group x Time interaction, $F(1, 153) = 5.13$, $p = .025$, $\eta^2_G = .039$. The interaction resulted from the autonomy-supportive language group having higher perceptions of competence than the controlling language group after acquisition ($p < .001$), but not before retention ($p = .830$).

Intrinsic motivation (adjusted for pre-test) scores remained consistent within both groups (Fig. 4.3C). Pre-test was a significant predictor of later time points, $F(1, 153) = 630.89$, $p < .001$. Participants in the controlling language group had slightly higher scores after pre-test and before retention compared to participants in the autonomy-supportive group whereas the reverse was shown after the acquisition phase. This pattern of results was supported by a significant Group x Time interaction, $F(1, 153) = 6.04$, $p = .015$, $\eta^2_G = .010$. The autonomy-supportive language group reported significantly lower intrinsic motivation scores before retention than after acquisition ($p = .007$), and no significant differences at any time point in the controlling language group (p's $> .9$).

We also performed some exploratory correlational analyses between our three psychological variables and performance in retention. We plotted pre-test adjusted retention stacking times as a function of perceived autonomy (Fig. 4.3D), perceived

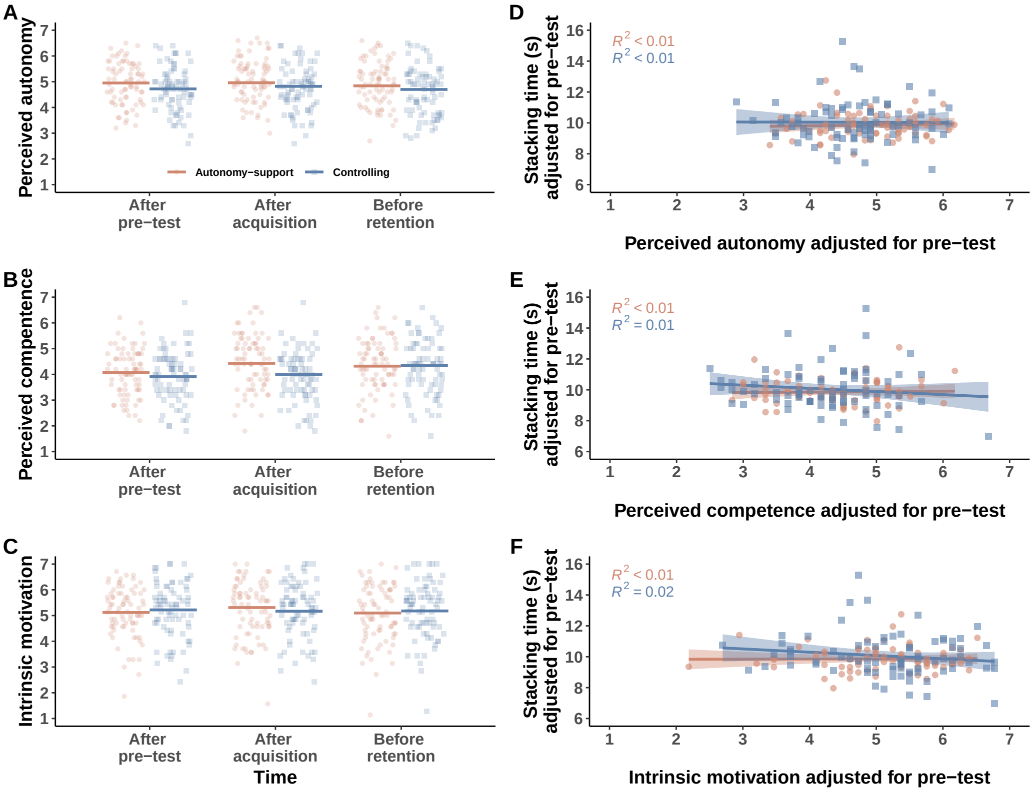

Figure 4.3. Questionnaire data. Self-reported scores for perceived autonomy **(A)**, perceived competence **(B)**, and intrinsic motivation **(C)** after the pre-test, after the acquisition phase, and before the delayed retention test for the Autonomy-supportive language (orange circles) and the Controlling language (blue squares) groups. The horizontal bars represent the group means, with the pre-test adjusted mean shown for after acquisition and before retention. Each data point represents the mean score across subscale items for an individual participant. The relationship between retention stacking time (s) adjusted for pre-test and perceived autonomy **(D)**, perceived competence **(E)**, and intrinsic motivation **(F)** before retention and adjusted for pre-test is shown. Each data point represents the mean score across subscale items for an individual participant in the Autonomy-supportive language (orange circles) and the Controlling language (blue squares) groups. The estimated regression fit (solid lines) for each group is shown. The shaded areas represent the 95% confidence intervals. A negative slope in these plots would suggest faster stacking times were associated with higher self-reported scores on the psychological variable of interest.

competence (Fig. 4.3E), and intrinsic motivation (Fig. 4.3F) scores before retention, adjusted for pre-test for each participant. If there were associations between these psychological variables and performance in retention, we expected to see a negative relationship (i.e., faster stacking times associated with higher self-reported scores). As can be seen in each figure, we instead found no relationship between retention performance and our psychological variables.

4.3.4 Equivalence test

Due to the null findings of the shift function and t-test on retention stacking times, we tested for equivalence with a noninferiority test as outlined in our pre-registration. Specifically, we used the two one-sided test procedure (Schuirmann, 1987) and a noninferiority bound of $d = .4$, which was our smallest effect size of interest. The test was non-significant, $t(124.5) = 1.50$, $p = .069$. The 90% confidence interval around the effect size in retention was [-.11, .43], indicating that these data are inconsistent with all effects larger than $d = \pm .43$.

4.4 Discussion

In their OPTIMAL theory of motor learning, Wulf & Lewthwaite (2016) suggested that motor performance and learning can be enhanced when learners receive task instructions that use autonomy-supportive rather than controlling language. In the present experiment we investigated the effect of autonomy-supportive instructional language on the acquisition and retention of a speed cup stacking task. Based on OPTIMAL theory, we predicted that participants in the autonomy-supportive language group would demonstrate faster stacking times in acquisition and delayed retention, and would also report higher perceptions of autonomy, competence, and intrinsic motivation compared to those in the controlling language group. Our results did not show a performance benefit for the autonomy-supportive language group in acquisition or retention compared to the controlling language group. We found significantly higher perceptions of autonomy in the autonomy-supportive language group compared to the controlling language group. After the acquisition phase, participants in the autonomy-supportive language group reported higher perceived competence than the controlling language group, but this effect did not persist before the delayed retention test. No significant group differences were found for intrinsic motivation. Taken together, our findings do not support key predictions of the OPTIMAL theory of motor learning (Wulf & Lewthwaite, 2016).

We failed to replicate the performance advantage of autonomy-supportive language in acquisition and delayed retention compared to controlling language that was reported by Hooyman et al. (2014). This is also inconsistent with Wulf and Lewthwaite's (2016) OPTIMAL theory wherein task instructions that utilize autonomy-supportive language results in a virtuous cycle that has positive influences on motor performance and learning. Importantly, our failed replication and lack of support for OPTIMAL theory are not the result of participants failing to improve at the motor task or an unsuccessful instructional language manipulation. That is, both the autonomy-supportive language and controlling language groups showed a decrease in stacking times from pre-test to the delayed retention test, suggesting learning occurred (see Fig. 4.2) and the autonomy-supportive language group reported higher perceptions of autonomy (see Fig. 4.3A). These conflicting findings may be due to the previously identified methodological limitation in Hooyman et al. (2014) of a small sample size (e.g., Button et al., 2013; Lohse et al., 2016) or potential flexibility in the data analysis (e.g., Simmons et al., 2011) as their experiment was not pre-registered (e.g., Munafò et al., 2017). Although such factors may have contributed, we believe the main reason for our discrepant results arise from the confounding analogy included in Hooyman and colleagues' (2014) autonomy-supportive instructions, but excluded from both their controlling and neutral language instructions. It is therefore possible that their autonomy-supportive language advantage was *actually* an analogy advantage (e.g., Liao & Masters, 2001; Masters et al., 2020). This possibility clearly highlights the importance of carefully crafting instructions that only differ in terms of the primary predictor variable of interest—instructional language—in future research.

Despite having the largest sample size in an instructional language motor learning experiment to date, the results of our robust shift function, a more traditional t-test, and non-inferiority test were inconclusive. Using our smallest effect size of interest, $d = .4$, as the noninferiority bound, the effect size at delayed retention in the present experiment is inconsistent with all effects larger than $d = \pm.43$. Although this is bigger than our pre-registered smallest effect size of interest, this test would reject the median effect size previously found in motor learning research ($d = .63$ by Lohse et al., 2016). As such, future research investigating the impact of instructional language on motor skill acquisition likely requires larger sample sizes than that used in the present experiment and what is commonly found in motor learning research (e.g., Lohse et al., 2016; McKay, Corson, et al., 2023).

When examining the stacking time distributions for each group in retention (see Fig.

4.1A), it is clear that the spread of the data in the two distributions is different. Such differences can be masked when researchers solely rely on standard summary statistics such as the mean (see Anscombe, 1973 for the famous Anscombe's quartet example). Although all adjusted decile comparisons in our primary shift function analysis were not significant, there were some interesting trends in this analysis that could have theoretical and/or practical significance for future work. Specifically, there was a trend for better performance with controlling language for the participants who were in the fastest (i.e., more skilled) stacking time decile (unadjusted $p = .051$) and a trend for better performance with autonomy-supportive language for the participants who were in the slowest (i.e., less skilled) stacking time decile (unadjusted $p = .017$). This patterns suggests that the motor learning benefits of different instructional language wording may potentially interact with skill level; however, a large N experiment would be required to test this hypothesis. If this hypothesis could be empirically supported, it would be incompatible with OPTIMAL theory as Wulf & Lewthwaite (2016) predicted that autonomy-supportive instructional language is beneficial irrespective of skill level. A possible explanation for why less skilled individuals could benefit from autonomy-supportive instructions compared to more skilled individuals benefiting from controlling language instructions is that the former may act as a buffer against poor performance by allowing learners to persevere and remain engaged in the task during practice. Thus, future work in this area should consider including behavioural, neural, and/or psychological measures related to task engagement (e.g., Fairclough, Ewing, & Roberts, 2009; Leiker et al., 2016; O'Brien & Toms, 2009). More generally, motor learning scientists may want to consider leveraging modern and robust statistical tools in their work as these techniques may provide greater insight and a more nuanced understanding of their data (Rousselet et al., 2017; Rand R. Wilcox, 2021).

Despite the prominent role of autonomy-support facilitating motor performance and learning in OPTIMAL theory, the higher perceptions of autonomy in our autonomy-supportive language group (see Fig. 4.3A) did not translate into superior performance in either acquisition or retention compared to the controlling language group. The higher reported perceived autonomy scores in the autonomy-supportive language group serves as a strong manipulation check and is also consistent with findings from previous research (e.g., Reeve & Tseng, 2011) and multiple meta-analyses (e.g., Mossman et al., 2022; Ng et al., 2012; Okada, 2021; Su & Reeve, 2011). However, our estimate of this effect on perceptions of autonomy is much smaller than previous estimates. The estimated size of the effect in the present experiment is $d = .34$ [.03, .66], which is outside the 95% confidence interval around Su and Reeve's (2011) estimate of d = .63 [.43, .83]. A potential explanation for our smaller estimate is that Su & Reeve (2011) identified five components that can make instructions autonomy-supportive: 1) use non-controlling language, 2) acknowledge negative feelings, 3) nurture inner motivational resources, 4) provide meaningful rationales, and 5) offer choices; and many of the experiments in their meta-analysis included either four or all five components. In contrast, our instructions only included the first three components. This suggests that not all components may be necessary to have a positive influence on perceptions of autonomy. However, the strength of the effect may scale with the number of components incorporated in the instructions and this may be important for seeing differences in motor performance and learning. Future research would be needed to test this possibility. Another possibility for the smaller effect size and lack of performance differences in acquisition and retention is that participants received the same pre-recorded, group-specific instructions in practice. During skill acquisition outside of a lab, coaches likely alter the wording of their instructions in a more dynamic way to meet an athlete's needs. Thus, future research could test this idea by having slight variations in the instructions each time they are provided to the learners during acquisition.

In OPTIMAL theory, autonomy-support is also predicted to facilitate performance by enhancing expectancies. Although participants in the autonomy-supportive language group had higher perceived competence scores (see Fig. 4.3B) at the end of the acquisition phase than those in the controlling language group, this was not associated with enhanced performance. Moreover, this group difference regarding perceptions of competence was no longer present before participants completed the delayed retention test. This pattern of results is similar to those found for self-efficacy by Hooyman et al. (2014). Intrinsic motivation is also fundamental to OPTIMAL theory; however, we did not find higher intrinsic motivation in the autonomy-supportive language group compared to the controlling language group (see Fig. 4.3C). Surprisingly, intrinsic motivation actually decreased from the end of acquisition to before retention in the autonomy-supportive group; without compromising retention performance relative to the controlling language group. Contrary to OPTIMAL theory where such psychological variables are argued to have a direct and lasting impact on motor performance and learning, our data suggests these effects may only appear when participants are experiencing the experimental manipulation (i.e., during practice) and are more transient in nature. That is, we did not see a significant relationship between self-reported scores for any of the psychological variables before retention with performance in retention (see Figs. 4.3D-F). Although transient or more indirect effects of motivational factors are consistent with seminal information-processing perspectives on motor learning (e.g., Salmoni et al., 1984), we acknowledge the exploratory nature of these correlational analyses in the present experiment. Overall, our findings regarding perceived competence, intrinsic motivation, and perceived autonomy are difficult to reconcile with OPTIMAL theory and suggest investigating *indirect* effects of motivational factors on skill acquisition may be a fruitful line of inquiry for future motor learning research.

A potential limitation of the current experiment is the lack of a neutral language group. We did not include a neutral language group for several reasons. First, the inclusion of a third group would have substantially increased the required sample size (from $N = 156$ to $N = 246$) to investigate our smallest effect size of interest with adequate power. Second, as such an increase in sample size would have exceeded our resource constraints (Daniël Lakens, 2022; Lenth, 2001), we instead decided to conduct a large N experiment that focused on the ends of the instructional language continuum, or in other words, the biggest potential difference. Third, in both Reeve & Tseng (2011) and Hooyman et al. (2014), the key differences were between the autonomy-supportive language group and the controlling language group. Lastly, autonomy-supportive and controlling language are often used in real-world settings such as physiotherapy (e.g., Murray et al., 2015) and coaching (e.g., Bartholomew, Ntoumanis, & Thøgersen-Ntoumani, 2009; Carroll & Allen, 2021), with little inclusion of neutral language. For these reasons, we contend that a neutral language group would not have added enough value to offset the costs associated with the dramatic increase in sample size.

In conclusion, we did not find a motor performance and learning advantage of autonomy-supportive instructional language compared to controlling instructional language. This finding is inconsistent with past motor learning research (Hooyman et al., 2014) and the OPTIMAL theory of motor learning (Wulf & Lewthwaite, 2016). Despite no motor performance or learning differences, we did find higher perceptions of autonomy in the participants that received autonomy-supportive instructional language compared to those that received controlling instructional language. While the primary goal of most motor learning interventions is a relatively permanent change in the capability for skill, it is worth noting that in some situations autonomy-support in and of itself might be a desired affective outcome (e.g., Ste-Marie, Lelievre, et al., 2020). In such situations, autonomy-supportive instructional language could be paired with another form of practice that has more reliable effects on motor learning. Our perceived competence and intrinsic motivation data were also not consistent with the direct and lasting effects proposed within OPTIMAL theory. While we do not discount the importance of motivation for motor skill acquisition, based on the current data we suggest that the motivational factors within OPTIMAL may instead have a more *indirect* influence on motor skill learning.

Chapter 5

General discussion

The goal of this dissertation was to critically test predictions made by OPTIMAL theory (Wulf & Lewthwaite, 2016) pertaining to the role of autonomy-support in motor learning. Within OPTIMAL theory, conditions of practice which support a learner's basic psychological need for autonomy, enhance expectancies for future success, and promote an external focus of attention lead to superior motor control and learning. The autonomy branch of the motivation pillar encompasses three ways to create a supportive (or not) practice environment: control over practice conditions (i.e., self-controlled learning), incidental choices, and instructional language. Over four experiments I tested the predictions that supporting a learner's basic psychological need for autonomy would improve motor learning and performance. Autonomy-support was manipulated in two ways: 1) the provision of choice over an aspect of their practice environment (i.e., self-controlled learning; Chapters 2 and 3) or 2) instructional language (Chapter 4). This led to three specific purposes:

1. Investigate the mechanisms underlying the self-controlled learning advantage by manipulating feedback characteristics (Chapter 2)
2. Investigate the effects of making participants in a yoked group explicitly aware of being denied the same choice opportunity as other participants (Chapter 3)
3. Investigate the effects of instructional language on motor learning (Chapter 4)

5.1 Summary of findings

In Chapter 2, two experiments were included to critically test between information-processing and motivational accounts of a self-controlled learning advantage by manipulating feedback characteristics in self-controlled and yoked groups.

Experiment 1 included a 2 Choice (self-controlled, yoked) x 2 Feedback characteristics (error, graded) factorial design and Experiment 2 included a self-controlled and yoked group who received binary feedback. Both experiments failed to replicate a self-controlled learning advantage. Further, perceptions of autonomy, competence, intrinsic motivation, and error estimation abilities were not different between self-controlled and yoked groups. The lack of a self-controlled learning advantage made commentary on underlying mechanisms moot.

Chapter 3 was used to address a methodological limitation in the experiments in Chapter 2, as well as the self-controlled learning literature as a whole. Here, a novel yoked group was included where participants were made explicitly aware that they were being denied choice opportunities afforded to others in the experiment. This resulted in 3 groups: self-controlled, traditional yoked, and explicit yoked. While participants in the self-controlled group reported higher perceptions of autonomy than those in both yoked groups, there were no differences in motor learning as captured by a delayed retention test.

In Chapter 4 I examined the instructional language leg of the autonomy branch. Here, participants (n = 78/group) received either autonomy-supportive or controlling instructional language during the acquisition phase. Similar to Chapter 3, perceptions of autonomy were successfully manipulated, but increased perceptions of autonomy in the autonomy-support group did not translate to improved learning at the delayed retention test. Taken together, the findings from the four experiments included in this dissertation provide limited support for predictions made by the autonomy branch of OPTIMAL theory.

5.2 Autonomy-supportive practice conditions

5.2.1 Control over practice conditions

The supposed self-controlled learning advantage has been touted as a robust motor learning phenomenon wherein participants who are provided choice over an aspect of their practice environment have enhanced motor learning compared to their no-choice counterparts (Sanli et al., 2013; Ste-Marie, Carter, et al., 2020; Wulf, 2007). From a motivational perspective, self-controlled practice conditions are beneficial for performance and learning as they support a learner's basic psychological need for autonomy (Ryan & Deci, 2000a), thereby increasing intrinsic motivation, motor performance, and motor learning (Wulf & Lewthwaite, 2016). The information-processing perspective suggests that learners engage in performance-contingent strategies to evaluate their performance and make decisions that will reduce the uncertainty about their movement outcome (Carter et al., 2014; Carter & Ste-Marie, 2017b, 2017a; Chiviacowsky & Wulf, 2005; Couvillion et al., 2020; Woodard & Fairbrother, 2020). Since Chiviavowsky and Wulf (2005) first attempted to determine whether the mechanism underlying the self-controlled learning advantage was more motivational or informational in nature, a fruitful line of research has emerged to further disentangle the two perspectives. At the outset of this dissertation work, the state of the literature was generally favourable in support of a self-controlled learning advantage, but had no consensus on the proposed underlying explanations. In the past five years, however, there has been a shift in the evidence to which the experiments from Chapters 2 and 3 have contributed.

While it is likely that the overwhelming support for a self-controlled learning advantage is due to publication bias (McKay, Yantha, et al., 2022), upon reviewing the literature it is clear that it may also be due to flexibility in reporting (i.e., cherry picking) positive findings within an experiment (Simmons et al., 2011). For example, authors have claimed support for a self-controlled learning advantage when an effect of choice was only found in a transfer test but not a retention test (Chiviacowsky & Wulf, 2002, 2005; Fairbrother et al., 2012; Grand et al., 2015). While some have suggested that transfer is a more sensitive measure of learning than retention (Chiviacowsky & Wulf, 2002, 2005), others have disagreed and concluded that retention and transfer offer different yet complementary information about learning (Carter et al., 2014). Another example of selectively highlighting positive findings for a self-controlled learning advantage is when researchers include several dependent measures to answer the same research question (Lohse et al., 2016) but only comment on those which support their predictions (Carter & Patterson, 2012; Chiviacowsky & Wulf, 2002, 2005; Grand et al., 2015; Hansen, Pfeiffer, & Patterson, 2011). By placing more emphasis on findings that support predictions, researchers may be overinflating the level of support for the phenomena they are studying (Button et al., 2013; Twomey et al., 2021). The self-controlled learning literature appears to suffer from highlighting findings that align with the prominent view of a learning advantage, which has likely contributed to the perhaps untrue consensus that this is a robust effect.

Another possible explanation for the lack of replication of a self-controlled learning advantage in this dissertation and more recent experiments is the inclusion of larger sample sizes. The motor learning literature in general has suffered from small, underpowered designs (Lohse et al., 2016), and experiments examining self-controlled learning are not exempt. The median *total* sample size in self-controlled learning studies is 36 (McKay, Yantha, et al., 2022), which is smaller than the sample size *per group* in Chapters 2 (n = 38g/group) and 3 (n = 50/group). Alarmingly, these small experiments have led the self-controlled learning literature to achieve an estimated only 5% statistical power (McKay, Yantha, et al., 2022). Under-powered studies are problematic as they are unlikely to detect an effect should it exist, when they do find a positive finding it is unlikely that it reflects a true effect (i.e., is likely a type I error) and that it overestimates the true size of the effect (Button et al., 2013). Small samples, along with publication bias, have likely littered the self-controlled learning literature with false positives (Simmons et al., 2011) which has led to a replication crisis of this effect. The large samples included in this dissertation may have allowed for the limitations associated with small sample sizes to be avoided, and thus resulted in a lack of replication. This is supported by other large (n ranges from 28 to 100/group), often pre-registered experiments also failing to replicate a self-controlled learning advantage (Bacelar, Parma, Cabral, et al., 2022; Grand et al., 2017; Leiker et al., 2019; McKay & Ste-Marie, 2020, 2022; Ziv, Lidor, & Levin, 2022).

The most likely explanation for the lack of replication of the self-controlled learning advantage is that the size of the effect is negligible, if it exists at all (McKay, Yantha, et al., 2022). In this dissertation, to plan effective experiments I conducted a-priori power calculations for all experiments. An early estimate for the size of the effect of self-controlled over yoked practice was Hedge's g of 0.63 (McKay et al., 2014). This estimate, converted to Cohen's f = .32, was used as a guide in Chapter 2, however due to the uncertainty of how self-controlled feedback would interact with feedback characteristics I opted for a more conservative f = .23 as the effect size estimate. While planning the experiment in Chapter 3, an updated estimate of the size of the effect was g = .52 (Z. Yantha, personal communication, October 2019). Again, given the uncertainty of the effect size including the novel explicit yoked group I used a more conservative g = .40 for my sample size calculation. Despite these efforts, the experiments were grossly underpowered to detect the current estimate of the effect size. The most recent estimate of the self-controlled learning advantage is g = .44 [0.31, .56] (uncorrected), however when various corrections are applied to the meta-analytic model the most conservative estimate is indistinguishable from 0 (g = .05 [-.18, .29]). To reliably detect a less conservative estimate of g = 0.11 with a simple t test, 1300 participants *per group* would be required, which is 26 times larger than the sample included in Chapter 3. Therefore, it is less surprising that an advantage for participants who had choice over an aspect of their practice environment was not found.

Taken together, the evidence to support the supposed self-controlled learning advantage is weak at best. Although there have been numerous studies which have found learning benefits for participants in self-controlled over yoked groups; selective highlighting of results which support predictions, small sample sizes, and publication bias likely inflate the perceptions of robustness of the effect. The results from Chapters 2 and 3 of this dissertation, along with other experiments which have included relatively large sample sizes and a recent meta-analysis, question the reliability of the purported self-controlled learning advantage. Given the lack of replication of a self-controlled learning advantage in both Chapters, commentary on the underlying mechanisms is not relevant.

5.2.2 Instructional language

Instructional language has been shown to be an effective way to manipulate perceptions of autonomy across a variety of settings (see Mossman et al., 2022; Okada, 2021; Reeve, 2009; Ryan & Deci, 2020; Su & Reeve, 2011 for reviews and meta-analyses). Despite being a component of the autonomy leg of OPTIMAL theory (Wulf & Lewthwaite, 2016), however, it has received minimal attention in the motor learning literature. The results from Chapter 4 of this dissertation add to and provide mixed support for the available literature on the role of instructional language in a motor performance and learning setting. Similar to previous literature (Arsham et al., 2021; Hooyman et al., 2014), instructional language was effective in manipulating perceptions of autonomy in supportive versus controlling groups. Results from the experiment in Chapter 4, however, differ from previous literature such that increased perceptions of autonomy did not coincide with increased motor performance (Arsham et al., 2021) or learning (Hooyman et al., 2014). Similar to control over practice conditions above, these conflicting findings could be due to false positives in previous literature due to small sample sizes (e.g., n = 16/group in both Arsham et al., 2021; Hooyman et al., 2014). Alternatively, it may be that not only is the effect size associated with choice over practice conditions extremely small (McKay, Yantha, et al., 2022), but rather that the effect size associated with the relationship between autonomy-support and learning is indistinguishable from zero, regardless of how autonomy is manipulated.

5.2.3 Mixed findings regarding autonomy-supportive practice manipulations

Similar to previous literature (Barros et al., 2019; Carter & Ste-Marie, 2017b; McKay & Ste-Marie, 2022; Ste-Marie et al., 2016, 2013), the findings of perceptions of autonomy were mixed between the chapters of this dissertation. In Chapter 2, there were no differences in perceptions of autonomy between self-controlled and yoked groups across both experiments. The autonomy-support manipulations, however, were successful in Chapters 3 and 4 as participants in the autonomy-support groups (Chapter 3: self-controlled, Chapter 4: autonomy-supportive instructional language) reported higher perceptions of autonomy than those in the autonomy-thwarted groups (Chapter 3: traditional and explicit yoked, Chapter 4: controlling instructional language).

A potential explanation for these conflicting findings is that the size of the psychological autonomy-support effect may also be quite small and require larger samples per group to detect. For example, the experiment in Chapter 4 had more than twice the sample size per group than that in Chapter 2 (78 compared to 38 per group, respectively). The effect size estimates for the significant findings were smaller than those previously reported for similar autonomy-support manipulations. Specifically, in Chapter 3 the estimated size of the main effect of Choice was $g = .20$, which is smaller than $g = .57$ previously reported by McKay and Ste-Marie (McKay & Ste-Marie, 2022). Similarly, the effect size estimate from Chapter 4 was $d = .34$ compared to $d = .63$ reported by Su & Reeve (2011). The sample sizes included in this dissertation were larger than those in McKay and Ste-Marie (2022) and Su and Reeve (2011) and therefore had more power and were likely able to detect more precise estimates of the size of the effect. Interestingly, Ziv and colleagues (2022) included 49 participants per group in a pre-registered self-controlled learning experiment, and reported a much larger effect size of $d = 1.0$ for perceptions of choice. Therefore, this interpretation is forwarded with caution.

Within the context of the measures included in the experiments of this dissertation, perceptions of autonomy were captured on a 7-point Likert scale. These small effect sizes correspond to less than half of a point difference between autonomy-support and control groups and likely hold limited value if applied in a more ecologically valid setting. Further, these increases in perceptions of autonomy did not translate into improvements in physical performance in either acquisition or retention in either experiment. These findings highlight that autonomy-support is likely a small effect, and is not likely to have direct causal influence on motor performance or learning as predicted by OPTIMAL

Table 5.1. Summary of findings related to key predictions of OPTIMAL theory.

Chapter	Autonomy	Competence	Motivation	Acquisition	Retention	Transfer
2, Expt 1	No	No	No	No	No	No
2, Expt 2	No	No	No	No	No	No
3	Yes	No	No	No	No	N/A
4	Yes	Yes	Yes	No	No	N/A

theory.

5.3 Current state of OPTIMAL theory

The primary objective of this dissertation was to critically test predictions made by OPTIMAL theory (Wulf & Lewthwaite, 2016) pertaining to the autonomy branch of the motivation pillar. Findings provide mixed support at best, although several key tenets were not supported (see Table 5.1). There was some evidence that autonomy-supportive practice conditions can manipulate some psychological constructs in line with OPTIMAL theory. Specifically, perceptions of autonomy were higher in the autonomy-support versus autonomy-controlling groups in Chapters 3 and 4 and perceptions of competence (i.e., enhanced expectancies) were higher in the autonomy-support group in Chapter 4. It is important to note, however, that perceptions of autonomy were not different between groups in either experiment in Chapter 2, nor was intrinsic motivation different between autonomy-support and controlling groups in any experiment in this dissertation. Critically, the lack of behavioural differences during acquisition and retention do not support OPTIMAL theory.

Throughout the experimental chapters of this dissertation, the discussion around support of OPTIMAL theory has been magnanimous. Taken together, however, the four experiments included here do not provide evidence that OPTIMAL theory is a viable mechanism to explain human motor performance and learning. A key component and contribution of OPTIMAL theory is the virtuous cycle, wherein "conditions that enhance expectancies, provide autonomy support, and promote an external focus [of attention] result in a virtuous cycle of enhanced motor [performance and] learning" (Wulf & Lewthwaite, 2016, p. 1405). That is, OPTIMAL theory predicts a direct *causal* influence of autonomy-support, enhanced expectancies, and intrinsic motivation on motor performance and learning. In this vein, increased perceptions of autonomy (Chapters 3 and 4) and competence (i.e., enhanced expectancies, Chapter 4) should have enhanced motor performance, to further increase these psychological constructs. This causal relationship, however, was not observed as motor performance was not enhanced in any experiment.

A limitation of experiments which have claimed support for OPTIMAL theory is the lack of analyses which test the causal relationships between key psychological tenets and motor performance and/or learning (importantly, these tests are missing from the majority of motor learning experiments, Carter, Lohse, & Miller, 2022). It is possible, however, that predictions made by OPTIMAL theory could apply on an individual participant level. That is participants who report higher perceptions of autonomy, competence, or intrinsic motivation may achieve superior performance and/or learning. For example, in the largest self-controlled learning experiment to date, a mixed-effects model revealed that intrinsic motivation predicted post-test performance (Bacelar, Parma, Cabral, et al., 2022). This effect of intrinsic motivation predicting motor learning on an individual participant level, however, was not replicated in another experiment examining the impacts of enhanced expectancies on motor learning (Parma et al., 2023). In its current state, OPTIMAL theory makes direct causal predictions on the group level between psychological constructs and motor performance and learning. There is limited evidence, either from psychological constructs being manipulated in conjunction with physical performance benefits or causal inference analyses, to support these predictions.

This is not to say, however, that motivation does not impact motor learning. Rather, the influence may follow an indirect pathway. That is, if a learner has increased perceptions of autonomy, competence, or intrinsic motivation, they may choose to practice a task more often or for longer periods of time and reap performance and learning benefits. For example, participants who got choice over the order of four exercises performed opted to engage in more sets and repetitions of each exercise compared to a no-choice control group (Wulf, Freitas, et al., 2014). Notably missing from this experiment, though, was any measure of motivation or autonomy. Interestingly, motivation was included in early information-processing theories of motor learning (Adams, 1971; Salmoni et al., 1984). Here, KR feedback was proposed to have a motivational influence on practice wherein participants become more interested in the task, work harder, and continue to engage in the task for longer after KR is removed (Salmoni et al., 1984). Current motor learning experiments are not designed to test such predictions as the number of practice trials are often kept constant between groups and intrinsic motivation is captured via self-report questionnaires. To assess this indirect influence of motivation on motor performance and learning, experiments would need to include an extinction period to assess persistence at the task (Deci, 1971) and capture retention after the extinction period.

Since a critical examination of the motor learning literature highlighted weaknesses of the motor learning literature (Lohse et al., 2016), there has been a shift towards larger sample sizes, more thoughtfully designed a-priori analysis plans, and meta-scientific approaches. This has allowed for more thorough tests of existing motor learning theories including key tenets of OPTIMAL theory. The four experiments in this dissertation, along with other recent experiments which included large samples (Bacelar, Parma, Cabral, et al., 2022; McKay & Ste-Marie, 2020, 2022; Yantha et al., 2022), provide limited support for the autonomy-support branch of OPTIMAL theory. That is, while some experiments have found an effect on perceptions of autonomy between supportive and controlling groups (Chapters 3 and 4 here, McKay & Ste-Marie, 2022), there has been limited recent evidence that autonomy-supportive practice environments enhance motor performance and learning (all chapters here, Bacelar, Parma, Cabral, et al., 2022; McKay & Ste-Marie, 2020, 2022; Yantha et al., 2022). Further, a recent meta-analysis on the effects of choice (including both task-relevant and task-irrelevant choices) on motor learning did not find evidence that this effect was distinguishable from zero (McKay, Yantha, et al., 2022). While outside the scope of this dissertation but relevant to the discussion surrounding OPTIMAL theory, similar outcomes were found in a meta-analysis on external focus of attention (McKay, Corson, et al., 2022). A meta-analysis of the effects of enhanced expectancies, however, did find a positive influence on motor learning, however this conclusion was forwarded with caution due to methodological limitations in the included experiments (Bacelar, Parma, Murrah, & Miller, 2022). Taken together, OPTIMAL theory filled a key void in motor learning theories by considering psychological and motivational needs of humans. Key tenets and predictions included in OPTIMAL theory, however, have been continuously challenged and unsupported, suggesting that it may not be a useful framework from which to design motor learning research.

5.4 Limitations and future directions

All experiments have limitations, and this dissertation is not exempt. A limitation of this dissertation is that the experiments in Chapter 1 were not pre-registered. While a thorough power analysis and analysis plan were completed a-priori, they were not formally submitted as a pre-registration. Further, no a-priori smallest effect size of interest was pre-registered for Chapters 2 or 3. This resulted in the need for exploratory, rather than confirmatory, equivalence tests of the non-significant differences between autonomy-support and autonomy-controlling groups. These limitations, however, were addressed later in this dissertation by including a pre-registration of my primary analysis in Chapter 3, and a thorough pre-registration of all potential analyses for Chapter 4.

Another potential limitation in this dissertation is that participants in the self-controlled group in Chapter 3 chose to watch a model on a relatively low number of trials. That is, they may not have benefited from cognitive processes associated with observational learning. It is important to note, however, that the mean viewing frequency (16.7%) is slightly higher than those reported in previous experiments (e.g., ~6% and ~10% reported by Wulf, Raupach, & Pfeiffer (2005) and Wrisberg and Pein (2002), respectively) where learning benefits were found with low viewing frequencies. Further, participants in the self-controlled group received a choice opportunity after every trial, which is what is predicted to lead to enhanced performance and learning in OPTIMAL theory. Taken together, the lack of learning differences between the groups in Chapter 3 are more likely due the very small estimated effect size of self-controlled learning rather than the low frequency of observation selected by those in the self-controlled group.

Addressing these limitations, alongside the approaches I took to this dissertation, highlight some future directions to move the motor learning field forward. These recommendations echo those forwarded by Lohse and colleagues (2016). Motor learning researchers are urged to adopt open science practices across all stages of the experimental process. This includes conducting and reporting all relevant information included in a-priori power analyses (McKay, Corson, et al., 2023; McKay, Bacelar, & Carter, 2023); pre-registering power analyses, experimental designs, and analysis plans; and openly sharing data and code (Allen & Mehler, 2019; McKiernan et al., 2016; Munafò et al., 2017). Another future direction is to increase statistical power of experimental designs. While the most obvious way to do so is to increase sample size, motor learning researchers are often working with limited resources which makes this a challenging endeavor. Instead, statistical power can be increased by collecting data across multiple sites and/or by including more powerful analyses such as one-tailed tests, analysis of covariance, or the shift function (Beck, 1994; McClelland, 2000; Rousselet et al., 2017). Finally, motor learning researchers are encouraged to include statistical models to test causal relationships between mechanistic and primary outcome variables (Carter et al., 2022), for example between autonomy-support and motor learning, to build understanding and future theories that explain the breadth of human motor behaviour.

5.5 Contributions to the literature and concluding remarks

The key theoretical contribution of this dissertation to the motor learning literature is a critical test of the autonomy branch of OPTIMAL theory. Through four experiments I found mixed support for the efficacy of so-called autonomy-supportive practice conditions on perceptions of autonomy and competence, and no evidence that such practice conditions increase intrinsic motivation. More importantly, autonomy-supportive practice conditions, including self-controlled learning and autonomy-supportive instructional language, were not found to influence motor performance or learning. In sum, there was limited evidence that the autonomy-support branch of OPTIMAL theory is a viable explanation for motor learning phenomena. Within the context of other well-powered experiments and meta-analyses, this dissertation adds to the growing body of evidence which questions OPTIMAL theory as an appropriate motor learning theory.

Taken together, the results for this dissertation provide limited support for OPTIMAL theory and question its usefulness in explaining motor learning phenomena. This highlights the need for experiments with large samples, open-science practices, and motor learning theories which can better capture current motor learning phenomena and create testable predictions to continue to move the field forward.

Apendix A

Questionnaire used in Chapter 2

Please read each of the following items carefully and then respond to each statement by indicating how true it is for you. Use the following scale to respond:

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

1. I enjoyed doing this aiming task very much.
2. I think I am pretty good at this aiming task.
3. I feel free to make choices and express my opinions in my practice session.
4. I think I did pretty well at this aiming task compared to others.
5. This aiming task was fun to do.
6. After working at this aiming task for a while, I felt pretty competent.
7. I thought this aiming task was a boring activity.
8. I feel like my choices and opinions were taken into consideration in my practice session.
9. I am satisfied with my performance on this aiming task.
10. This aiming task did not hold my attention at all.
11. I was pretty skilled at this aiming task.
12. There is not much opportunity for me to exercise choices in my practice session.
13. I would describe this aiming task as very interesting.

14. The aiming task is an activity that I couldn't do very well.
15. I thought this aiming task was quite enjoyable.
16. I feel controlled and pressured in my practice session.
17. While I was doing this aiming task, I was thinking about how much I enjoyed it.

Apendix B

Questionnaire used in Chapter 3

Please read each of the following items carefully and then respond to each statement by indicating how true it is for you. Use the following scale to respond:

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

1. I enjoyed doing this cup stacking task very much.
2. I think I am pretty good at this cup stacking task.
3. I feel free to make choices and express my opinions in my practice session.
4. I think I did pretty well at this cup stacking task compared to others.
5. This cup stacking task was fun to do.
6. After working at this cup stacking task for a while, I felt pretty competent.
7. I thought this cup stacking task was a boring activity.
8. I feel like my choices and opinions were taken into consideration in my practice session.
9. I am satisfied with my performance on this cup stacking task.
10. This cup stacking task did not hold my attention at all.
11. I was pretty skilled at this cup stacking task.
12. There is not much opportunity for me to exercise choices in my practice session.
13. I would describe this cup stacking task as very interesting.

14. The cup stacking task is an activity that I couldn't do very well.
15. I thought this cup stacking task was quite enjoyable.
16. I feel controlled and pressured in my practice session.
17. While I was doing this cup stacking task, I was thinking about how much I enjoyed it.

Apendix C

Questionnaire used in Chapter 4

Please read each of the following items carefully and then respond to each statement by indicating how true it is for you. Use the following scale to respond:

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

1. In this study, I get opportunities to make decisions.
2. In this study, I feel I am being forced to do things that I don't want to do.
3. I can overcome challenges in this cup stacking task.
4. I would describe this cup stacking task as very interesting.
5. In this study, I feel I am pursuing goals that are my own.
6. I enjoyed doing this cup stacking task very much.
7. In this study, I have a say in how things are done.
8. I thought this cup stacking task was a boring activity.
9. I am skilled at this cup stacking task.
10. In this study, I really have a sense of wanting to be here.
11. This cup stacking task did not hold my attention at all.
12. I feel I participate in this study willingly.
13. I thought this cup stacking task was quite enjoyable.
14. I get opportunities to feel that I am good at this cup stacking task.

15. In this study, I feel I am doing what I want to be doing.
16. This cup stacking task was fun to do.
17. In this study, I get opportunities to make choices.
18. I have the ability to perform well in this cup stacking task.
19. In this study, I can take part in the decision-making process.
20. I feel I am good at this cup stacking task.
21. I choose to participate in this study according to my own free will.
22. While I was doing this cup stacking task, I was thinking about how much I enjoyed it.

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